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THE
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THE
EDINBURGH NEW
PHILOSOPHICAL JOURNAL,

EXHIBITING A VIEW OF THE
PROGRESSIVE DISCOVERIES AND IMPROVEMENTS

IN THE
SCIENCES AND THE ARTS.

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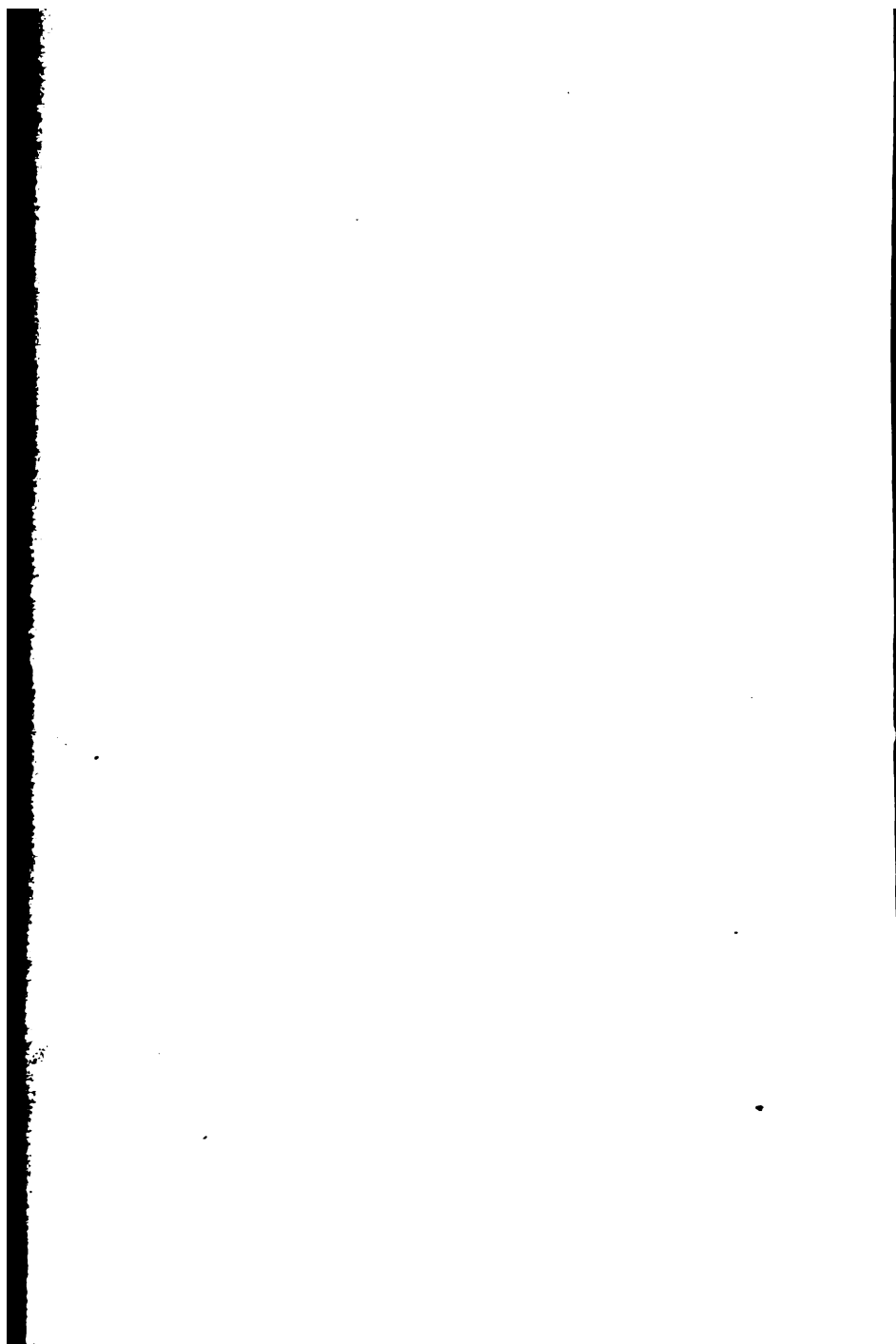
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THE
EDINBURGH NEW
PHILOSOPHICAL JOURNAL.

On the Phenomena of the Scratched Rock Surfaces. By THEODORE KJERULF; with a Notice of the Post-Pliocene Fossils of Southern Norway. By Dr SARS, Professor of Natural History, Christiania.*

[The following paper on the Boulder-clay of Norway, by the well-known Norwegian geologist, Kjerulf, is presented to our readers as likely to be interesting and instructive to the many students of this difficult subject, who may not have access to it in the original Norwegian.

The translator thinks it proper to mention that he has not adhered slavishly to the original text, having thrown two papers of Mr Kjerulf into one, and having omitted all that was of merely local interest. The second diagram here given is not strictly the same as any one of the many supplied by Mr Kjerulf, but is an attempt to reproduce in small compass all the essential features to be gathered from the whole of these. The most important alteration which it presents is the introduction of the small heap of boulder clay, underlying the marl, in a somewhat more distinct shape than it bears in the original diagram. This change is, however, justified, not only in part, by the particular diagram referred to, but still more by many statements in the text, more especially in some of those more local descriptions which have been omitted. In all cases the translator has endeavoured to

* Translated from the Danish, for the Edinburgh New Philosophical Journal, by the Rev. R. Boog Watson, F.R.S.E.

reproduce, if not the very words, for which space was often wanting, yet at least always, and strictly, the views expressed by Mr Kjerulf; and wherever he has felt that the responsibility of a statement or an inference rested with himself, he has put it in the form of a note.—*Translator.*]

It is well known that the surface of our mountains is in many places scoured, polished, striated, and bared, and that at times to a height of 4000 or even 5000 feet above the sea. All over the country such surfaces may be seen; for as the country is covered with mountains, so are the mountains with scratches. These had attracted the attention of scientific miners in Sweden, but Sefström was the first to study them carefully. He remarked that the striæ followed a determinate direction, and bore a relation to the course in which the boulders of the country had travelled. Thus the furrows follow a southerly direction; and in like manner the porphyry blocks of Elfdal have been carried south to Landskab, where they now lie among totally different formations. Hence he inferred that the direction pervading the entire phenomenon was one from north to south. He believed that the agent in all this was a flood—a vast mass of boulders, stones, debris, and sand swept away by water, which had poured across the mountains with incredible rapidity and force, sufficient to make the large stones slide over the surface of the mountains and scratch these, as a grain of sand will do when made to grate along under the finger over a marble slab; while the smaller masses were ground against each other, and left piled up in the long backed Åsar of Sweden, the Raer of our Smaalehn district.

Sefström's paper is found in the Kongl. Vetenskaps Akad. Handl., 1836.

Hoffmann, Pusch, and Böhlingk, suggested a different direction for the phenomena. They showed that the erratic blocks are distributed over the flat of Northern Europe in a great circle, of which Scandinavia and Finland form the centre—that in Northern Russia, the blocks are Finnish; in Poland, are mixed with Swedish; in the Netherlands and North Germany, are Norwegian and Swedish—that beyond these limits the blocks have not travelled—and that

in Finland, at the White Sea, and on the northern glacial ocean, the striation radiates outwards.

If, then, there was a stream, it was not moving in any *one* direction alone, but must have had, in addition, an outward lateral motion along the axis of its general course; and the question arose whether the motion was from one centre or from several. The utmost attention was now directed to the striations in various localities, and hundreds of observations were collected; blocks were carefully tracked back to their original site, the weather or exposed, and the lee side of rocks were attentively noted, and the materials accumulated became enormous. These were tabulated on maps by Hörbye and Rördam in 1857, &c.

As to the direction of the scratches, there can be now no doubt; but till this was ascertained, the question whence and whether the grinding down proceeded could only be determined from the somewhat doubtful indications of the "weather and lee sides" and the boulders. Now, however, if we examine the maps, we shall observe spots from which the arrow markings radiate out in different directions. Such spots must have been the centres of motion, and they lie, it will be found, in the higher districts of our mountains; or where, at a lower level, they occur in the form of cross scratches, they may indicate the successive application of friction in slightly different directions.

Thus far, then, we have got; the phenomenon itself is no fiction, however erroneous may be the explanation of it by a flood. Innumerable observations with the compass have shown that the scratches and striations are there, that there must further have been various centres whence the phenomena originated, and finally, that in some places at least, the force has operated again and again.

When now we consider that glacier ice, with the debris which it carries along with it, produces striations on the surface of the rocks over which it is ever, though slowly, advancing; when we consider, further, that a glacier moves boulders either involved in the debris of the end and side moraines, or in the form of huge, scattered, sharp-angled blocks, which in one way or other have reached the surface of the glacier, and now share in its movements, we may

infer that we have obtained the key to all the phenomena of friction which we have to explain. The comparatively small scale of the glacier phenomena with which we are acquainted, leaves, indeed, many difficulties in our way ; but here, fortunately, we meet with Mr Rink's valuable observations, prosecuted during the many years of his stay on the west coast of Greenland, of glacier action in that country. From his descriptions, it is obvious that here, as in our own land, we have an enormous stretch of country ; that this country is completely covered with ice, which, on the west coast, is ever slowly pressing outwards to the sea, where it "calves;" that immense loads of these ice "calves" are every year floated away by currents in the same constant direction ; and finally, that this ice-cake attains a thickness of no less than 1000 feet.

Here, then, all the conditions of our problem are provided for. Let us figure to ourselves a universal ice covering, a complete glaciation instead of mere individual glaciers, and the phenomenon of friction takes its place harmoniously with all the other phenomena of the glacial epoch. In the existing state of Greenland is the very analogy we needed to justify our supposition—a gigantic cake of ice slowly moving outwards, and with the might of its tremendous pressure, stripping, polishing, and striating the rocks over which it glides, just like a common glacier, only in so much greater a proportion as the ice-cake of Greenland surpasses any glacier of Switzerland.

I shall endeavour to show that, in explaining the phenomena of friction, we know of no better agent than the ice-cake.

In respect of the direction of the scratches inferences have been drawn, (1.) From the boulders or erratic blocks. (2.) From the so-called "lee and weather sides." All the arrows on the striation maps have been laid down on the supposition that the blocks were moved by the same stream which was striating the rocks, and that all this tallies with the lee and weather sides which present themselves to our notice. Now, here it is important to observe,

1. That the boulders in the lower districts do *not* indicate the direction either whence or whither of the friction.

The boulders which we usually call erratic—great, generally sharp-cornered blocks which easily catch the eye—have doubtless been moved, but not always by the same “stream” which scratched. There is here a confounding of quite different things. These blocks in the lower districts lie on the top of the boulder-clay banks. How could it, then, be they that scratched the rocks. But it is precisely the erratic rocks which most catch the eye, lying as they do on the surface, large in size, sharp at their edges, and easily recognised. On the other hand who has ever broken his hammer on the true ‘scratchers,’—the stones great and small—crushed, rubbed, rounded, and broken—that lie at the bottom of the boulder-clay banks,—these heaps of debris which rest immediately on the rock?

Things are somewhat different in the higher districts. There one may, indeed, more safely draw inferences from the erratic blocks; but there, what with the vastness of the field and the fewness of the observers, we know too little of the minute lithology to entitle us to assert that any particular foreign block we may meet can only have proceeded from one special bed. If we could do this, we could indeed determine the direction of the moving power; but this we cannot do. Further, the blocks have been moved in various ways, and they cannot therefore be depended on for indicating how the arrows showing direction of motion should be laid down in the maps.

2. There has been too much of rash talking about lee and weather sides. In some cases such phenomena really exist; but such cases are rare; and generally the gentle slope on one side, and the steep break on the other, which have been described as instances of lee and weather sides, arise simply from the constitution and position of the rock itself. The mountains of Eggedal, for instance, all seem to show fine examples of a weather side towards the north, and a lee side towards the south, but this arises merely from the lie of the schist strata which dip to the north. Similarly, the porphyry hillocks in Asker and Barum seem to have weather sides to the north and lee sides to the south; but the cause of this is, that the porphyry beds dip north conformably with the underlying sandstone. When we are told, therefore, that the observer

has remarked fine examples of weather sides at a distance. we have good ground for distrusting his conclusions.

3. As to the marking of the arrows on the maps they often convey a false impression. The striations do indeed run towards valleys, and in a general way follow the lines of the valleys; but even the mountain plateaus have their valleys, which do not appear in maps of the small scale ordinarily used; were these marked, we should at once see that the friction has followed the valleys.

4. Finally, there is this to be laid down as a first principle, that the mere study of the phenomena and direction by compass of the scratches, and of the often quite independent boulders, is not sufficient to determine whether the agent which produced them was a stream or a glacier. Brogniart has called the striations the wheel-ruts of the phenomenon; but we have kept too much to the ruts, and forgotten the carriage and team. The loose masses, which owed their presence to this phenomenon, have passed unobserved. It is these we must study, if we are to understand the phenomena of friction; and it is as a name for these, the clay-sand and masses of debris connected with the phenomena of friction, that the term, "the Glacial Formation" has been introduced. And in fact there is more to be learned from the study of a single example of the glacial phenomena, in which all the various members of the formation are exhibited, than from thousands of scratches traced by the compass.

I proceed, therefore, to examine the glacial formation in detail as it presents itself in the district around Christiania.

The glacial formation includes masses of debris, blocks of stone, varieties of clay, and layers of sand. The term glacial is applied to it, partly because the oldest fossil shells found in it are of arctic species, and partly because the debris, the scratched rock surfaces and the boulders, connected as these are together, indicate that when they were produced the whole country was covered with ice. The entire formation may be divided into an earlier and a later group, of which the earlier alone is strictly glacial; but since the material of which the later group is composed was also originally glacial, and since the two groups are

very closely connected with one another, the name of the Glacial Formation may, for shortness sake, be applied to both.

The older and strictly glacial group consists of, 1. Glacial banks and debris heaps. 2. Glacial sand and marly clay, with the oldest glacial shell-beds. The later and post-glacial group consists of, 1. Shell-clay and shell-bearing sand—the later post-glacial shell-beds. 2. Brick-clay and inland clay. 3. Sandy clay and flood sand.

Following these is a third group of still later alluviums, river deltas, peat mosses, and river sand and gravel, all going on at the present day.

I. First, then, as to the older and strictly glacial group. (1.) The first part of this which we have to consider is the glacial banks. They consist of sand mixed with clay or with debris in general (*aur*), blocks, and the so-called rolled stones, which, however, are not rolled but scratched stones. Fragments of the most varied rocks are here mixed together. The position of these heaps of sand debris and blocks is always immediately on the furrowed rock surface itself. These banks are present everywhere, and everywhere are composed of the same heterogeneous materials. On the hills, at an elevation of 600 or 700 feet above the sea, this is the only formation present. At the lower levels it only appears in banks (*Åsar*, in Swedish; *Raer*, in Norwegian; *Kaims*, in Scotch) which rise amidst flat expanses of clay and sand, or sometimes appear in detached hillocks. In this form, which belongs to these banks in the lower districts, they rise to a level above the sea of from 6 to 700 feet. Such lower level glacial banks either stretch in long-backed ridges, which project markedly above the clay and sand flats, or lie completely buried in the interior of elevated sand plateaus, where their form and extension can in general only be inferred from the position of small lakes in their bendings, or from the curves which rivers and brooks, unable to cut through them, are compelled to follow. But the higher level banks, those, that is to say, which are found in the upper parts of the valleys and on the mountain slopes, rise to all elevations, even to a great height, sometimes fring-

ing the valleys as great heaps of debris, sometimes stretching right across, directly transverse to the course of the stream.

All these masses were originally true moraines.

It seldom happens that a good section of these banks can be seen; but as a rule they consist of debris (*aur*), with sand, clay, small and large stones, all irregularly heaped together without any trace of stratification. They rest directly on the surface of the rock, and are obviously the oldest of all the superficial deposits.

Besides such masses as have been now described, and which really deserve the name of banks, smaller heaps of debris and blocks exist, generally lying covered up and hidden, or protected by some projection of the rocks. Their origin and composition is exactly the same as those of the banks, from which they differ only in being less likely to attract notice. These are the glacial debris heaps.

(2.) There are also found in some places, resting immediately on the scratched surface of the rock, thin alternating layers of sand and clay. The sand is chiefly reddish and brown in colour, and sometimes preponderates decidedly over the clay. The sand and clay are occasionally twenty-five feet thick. Accumulations of big stones are at times found at the bottom of this deposit, in places where the form of the ground had afforded them shelter. Such stones are also scattered irregularly through the bed. This marly clay was formed from the destruction of Silurian limestones and schists. It was in all probability carried down by muddy rivers from the inland ice, and deposited as mud wherever the sea presented the necessary conditions of stillness. Freshly dug, this clay is always damp, often dripping with water. Its colour is bluish, seldom brown. Traces of a thin leafy or stratified structure may often be seen. Lime is pretty uniformly present to the extent of about two per cent. Exposed to the air this marly clay disintegrates more or less, but gets baked together again by rain. Its presence may generally be recognised by the thick clumps of the tussilago which it bears.

Balls or knots so round as almost to seem artificial are found in this marl, and have been considered as concretions

produced by the presence of a larger quantity of lime than usual; but they are much more probably fragments of the Silurian limestones, rubbed and worn to their present shape during the ice period, by the same agency as broke and scoured and polished and crushed and rubbed all the surface of our rocks, and so prepared the stores of debris that form our boulder-clay. Such rounded lumps or balls are the distinctive characteristic by which to recognise this marl.

The marl often contains also extremely thin and small scales of mica, which glitter in the sun,—a proof how slowly this marly clay was deposited, and how still the water in which it was allowed to sink. It was only when they came to rest in the bosom of the deep and quiet ocean that the mud-laden waters which poured from the inland ice could part with their burden; and in harmony with this we find, that the maximum height at which this marl is found is 450 feet above the present sea-level, a height which, as we shall see, corresponds exactly with that of the older marine shell-beds—[both facts thus pointing to the same conclusion,—the greater elevation of the sea in the later part of the glacial period, while our present boulder-clays were being formed in the interior of the land, and deposited along the shores and in the neighbouring shallows of the glacial ocean.—*Translator.*]

Where this marl is absent, the reason will be found to be the absence from the neighbourhood of such Silurian rocks as alone supply the materials from which it is formed.

As has been already mentioned, the marl sometimes interchanges endlessly with thinner or thicker layers of glacial sand, and wherever it does not rest directly on the scratched rock below, it is only separated by glacial sand or debris.

In general, no shells are found in the marly clay, the ice-cold fresh glacier water from which it was deposited being unfavourable to molluscan life, which would only develop itself in the salt water of the greater depths, or where at least the fresh water was largely mingled with the sea.—[These localities are still probably hidden from us below the present sea-level. We have, perhaps, instances of them in the Elie bank (p. 27), and the Dröbak bank (p. 25).—*Translator.*]



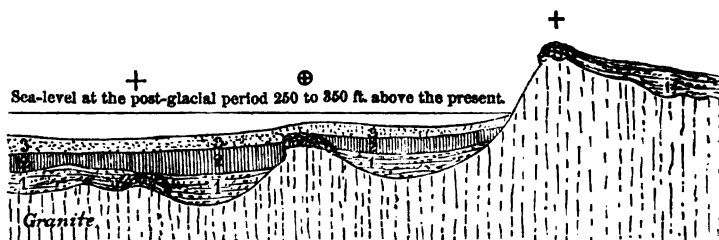
The few shells I have met with in the marl, or in the contemporary glacial sand, have been ascertained by Professor Sars to be either strictly arctic species, or marked by size and other peculiarities, with a distinctively arctic character.

Shell-beds, however, are found, which, though not actually in the marl, are yet closely connected with it, both in the strictly glacial character of their shells, and in the range above the sea-level to which they ascend. These beds are loose heaps of entire and broken shells, sometimes without the addition of any other substances, sometimes mixed with sand and sandy clay. They are, in fact, true littoral banks.

These shell-beds must not be confounded with the later shell-beds. The latter belong to the post-glacial period, when the sea stood at a lower elevation than before. The true glacial shell-beds, of which we are now speaking, lie at a level of from 400 to 470 feet, and belong to that earlier period, when the land was depressed not less than 500 or 600 feet below the present level of the sea. They are probably contemporaneous, or nearly so, with the glacial banks formerly described, and with the sands and marls which have been mentioned as resting directly on the furrowed surface of the underlying rock.

The accompanying diagram may make this more intelligible.

Old sea-level about 600 feet above the present.



Present sea-level.

Fig. 1.

At the bottom is granite; above this is (1.) Sand and marl, associated with the cotemporary shell beds marked +; (2.) Are the later clays; (3.) Sand. The sand and marl marked (1.), and the shell beds +, belong to the earlier glacial period, during the latter part of which the sea-level

stood at least 600 feet higher than at present. Any shells of this period, at the lower levels, will be found deeply buried under later deposits, while at the higher levels they lie exposed.

(2.) and (3.) The clays and sands, and their accompanying shell-bed marked $\oplus$ belong to the later period, when the sea-level had fallen 400 feet. The higher levels being by this elevation of the land protruded above the sea, the shell-beds already found on them were preserved from being buried under later deposits. None of the *later* shell-beds are therefore to be found at this elevation, but they present themselves at the lower levels, at or near the surface, among the later clays and sands.

II. We now come to the second or post-glacial group of deposits,—(1.) The first member of which is the later shell-clay to which we adverted above. Like the marl, it is derived from disintegrated Silurian strata, but more especially from the clay schists of that age. When freshly dug out, it is generally of a bluish-grey colour, and less wet than the marl. It has been deposited in salt water, and very often contains well-preserved shells of the same species as are still living on our coasts. In position it is superior to the marl and glacial sand; but it does not rise to so high a level above the sea as these, its maximum height being about 350 feet. In general, it does not ascend above 250 or 270 feet. This shell-clay is sometimes, though not always, mixed with lime. It is frequently worked for bricks.

(2.) The brick-clay in the *marine* beds is identical with the *inland* clays of the higher levels, from which it only differs in the greater thickness and purity of the banks in which it presents itself. It is generally blue or brown in colour, and these shades sometimes alternate in clods or stripes. No shells are found in the brick-clay.

In the course of the valleys this clay is found at all elevations deposited in old lake basins; it disappears, however, in proportion as we advance into the quartzose districts. Above the old sea-level it is very often mixed with sand, and contains layers, more or less thick, of fine sand. Since even at the lower levels it contains no shells, we must infer

that it has been deposited from streams of fresh water. It overlies the shell-clay.

(3.) Sandy clay and stratified flood sand, which alternate with another. The clay is generally light-grey in colour, and more or less mixed with fine sand, in consequence of which it is less plastic, and when dry does not stick to the tongue. The transition from sandy clay to clayey sand is sometimes complete. The sand is brown or grey, and the beds vary much in thickness. Here and there large gatherings of boulder stones may be found in it, especially towards the top.

The source whence this sandy clay and river sand were derived is the old glacial banks of the boulder-clay. It was from these banks that the sands and clays were washed, and the finer the particles the greater the distance to which they were removed. Hence the sand generally lies nearest the boulder-clay banks, while the sandy clay rests further off. Hence, also, we find sandy plateaus so constantly associated with the boulder-clay banks at the lower levels; for there these banks have uniformly been reached and washed by the sea when it stood at its former higher level. In Southern Norway, however, it must be remarked, that the distribution of the sand is especially to the south of the banks, a fact which seems to indicate the influence of fresh-water currents pouring out seaward from the land on the north.

No shells are found in this sandy clay or river sand, which overlie the previously mentioned clays, Nos. (1) and (2). The height to which they rise is, at the lower levels, largely dependent on the original height of the nearest boulder-clay banks. Thus, in the more open country, they have been traced to elevations of from 400 to 600 feet. Up the river courses again they rise to any elevation, so long as the valley slopes exist, and the lake basins continue to present themselves.

The accompanying section shows the succession of the various beds. They rest on the underlying granite. On the surface of the granite, where an angle of the rock has afforded shelter, is a heap of boulder-clay with boulders 1.*

* Such heaps of boulder-clay below the marl belong to the oldest part of the deposit, and are peculiarly interesting, as the only records of that earlier

Overlying this, but resting generally on the rock, is the marl 2 of No. I. division. To this succeeds the second division: 1. The shell-clay, &c.; 2. The brick-clay; 3. Sand

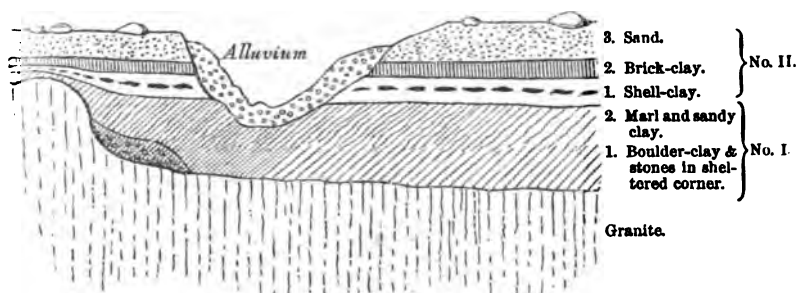


Fig. 2.

or sandy clay, on which large boulders rest loosely. Through these a valley has been cut, where, however, the section presented in its banks is as often obscured by alluvium.

A diminution of the glacial cold, and along with this a great thawing of the inland ice, seems to have occurred between the deposition of the marl with its associated shell-beds and the later shell-clay, for the more markedly arctic character of the Fauna is wanting in the latter clay. Hence the division of the beds into the glacial and the post-glacial groups.

This shell-clay was a regular marine formation, deposited in a perfectly orderly and quiet state of things; but after its formation there must have followed a time of floods, during which the many dams, forming basins and lakes in the valleys, were cut through, and the clay, the sand, the

period which we possess. They were formed at the same time that the rock surfaces in the lower districts were striated under the general covering of ice. The greatest mass of the boulder-clay, however, formed during that period, seems to have been either thrust into the sea and left at depths still below our reach, or so mixed up with the boulder-clay of a later period as to be undistinguishable from it. That our existing boulder-clays generally belong to a later period is obvious, since they are contemporary with the marls, which are a sea deposit, formed on the submerged land, which had previously been furrowed and scratched by the old ice-covering grinding over its surface, and leaving its detritus only in such places as were protected from the motion of the ice.—[Translator.]

debris and rolled stones of the interior, were washed further and further down. The striking absence of fossil shells in the brick-clay, and in the sandy clay and flood sand; the complete identity of the brick-clay and the inland fresh-water clays; the expansion of the plateaus of stratified sand around original banks,—all these strongly indicate such a flood period.

III. In regard to the third group (p. 7), consisting of alluvium, peat, and other fresh-water formations, it is unnecessary to say more, than that their extension at certain points indicates that the lakes to which they owe their origin formerly stood at a higher level than that which they now occupy.

From an examination then of the whole upper covering of our country, we learn that the older part of this formation has a markedly arctic character, and that the oldest masses of all are moraines. We are entitled, therefore, I think, to conceive of Norway, at the close of the Tertiary period, as being in a true glacial condition. Its ice covering had an outward movement like the present inland ice of Greenland. By this ice "rolled stones" and debris were borne outwards to the sea-shore, where large and small blocks of stone were floated off, and carried away on ice-floes. Hence originated those huge moraine masses (far too vast to have been produced by mere separate valley glaciers) which cover the whole of the flat land near the coast, and which may be traced on both sides of the Christiania Fjord. From this universal covering of moving land ice resulted the first great network of scorings and scratches, as well as the moraine masses extending out to the edge of the sea. Then followed a diminution of the glacial intensity. Instead of a continuous covering of ice, there were separate glaciers, which laboured down from the various mountain slopes into the open valleys at their feet. All the loose material which lay in their way they bore off as moraines, either along their sides or in front, or where two valleys converged as medial moraines. The process of striation thus went on anew in all these valleys; and in this way two

different sets of scratches, lying across one another, might sometimes be produced.

In our larger valleys we find masses of debris, stones, and sand, lying like long terraces along the one or the other side, sometimes along both. These are side moraines. Further down, nearer the mouth of the valleys, and also higher up, more among the mountains themselves, the same masses—banks of boulder-clay—lie right across the valleys. These are terminal moraines, left where the glaciers pushed out in the lower districts towards the coast, or higher up among the mountains, as the last glaciers shrunk and retreated.

The glaciation ceased by a gradual melting and disappearance of the ice. During this perhaps protracted period, the mud-laden glacier rivers were bearing down with them the detritus with which they were charged, depositing it in the sea or in inland lakes, wherever the water was sufficiently still to allow it to sink. Hence our marine and lake clays. Running water, too, acted on the moraines, washing out the sand from these masses of debris, and spreading it around and over them. Hence the stratified upper part of the banks and the sand spread far out over the clay around the old banks.

On the coast, where the sea received the matter thus brought down, shell-fish made their appearance, partly buried in the clay, partly in the form of entire beds. These beds are found up to a height of 500 feet. To this extent, therefore, the land must have stood at a lower elevation.

Let us now see in what order this glacial matter has been sorted and heaped up by the sea:—

At the bottom of all we find, except where they have been afterwards swept away, sand and rolled stones. These were the scratching agents. It was they which were dragged by the ice across the rock faces; and if we are to deduce from the blocks the direction of the friction, it is these blocks we must examine; but as they are most frequently broken, reduced in size, and often rounded, they are generally called not blocks but rolled stones, though the name is not strictly correct, and they should rather be called "scouring stones." They have not been rolled, but have mutually

crushed one another, and, fixed in the ice like a diamond in a glazier's cutting-instrument, they have scored the hill side with scratches and furrows.

Over these "scouring stones" and sand lie the various clays:—Calcareous and marl clays, in such districts as presented Silurian limestones and schists to supply detritus to the glacier rivers. Above this, shell-clay, wherever the height was not too great, or the streams of cold fresh-water too powerful to admit of molluscan life. Then follows brick-clay without shells, belonging probably to the time when the flush of water from the land was at its height. Finally, sand and sandy clay above all.

Stones brought from a distance may be found here and there through all these layers; but most distinctively the erratic blocks are found lying stranded on the top of the banks. (See fig. 2, p. 13). All parties are agreed that ice was the agent which brought down these blocks.

When moraines had been left stretching across the valleys, they would often in the upper reaches become, during the time of the glacier's melting, a dam for an inland lake. The floating particles carried along by the river would be deposited in such quiet basins, and hence come those inland clays which lie at heights above that of the marine formation. In such lakes, just as in the sea below, ice-floes might drift about laden with small and large blocks, which thus became erratic. Even in the ice itself, far up in the interior, large temporary lakes might be formed when the ice was melting rapidly; and there blocks might be drifted on floes in a totally different direction from that in which the inland ice itself was moving and tracing its furrows. This may serve to explain how in some places the direction in which the erratic blocks have journeyed does not correspond with that of the scratchings.

Such phenomena, then, as we have been considering, could not be the work of a flood. A flood strong enough to furrow both hard and soft rock, and even to give their shape to the mountains, with their weather and lee sides—a flood of such incredible force could not have failed to sweep away before it all these loose masses of scratched stones and debris of sand and mud. Yet here we find them

set down, sometimes in the midst of a valley, sometimes along its edges, nay, sometimes stretching right across, with only a small opening cut through at one side, obviously by a stream quietly escaping from a lake. Nor can we see whence such a flood could come. The mountain tops would not supply it; nor would a sea wave in any form produce the regularity and uniformity of the phenomena, nor transport the boulders, nor trace the furrows.

Neither is Sir Charles Lyell's theory of floating icebergs much more satisfactory. Even were the striations such as icebergs could produce, which is not the case, some traces of the sea and its inhabitants would surely have been found on the higher levels, depressed beneath the sea, as the land, on this theory, was to the extent of 4000 feet at least; since to that height the scratching has been distinctly traced. Arctic shells are indeed found abundantly among our clays and sands, but never at a greater height than 600 feet above the sea.

Further, it must be remembered that oscillations of the land, of such frequency and extent as this theory imagines, are eminently improbable in Norway, where the entire series of the secondary and tertiary strata are wanting, and where, therefore, the land has been singularly stationary.

Shells from the Earlier and Later Beds of the Glacial Formation. By Dr M. SARS, Professor of Natural History, Christiania.

[The only beds here examined are in the southern part of Norway, and even of these the author states that the examination is far from complete. In explanation of the author's headings in the following list, it must be remembered that while the beds are to be found at all elevations from the sea-level to a height of 500 feet, yet at the lower levels (up to 200 feet) such shell-beds as present themselves on the surface belong to a later period; while the older beds must be sought either on the surface at the higher levels, or buried deep under the later clays and sands at the lower levels. But while this holds true in general, partial exceptions to the rule may be expected, as in the case mentioned

at p. 25, where a current has probably prevented the accumulation of later deposits.—*Translator.*]

Note.—The lists of shells are printed exactly as given by the author. Where his name is not the one received here, the translator has supplied the other with the mark *syn.*

HIGHER LYING (OLDEST) SHELL BEDS.

Killebo in Rakkestadt. Height 400 to 440 feet.

This bed rests partly on the solid rock, partly on an intervening bed of clay four or five feet thick.

Balanus porcatus, da Costa, Darw. *B. crenatus*, Brug., Darw.
Ochthosia Stroemii (Lepas), Müll., *syn.* *Verruca strömia*.
Serpula triquetra, L. *S. vermicularis*, Müll.
Trophon clathratus (Murex), L., *syn.* *Fusus banffius* var. *major*,
 Loven. *Nassa reticulata* (Buccinum), L. *Natica nitida*,
 Don. *Littorinalittorea* (Turbo), L. *L. littoralis* (Nerita), L.
Trochus tumidus, Mont. *T. magus*, L. *Puncturella no-*
china (Patella), L. *Lepeta cæca* (Patella), Müll., *syn.* *Propili-*
dium ancyloide. *Pilidium fulvum* (Patella), Müll. *Anomia*
ephippium, L. *A. patelliformis*, L. *Ostrea edulis*, L.
Pecten islandicus, Müll. *Mytilus edulis*, L. *Modiola*
modiolus (Mytilus), L. *Cardium edule*, L. *Lucina borealis*
 (Venus), L. *Venus ovata*, Penn. *Tapes pullastra* (Venus),
 Mont. *Astarte arctica*, Gray. *A. compressa*, Mont. *A.*
elliptica, Brown. *Tellina proxima*, Brown. *Mya truncata*, L.
Saxicava pholadis (Mytilus), L., *syn.* *S. rugosa*. *S. arctica*
 (Mya), L.

Skjældalen in Aremark. Height 470 feet.

On the rock rests a layer of clay of varying thickness, in which are shells. Above this is the shell-bed ten feet thick, in the midst of which occurs a bed of clay deposited from water.

Balanus porcatus, da Costa, Darw.
Trophon clathratus (Murex), L., var. *major* Lovén. *Buc-*
cinum undatum, L. *B. grønlandicum*, Chemn. *Natica*
clausa, Sow. *Tapes pullastra* (Venus), Mont. *Tellina*
proxima, Brown. *Mya truncata*, L., et var. *Udewallensis*,
 Forb. *Saxicava pholadis* (Mytilus), L., *syn.* *S. rugosa*.

(Five new species since found. Names not given.—*Translator.*)

Skullerud in Höland. Height 450 feet.

On the top is a layer of sand mixed with shells, below is a sandy clay with shells. At the bottom common sandy clay.

Balanus porcatus, da Costa, Darw.

Tritonium despectum (Murex), L., *syn.* *Fusus antiquus*. *Trophon clathratus* (Murex), L., var. *major* Lovén. *Buccinum grönladicum*, Chemn. *Natica clausa*, Sow. *Pecten islandicus*, Müll. *Mytilus edulis*, L. *Tellina solidula*, L.

(Thirteen new species since found. Names not given.—*Translator.*)

Björum in Asker. Height 460 feet.

Tritonium despectum (Murex), L., *syn.* *Fusus antiquus*. *Buccinum undatum*, L. *Natica grönlantica*, Beck. *Aporrhais pes pelicani* (Strombus), L. *Littorina littorea* (Turbo), L. *Dentalium abyssorum*, Sars. *Pecten danicus*, Chemn. *Leda pernula* (Arca), Müll. *Cyprina islandica* (Venus), L. *Panopæa norvegica* (Mya), Spengl.

OLDEST CLAY UNDERLYING LATER DEPOSITS.

Brynd, near Christiania. Height 200 feet and upwards.

The shells occur in a sandy marl, with thin layers of sand interspersed.

Siphonodentalium vitreum, Sars. *Arca raridentata*, Wood, var. *major*, Sars. *Yoldia pygmæa* (Nucula), Münst.

Somewhat Higher.

Pecten danicus, Chemn.

Skibtvædt. Height 90 feet.

Close beside the church; on the very edge of the River Glommen, and pretty deep below the surface of the clay-flat; in beds of sand, which occur in clay beds.

Siphonodentalium vitreum, Sars. *Arca raridentata*, Wood, var. *major*, Sars. *Leda pernula* (Arca), Müll. *Pecten danicus*, Chemn.

Bakkehuus, near Christiania.

Nassa reticulata (Buccinum), L. *Cyprina islandica* (Venus), L. *Corbula nucleus*, Lamk.

Lekum in Edsberg. Height 220 feet.

Here the shell-beds are of small extent and thickness. They lie in the upper part of the clay flat which extends all around.

Littorina littorea (Turbo), L. *L. littoralis* (Nerita), L.
Mytilus edulis, L.

Grorud near Christiania.

In the sea-weed clay, which takes the place of the shell-clay.

Tellina solidula, L. *Saxicava pholadis* (*Mytilus*), L. *syn. S. rugosa*.

Glaeng and Sarpen. Height 80 to 100 feet.

Ostrea edulis, L. *Pecten danicus*, Chemn. *P. opercularis* (*Ostrea*), L. *Cardium echinatum*, L. *Cyprina islandica* (*Venus*) L., (often perforated by *Cliona celata*, Grant.)
Astarte compressa, Mont. *Tellina proxima*, Brown.

Kaholmen near Dröbak. Height 20 to 30 feet.

Lowest clay.

Tritonium despectum (*Murex*), L., *syn. Fusus antiquus*. *Pecten maximus*, L. *Lima excavata* (*Ostrea*), Gmel. *Cyprina islandica* (*Venus*) L.

Oculina prolifera (*Madrepora*) L. (also in a mass of clay 20-30 feet and upwards above the sea, south of Dröbak).

CLAY OF BOTH AGES, EARLIER AND LATER.

Lower and Upper Foss near Christiania.

Partly in the marl, interspersed with thin layers of sand; partly in the overlying shell-clay.

Turritella communis, Risso. *Philina scabra* (*Bulla*), Müll.
Dentalium abyssorum, Sars. *D. entalis*, L. *Siphonodentalium vitreum*, Sars. *Ostrea edulis*, L. *Pecten islandicus*, Müll. *P. danicus*, Chemn. *P. tigrinus*, Müll. *P. sinuosus*, Turt. *Arca raridentata*, Wood, var. *major*, Sars. *Nucula tenuis* (*Arca*), Mont. *Leda caudata* (*Arca*), Don. *L. pernula* (*Arca*), Müll., *syn. L. oblonga*. *Yoldia pygmaea* (*Nucula*), Münt. *Cardium edule*, L. *C. suecicum*, Reeve. *Isocardia cor* (*Cardium*), L. *Syndosmya nitida* (*Mya*), Müll., *syn. S. intermedia*. *Corbula nucleus*, Lamk. *Saxicava arctica* (*Mya*), L.

(Eighteen new species since found. Those named are echinoderms; *Ophiura sarsii*, *Otenodiscus crispatus*, *Tripylus fragilis*, previously unknown in the glacial formation.—*Trans.*)

LATER SHELL-BEDS, BELONGING TO THE LOWER LEVELS.

Hövig near Christiania. Height 100 to 150 feet and upwards.

In sand, resting directly on the rock with clay around.

Tritonium despectum (Murex), L., *syn.* *Fusus antiquus*. *Nassa reticulata* (Buccinum), L. *Cerithium reticulatum*, da Costa. *Aporrhais pes pelicani* (Strombus), L. *Littorina littorea* (Turbo), L. *L. littoralis* (Nerita), L. *Rissoa membranacea* (Turbo), Ad. *syn.* *R. labiosa*. *Trochus cinerarius*, L. *Dentalium entalis*, L. *Ostrea edulis*, L. *Pecten varius* (Ostrea), L. *Nucula margaritacea*, Lamk., *syn.* *N. nucleus*. *Cardium fasciatum*, Mont. *C. pygmæum*, Don. *Tapes decussata* (Venus), L. *Astarte elliptica*, Brown. *A. compressa* (Venus), Mont. *Corbula nucleus*, Lamk. *Saxicava pholadis* (Mytilus), L., *syn.* *S. rugosa*. *S. arctica* (Mya), L.

Heistadt near Eidanger Fjord. Height 100 to 150 feet and upwards

In digging a well, shells found in the sand exactly as at Hövig.

Nassa reticulata (Buccinum), L. *Cerithium reticulatum*, da Costa. *Rissoa parva*, da Costa. *Emarginula reticulata*, Sow. *Acmaea virginea* (Patella), Müll. *Venus ovata*, Penn. *Lucina borealis* (Venus), L.

Ravnsborg near Christiania. Height 50 feet and upwards.

In sand resting directly on the rock.

Ostrea edulis, L. *Cardium echinatum*, L. *Astarte compressa* (Venus), Mont. *Lucina borealis* (Venus), L.

Aafoss near Skien. Height 100 feet and upwards.

At the bottom is sand and sandy clay, above this lies a bed of shells more than ten feet thick.

Balanus crenatus, Brug., Darw.

Buccinum undatum, L. *Purpura lapillus* (Buccinum), L. *Natica nitida*, Don. *Littorina littoralis* (Nerita), L. *Lacuna vincta* (Turbo), Mont. *Trochus cinerarius*, L. *Patella pellucida*, L. *Anomia patelliformis*, L. *A. aculeata*,

Müll. *Pecten islandicus*, Müll. *Cardium fasciatum*,
 Mont. *Venus ovata*, Penn. *Tapes pullastra* (*Venus*),
 Mont. *Tellina proxima*, Brown. *Saxicava pholadis* (*My-*
tilus), L., *syn.* *S. rugosa*. *Thracia villosiuscula*, Macgill.

(Twenty-seven new species found. Names not given.
 Another bed found, about 30 feet lower, containing thirty-
 two species, all now living in the neighbourhood, among
 which are *Echinus dröbachiensis* and an *Echinocyamus angu-*
losus.—Translator.)

Löveidet near Skien. Height 120 feet and upwards.

Shell-bed rests on the rock.

Balanus porcatus, da Costa, Darw. *B. crenatus*, Brug., Darw.
Buccinum undatum, L. *Nassa incrassata* (*Buccinum*),
 Ström. *Natica nitida*, Don. *Velutina lævigata* (*Helix*),
 L. *Littorina littorea* (*Turbo*), L. *L. littoralis* (*Nerita*), L.
Patella vulgata, L. *Acmaea virginea* (*Patella*), Müll.
Pilidium fulvum (*Patella*), Müll. *Dentalium abyssorum*,
 Sars. *Anomia ephippium*, L. *A. patelliformis*, L. *Pec-*
ten islandicus, Müll. *P. striatus*, Müll. *P. danicus*,
 Chemn. *P. tigrinus*, Müll. *P. sinuosus*, Turt., *syn.* *P. pusio*.
Mytilus edulis, L. *Leda pernula* (*Arca*), Müll. *Venus*
striatula, Don. *V. Ovata*, Penn. *Tapes pullastra* (*Venus*),
 Mont. *Astarte sulcata*, da Costa. *A. compressa*, Mont.
Mactra elliptica, Brown. *Tellina proxima*, Brown. *Mya*
arenaria, L. *Thracia villosiuscula*, Macgill. *Pholas can-*
dida, L.

Echinus dröbachiensis, Müll.

Ommedsstrand near Skien. Height 100 feet and upwards.

Sandy clay, and fine sand mixed with shell fragments, in
 all some 50 feet thick.

Balanus porcatus, da Costa, Darw. *B. crenatus*, Brug., Darw.
Serpula triquetra, L. *S. polita*, Sars.
Buccinum undatum, L. *Nassa incrassata* (*Buccinum*), Ström.
Natica nitida, Don. *Velutina lævigata* (*Helix*), L. *Lit-*
torina littorea (*Turbo*), L. *L. littoralis* (*Nerita*), L.
Rissoa parva, da Costa = v. R. interr. *R. striata* (*Turbo*)
 Ad. *Emarginula reticulata*, Sow. *Capulus hungaricus*
 (*Patella*), L. *Patella vulgata*, L. *Acmaea virginea* (*Pa-*
tella), Müll. *Pilidium fulvum* (*Patella*), Müll. *Den-*
talium abyssorum, Sars. *Anomia ephippium*, L. *A.*
patelliformis, L. *Pecten islandicus*, Müll. *P. danicus*,
 Chemn. *P. tigrinus*, Müll. *P. striatus*, Müll. *P. sinuosus*,

Turt., *syn.* P. pusio. Mytilus edulis, L. Nucula margaritacea, Lamk., *syn.* N. nucleus. Leda pernula (Arca), Müll., *syn.* L. oblonga. Cardium echinatum, L. Lucina borealis (Venus), L. Cyprina islandica (Venus), L. Venus ovata, Penn. V. striatula, Don. Astarte sulcata, da Costa. A. compressa, Mont. Mactra elliptica, Brown. Syndosmya alba (Mactra), Wood. Solen ensis, L. Saxicava pholadis (Mytilus), L., *syn.* S. rugosa. Cochloidesma prætenue (Mya), Pult. Pholas crispata, L. P. candida, L. Waldheimia cranium (Terebrat), Müll. Echinus dröbachiensis, Müll. E. esculentus, Linn.

(Twenty-one new species found. Names not given.—*Trans.*)

Remarks—

1. *Tritonium despectum* (Murex), L., *syn.* *Fusus antiquus* E. Forbes.—Distinctively arctic; common in extreme north of Norway, Russia, Greenland; very rare and small at Christiansund; Lat. 63°10'.

2. *Trophon clathratus*, L., *syn.* *Fusus banffius*, E. Forbes.—Arctic and circumpolar; diminishes in size as it goes south as far as Ireland. In north of Norway from 12 to 15^{mm} in length, Greenland 24^{mm}, Spitzbergen 32^{mm}. Fossil, frequent; 40^{mm} in length.

3. *Buccinum grælandicum*, Chemn.—Arctic and circumpolar; frequent in Finmark, but not further south, size there 53^{mm}; fossil, very frequent, and up to 67^{mm} in length.

4. *Natica clausa*, Sow.—Arctic and circumpolar, size there 26^{mm}; common in Finmark up to 20^{mm}; south as far as Bergen, but dwarfed and rare, 4 to 5^{mm}; fossil, frequent and full sized, up to 30^{mm} in length.

5. *Natica grælandica*, Beck., *syn.* *N. pusilla*.—Arctic and circumpolar; common in north of Norway, becoming rarer and smaller towards the south, reaching the coast of Britain; fossil differs slightly from the common living *Natica grælandica*, but approaches the form of the full-grown specimens of these.

6. *Trochus magus*, L.—Canaries and Mediterranean, and north as far as Shetland; not living in Norway; fossil, one specimen, agrees exactly with living species.



7. *Siphonodentalium vitreum*, Sars.—Only found in Finmark, north coast of Norway; rare; fossil pretty frequent in the oldest beds. This fact, and its present rarity, seem to indicate that it is dying out.

8. *Pecten islandicus*, Müll.—Distinctively Arctic; found on north coast of Norway and Russia, Greenland and east coast of America; goes south as far as the Christiania fjord and Bohuslän, but does not reach Britain; diminishes in frequency and size as it comes south; fossil frequent, agrees in size with the northern specimens.

9. *Lima excavata* (*ostræa*), J. C. Fabr.—A large species, well known on our west coast, and probably to be reckoned as Arctic, though found to the south as far as Bohuslän; it lives only in the region of the deep-sea corals, at from 150 to 300 fathoms; fossil, in the lowest clay, also found amongst the innumerable dead masses of *Oculina prolifera* (which see, p. 25), at a depth below the sea of from 10 to 15 fathoms on the Dröbak bank, which must therefore have been elevated at least 135 fathoms, or 800 feet.

10. *Arca raridentata*, Wood. — *Pectunculoides*, Scacchi.—Common along our whole coast; extends south to Gibraltar and even to the Ægean Sea, but in the far north attains a much larger size than to the south, and may therefore be considered as arctic; varies very much in form, both in length and depth, and in the projection of its umbo. The hinge teeth increase in number with age. It is both in its living and fossil state covered with a *hairy* membrane, though Forbes and Hanley state the contrary. Fossil, it is very frequent in the lowest clay, where the shells are found in pairs. It is so much larger than, and different from, the living specimens, as at first to appear specifically distinct, but the transitions between the two prove their identity. The great projection backwards of the ventral margin, and the great number of cardinal teeth—two respects in which the difference between the fossil and the living form is most marked—appear to be merely the effects of age and growth. It is a curious fact, that this species, which extends back into the newer Pliocene of Apulia and Sicily, and the Coralline Crag of England, appears there in the smaller form which it bears in its more southern habitats at the

present day, while the Post-pliocene specimens of the species, the descendants of the small Pliocene race, and the ancestors of the small living race, excel them both in size in the proportion of three to one.

11. *Astarte arctica*, Gray.—Arctic and circumpolar; extends south as far as Bergen, but is there exceedingly rare; does not reach Britain. (It is *very* rare in Britain, see Forbes and Hanley.—*Translator*.)

12. *Tapes decussata* (Venus), L.—A Mediterranean species; extends to England, on the south coast of which it is common, in Wales and Northumberland very scarce; not found further north. (Forbes and Hanley speak of it as *very* rare in north Britain, but as found buried in gravel in Skye and Shetland.—*Translator*.) Fossil, found in the later shell-beds of the lower levels, and is as large as the largest Mediterranean living specimens.

13. *Panopæa norvegica* (Mya.), Spengler.—Arctic and circumpolar; extends to the Cattegat and England, but there, as on our own coast, is exceedingly rare. Fossil, much more frequent than in the living state.

14. *Pholas candida*, L.—Extends from the Mediterranean to Scotland, and perhaps Norway. Fossil, frequent in the later shell-beds of the lower levels.

15. *Oculina prolifera* (Madrepora) L.; *Lophella prolifera*, Edw. and Haime.—This coral is found on our western and northern coast, at the vast depth of from 150 to 300 fathoms,—never less than this,—and forms pretty large bush-shaped masses of 2 feet in diameter. Fossil, it is found in the lowest clay, and also in vast quantities on the shoal which lies out in the Fjord to the west of Dröbak, and which has been already stated to be from 10 to 15 fathoms (60 to 90 feet) deep. It cannot have been washed thither by a stream or tide, for it is firmly attached to the solid rock just as it grew; of course, however, it is dead, and stripped bare of the formative polyp which could not survive the increased temperature of the shallower sea. We have thus the best possible proof of an elevation of the land in this upheaval of a portion of the sea-bottom, and that to the extent of at least 135 fathoms, or 800 feet; and with this evidence it is interesting further to compare

the fact of *Balanus* shells being found on the solid rock by Brogniart at Udevalla, 200 feet above the sea; by Lyell 14 miles north of Kured 100 feet above the sea; and by Keilhau near Hellesaa, in Aremark, 450 feet above the sea.

The above sketch is very imperfect, scarcely any of the places mentioned having been satisfactorily examined by any zoologist; hitherto it has been geologists alone who have paid any attention to the subject, and they, occupied with many other matters, have given little time to the laborious task of collecting the numerous species which exist.

On such imperfect data, therefore, it would be rash to generalise; one or two inferences are however obvious.

1. We are assured that we are really dealing with Post-pliocene formations, since all the organic remains which have been found belong without exception to living species.

2. The fossiliferous strata of this period are distinguishable into two classes. The first class, both from stratigraphical position and organic contents, is obviously older. It includes those masses of shells which ascend to 470 feet above the level of the sea, and the deepest situated clay, which rises to a height of 240 feet above the sea. The second class belongs to a later period, and consists of the lower lying shell-beds, which rise to a height of 200 feet above the sea, and the surface clays, which rise to 350 feet above the sea.

3. Among the fossil species of the older beds there are some which no longer inhabit our southern coast, but have their true habitat on the north coast of Norway, and some or all of the other arctic coasts. Such are *Tritonium despectum* (which, however, appears also in the later beds), *Buccinum islandicum*, *Natica clausa*, *Siphonodentalium vitreum*, *Astarte arctica*.

4. Other species also present in the older beds are more frequent, and generally larger than those of the same species now inhabiting our southern coasts, and specimens rivalling the fossils in frequency and in size must be sought in the far north. Such are *Trophon clathratus*, *Natica grœnlandica*, *Pecten islandicus* (which is also found in the later beds), *Lima excavata*, *Arca raridentata*, *Panopœa norvegica*.

5. Those in the later beds, on the contrary, with few

exceptions, agree perfectly with those now inhabiting our southern coasts.

6. From the fossil species we may infer that in the earlier post pliocene period a more northern fauna occupied our southern coasts than now prevails there ; in other words, such a fauna as now exists only in the arctic zone. On the other hand, in the later period indicated by the newer beds, the arctic species gradually retired to the far north, and were replaced by the more southern forms, which now constitute so large a part of our marine fauna.

Loven, in his "Öfversigt af Ventensk. Akad. Forh., 1846," p. 254, has also arrived at a similar conclusion from an examination of the fossil mollusca in the neighbourhood of the west coast of Sweden ; with this difference, however, that, according to him, " the arctic species present themselves at those spots which are furthest from the coast, while the nearer any locality is to the shore, the rarer are the arctic species, these being gradually replaced by more southern species." Whereas, with us, mere distance from the coast does not afford a test of the age of a fossil, and the older or arctic species are found in the shell-beds at the greatest heights above the sea, and in the clay furthest below the surface, independent of distance from the coast. [This view is remarkably confirmed by the Elie shell-bed described by the Rev. Thomas Brown, F.R.S.E., in his paper read before the Royal Society Edinburgh, March 2, 1863. *Translator.*]

7. Finally, two species have been noticed, viz. *Trochus magus* (in the older beds), and *Tapes decussata* (in the later beds), and to these, perhaps, should be added *Pholas candida*, which no longer inhabit our coasts, but are distributed from the Mediterranean to England. In regard to these species, we can only conjecture that in the Post-pliocene epoch they reached Norway, not from England but from the Mediterranean, between which and the North Sea a communication existed to the east of the Alps.

Nor does this fact stand alone. I have already elsewhere called attention to two other indications of such a connection between these seas at an earlier period, viz.—the well known fact that some of the characteristically

arctic species are found as fossils in Italy and Sicily (*Forhandl. i Vid. Selsk. i Christiana*, 1858, p. 78); and further, the very remarkable fact, that in both these seas perfectly identical species are found living which are absent on the intervening coasts of the Atlantic. Such are, *Nephrops norvegicus* (Cancer), *L. Lotaabyssorum* (Nilss.), (= *Elongata*, Risso. L.), *Sebastes imperialis* (Cuv.), and *Macrourus (Lepidoleprus) cælorhynchus* (Risso). To these I have recently (l. c. p. 86) added two shells discovered by me in the sea at Bergen, viz.—*Cerithium vulgatum* (Brug.), and *Monodonta limbata* (Phil.), which are found living in the Mediterranean, but on none of the intermediate coasts between that sea and Norway.

To explain, then, the presence of these species in the Mediterranean and in Norway, with their absence from the intervening coast, we must, as I have already elsewhere remarked (see *Bemærkninger over det adriatiske Havs Fauna*, *Magaz. f. Naturv.* 7 B. 1853, p. 395), suppose, either that nature, which has so often produced analogous but distinct species, in localities far remote from one another, has in this instance, created perfectly identical species at two so distant points, a fact of which we have absolutely no other instance; or, which is far more probable, that the existence of these species dates from a period—viz., the Post-pliocene, when towards the east a connection existed between the Mediterranean and the North Sea, which at a later period was interrupted by the elevation of the Alps.

In a paper since communicated to the "Nyt Magazin," Dr Sars distinguishes the fossil-bearing beds of the glacial formation into four kinds, different in age and constitution.

1. The older shell-beds, which lie at from 300 to 500 feet above the sea. Loose heaps of entire or broken shells, the bivalves seldom in pairs. The mass of shells sometimes pure, sometimes mixed with fine sand.

These facts prove that such beds are of littoral origin, formed in bays, where their sheltered position preserved them from being swept away and scattered by the waves during the subsequent elevation of the land. That they

have not been carried to their present position is obvious, from the frequency of the littoral *Balanus*, still entire, adhering to the shells and stones, and even to the rock itself.

Further, the shells found in these masses are, without exception, of littoral (for the most part) or laminarian species, the latter, as is the case on existing sea-beaches, having been washed up from the lower zone. Not one deep-water species has ever yet been found in these shell-beds.

The arctic character of these beds is still further confirmed afresh by the specific nature, the numerical proportions,* and the actual size and shape of the shells which form them.

The elevation of the land seems to have taken place gradually and without violence, accompanied by a corresponding diminution in the arctic character of the fauna.

2. The older clay containing shells rarely aggregated in any number, generally sporadically dispersed or thinly spread, usually entire and well preserved, and the bivalves in pairs.

This is plainly a deep-water formation, the shells and Echinoderms found in it belonging to the deep-sea zone. *Dentalium abyssorum*, *Yoldia pygmaea*, *Y. lucida*, *Arca glacialis*, &c., are frequent. *Siphonodentalium vitreum*, *Isocardia cor*, &c., are rarer. *Serpula polita* is frequently found attached to the erratic blocks which abound in this clay. The fauna of this clay is markedly arctic. Thus *Arca glacialis* lives now only in Finmark, Spitzbergen, and Melville Island: *Yoldia intermedia*, discovered by Dr Sars in Finmark at 100 fathoms deep, and *Siphonodentalium vitreum*, a rare Finmark species, living from 40 to 100 fathoms deep, and apparently approaching extinction, are much more frequent in the clay-beds.

The evidence of the Echinoderms supports that hitherto obtained from molluscs alone. *Ophiura Sarsii* lives along the whole coast of Norway, at from 20 to 100 fathoms and upwards, but is more frequent and larger in the far north. *Otenodiscus crispatus*, distinctively arctic, comes south to Christiansund, Lat. 63°-10, at from 40 to 200 fathoms. *Tripylus fragilis*, peculiar to Norway, is larger and more frequent in the north, but comes south to Bergen at from 30 to 120 fathoms.

* Thus *Saxicava rugosa*, *Mya truncata*, and *Pecten islandicus*, form from 50 to 80 per cent. of the whole shells.

The age of these beds cannot be determined, like that of the littoral shell-beds, from their elevation above the sea, since they have been deposited sometimes in deeper, sometimes in shallower water. It is their organic contents, or their geological relations, which must determine the question of their age.

3. Later shell-beds, less than 300 feet above the sea. These, like the older shell-beds, are of littoral formation, but they have entirely lost the arctic character of the former. *Natica clausa* and *Trophon clathratus* have utterly disappeared, as has also *Buccinum Grœnlandicum*, its place being taken by the abundant *B. undatum*. *Pecten islandicus* is rarer; and *Mya truncata* and *Saxicava rugosa*, though frequent, belong to the thin-shelled variety. On the other hand, the well-known species of the southern coast of Norway, which are wholly wanting in the older shell-beds, are here present in large numbers. Finally, these later beds are remarkable for their wealth, the older for their poverty of species—the largest proportion in any one locality being for the former 65, for the latter 21.

4. Later clay, containing sporadic, or but thinly spread, generally well-preserved shells.

Of these beds Dr Sars was only able to examine one—the new locality referred to at page 22, as near Aafoss, some twenty or thirty feet lower than the previously examined shell-bed there. This, if not a littoral, was at least a shallow-water formation. It is not at all arctic in character, the thirty-two species of shells which were found there being all inhabitants of the southern coast of Norway at the present day. The presence of fragments of *Echinus dröbachiensis*, and an *Echinocyamus (spatangus) angulosus*, is interesting, echinoderms being very rare in the glacial formation.

Note on Lemania variegata of Agardh. By GEORGE LAWSON, LL.D., Ph.D., Professor of Chemistry and Natural History in the Queen's University of Canada.*

In the first part of the second volume of Bishop Agardh's

* Read before the Botanical Society of Edinburgh, 9th April 1868.

"Species Algarum," published in 1828, an alga said to have been found "in *fluviis Americæ borealis*," was described under the name of *Lemania variegata*. Agardh's original description of the plant appears, however, to have been published in the Stockholm Transactions in 1814, to which I have no means of access at the present time. The specimen upon which the species was founded had been given to Agardh by Olaf Swartz, his first master in Algology, who obtained it from the collector, the Rev. Dr Mühlenberg of Lancaster, in Pennsylvania. Not having been met with by subsequent observers, *Lemania variegata* has been looked upon as a long-lost plant.

In a parcel of specimens of cryptogamic plants sent to me in August 1862, by Mr John Macoun of Belleville, Canada West, a most zealous and successful explorer, I at once recognised a *Lemania*, remarkable for its extremely rigid, prominently moniliform, curved filaments, attenuated towards the base and apex, and regularly marked throughout by alternate bands, dark and white,—agreeing, in fact, very well with Agardh's description of *L. variegata*. I doubt not that the Belleville plant is conspecific with that of Agardh, and it is probably the identical form described in the "Species Algarum."

Lemania, Bory.

Generic character.—Fronds bristle-like, rising in clusters from a common adherent base, cartilaginous or corneous, continuously tubular, more or less nodose (brown, dull-green, blackish or parti-coloured), the tube-membrane composed of two distinct closely adherent strata of cells, those of the outer stratum minute, irregularly polygonal, closely united pavement-wise in radiating groups, those of the inner stratum rounded and not conformable, much larger than the others. Spores (so called by authors) in seriated stalked tufts, inside the swollen joints of the tube, and arising either from a central axis (according to Dr W. J. Thomson), or from the inner peripheral layer of cells, or from both.

This genus is named *Lemania* by Bory, in honour of M. Lemaire of Paris, "a modest naturalist not less learned in

botany than in the other branches of science," embraces three species of aquatic algæ of very remarkable aspect and structure, which grow attached to stones, rocks, wood, &c. in the bottoms of shallow, rapid, fresh-water streams. Unlike most fresh water algæ, they have dense compact tissue giving them firm consistence; they are rich in nitrogen, and when burned yield ammoniacal vapours. The plant usually consists of a little tuft of stiff erect or curved bristle-like fronds, which adhere by a common discoid root to submerged objects. The minute structure of these plants has been illustrated very fully by authors at different times, from Vaillant (1727) downwards, with singularly conflicting results. The most recent, and perhaps most valuable contribution that has been made to the history of *Lemania*, is the remarkably lucid description of Dr W. J. Thomson, in the Transactions of the Botanical Society of Edinburgh, vol. vi. page 243, to which I would refer observers as an excellent basis for further inquiry, although I have been unable (probably from my specimens being too matured) to confirm some of Dr Thomson's results. Mr Thwaites of Ceylon has carefully studied the early development of the frond, and states that the spores at first vegetate into slender confervoid filaments, with long joints containing spirally-arranged endochroms. The filaments constitute a sort of prothallus or pro-embryo, the initial state of the plant. After a time thick branchlets, the germs of the perfect and permanent frond, spring from the cells of the confervoid filament; they are at first wholly dependent upon the cell from which they rise, but soon acquire rootlets at their base, and, rapidly elongating, grow into the densely cellular, opaque, cartilaginous bristle-like tubes, so characteristic of the mature plant in this genus.

L. variegata.—Fronds tufted, of simple bristle-like tubes, rigid, corneous, attenuated towards the base and apex, moniliform, with black or dark-brown swollen elliptical nodes, and pellucid, colourless, constricted internodes of equal length.

“ *Hippuris fluviatilis petræa nuda Virginiensis*, Pluk. Phyt. tab. 193, fig. 7.” (Agardh.)

"*Lemania variegata*, Agardh, Act. Holm., 1814, tab. 2, fig. 3." (Agardh.)

"*Lemania variegata*" (Agardh), Species Algarum, vol. ii. page 7.

In the bed of the Moira above Belleville, Canada West, on rocks, rare (August 1862), John Macoun, sp. "In fluviis Americæ borealis. Specimina a Muhlenbergio missa communicavit Swartz" (Agardh).

The following eight forms of this extremely interesting genus have been described by Agardh :—

1. *L. fluviatilis* = (Conferva fluviatilis lubrica setosa, Equiseti facie, Horse-tail River Conferva, *Dillenius*, Hist. Musc., tab. vii. fig. 47. Conferva fluviatilis, *Linn.*, *Mohr*, *Roth.*, &c. Polysperma fluviatilis, *Vauch.* Chantransia fluviatilis, *DC.* Lemania corallina, *Bory.* Nodularia fluviatilis, *Lyngb.*) This is the more common British species which I gathered in quantity in a stream on the Ochil Hills, near Stirling, in 1857. It has also been recorded as growing near Bangor (*Dillenius*), in Winterbourne Stream, Lewes (*W. Borrer*); at Hamsell, and at the waterfall at Harrison's rocks (*E. Jenner*); Aberdeen, abundant (*Professor Dickie, M.D.*); Ireland, frequent (*D. Moore*); Scandinavia, Germany, France, Corsica.
2. *β. tuberculosa* = (Nodularia fluviatilis ramosa, *Lyngb.*) Denmark.
3. *γ. media* = (Conferva fluviatilis, *Dillw.*, E.B., t. 1763). England.
4. *δ. fucina* = (Lemania fucina, *Bory.* Chantransia dichotoma, *DC.* France, chiefly in Bretagne.
5. *ε. subtilis* = (Lemania subtilis, *Agardh* (in Act. Holm. 1814, t. 2, f. 4, *Kützing*.) Sweden, &c.
6. *L. torulosa* = (Conferva fluviatilis nodosa Fucum æmulans, Sea Horse-tail-like Conferva, *Dill.* Hist. Musc. tab. vii. fig. 48. Conferva torulosa, *Roth.*, *Mohr.*, *Dillw.*, &c. Lemania incurvata, *Bory.*) Recorded as occurring in mountain streams near Ludlow, Salop (*Dillenius*), Anglesea (*Rev. H. Davies*); also in France, Germany, Kentucky, United States (*Dr Short* in *Harvey, Nereis*).
7. *β. usneoides* = (Conferva usneoides, *Wallr.*) Saxony.
8. *L. variegata* = (Hippuris fluviatilis petraea nuda Virginienensis, *Pluk.*) Belleville, Canada West (*J. Macoun.*) United States, Pennsylvania? (*Mühlenberg*).

Probably *L. flu.*, *ε. subtilis*, and *L. torulosa*, *β. usneoides*,
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may be found, on investigation, to be well-marked species. The various forms deserve a careful examination, and I would beg to direct the attention of British botanists to the subject.

Descriptions of New Genera and Species of Diatoms from the South Pacific. By R. K. GREVILLE, LL.D., F.R.S.E., &c.* (Plate I).

The diatoms described in the present communication were all obtained from dredgings made on various coasts in the South Pacific, and kindly placed in my hands by my obliging correspondent Dr Roberts of Sydney. Many other novelties, of equal interest, contained in these dredgings, will (D.V.) form the subject of future papers.

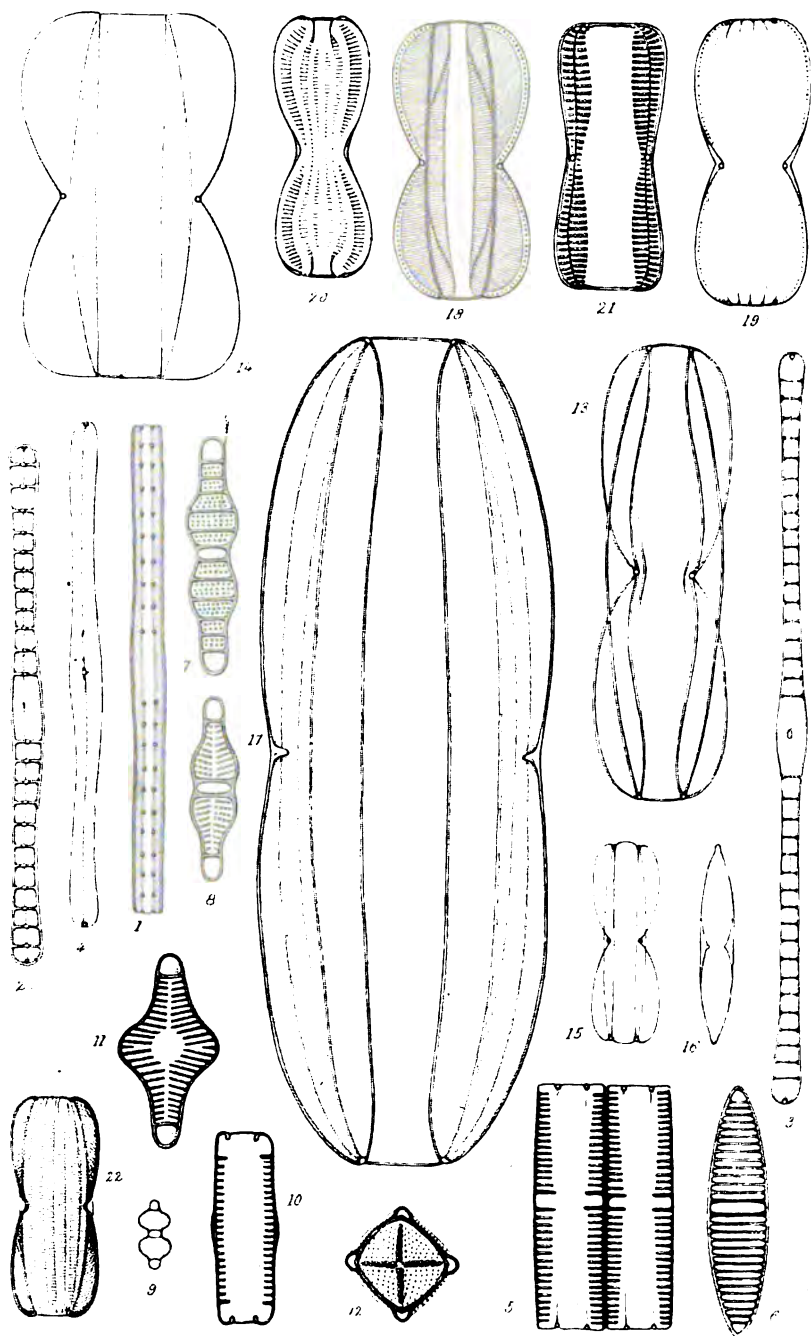
STICTODESMIS, nov. gen., Grev.

Frustules elongated (sessile?), in front view linear, rectangular, with two conspicuous rows of puncta disposed in pairs; side view linear, with terminal and central nodules, median line, and numerous transverse pervious septa.

It is difficult to indicate the systematic position of this curious diatom. The transverse septa, as shown in the lateral view, and very elongated form, are suggestive of some affinity with *Olimacosphenia*: but from that genus and the other, *Licmophora*, it differs in the linearity of the frustules, and in the presence of nodules and a median line. as well as in the different nature of the septa themselves. With the *Fragillariæ* it might be compared in its pervious septa, conjoined with the similarity of both ends of the frustule; but *Stictodesmis* is not compressed in the lateral view; and here also the median line and nodule, and position of the septa, indicate a different structure and different relations. The genus, in fact, cannot be referred to any of the families as at present constituted.

Stictodesmis australis, n. sp., Grev.—(Pl. I. figs. 1–4).
Hab. Harvey Bay, Queensland, and at Port of France, New Caledonia; in dredgings communicated by Dr Roberts of Sydney.

* Read before the Botanical Society of Edinburgh, 14th May 1868.





Frustules much elongated, from '0055" to '0100", the breadth scarcely more than '0004". Front view linear, rectangular, with two longitudinal rows of brilliant puncta in pairs, at some distance from the margin, the space between the rows and the margin very finely striated, while that between the rows forms a smooth, blank line. Lateral view linear, slightly dilated at the middle, and generally, also, more or less at the rounded ends, with a median line, and terminal and central nodules, the line being situated slightly nearer to one side than the other; transverse septa pervious, strong, subequidistant, numerous, upwards of twenty in the shorter frustules, nearly double that number in the longest; very fine transverse striæ also cover the whole space. Besides the above front and lateral views, the valve must be noticed separately, being distinguished by the median line and nodules and the fine striæ only, and is so thin and transparent that it is easily overlooked. The septa belong to an internal arrangement, being perceived in the front view to be placed at about a third of the entire breadth of the frustule from each side, or, in other words, from the valve. The puncta, which shine like brilliant points of light, are of course the sutural terminations of the septa viewed vertically. The shorter figures are from the Harvey Bay dredging, the long one from New California.

PLAGIOGRAMMA.

Plagiogramma costatum, n. sp., Grev.—Front view with nearly straight sides and sharp angles; valves lanceolate, with two central vittæ; striæ costate, pervious, 10 in '001". Length about '0030" (figs. 5, 6).

Hab. New Caledonia; in a dredging communicated by Dr Roberts.

This species is well distinguished by its clear costation and central pair of vittæ. It is by no means unfrequent in the dredging.

Plagiogramma spectabile, n. sp., Grev.—Valves constricted in the middle, then dilated and slightly lyriiform; vittæ numerous (about twelve), with intermediate transverse lines of minute puncta. Length about '0030". (fig. 7).

Hab. New Caledonia ; in a dredging communicated by Dr Roberts. Very rare.

Of this diatom I have only seen two examples ; but the characters are so remarkable, that there can be no doubt of its novelty as a species. The central constriction, it now appears, is not uncommon in this genus.

Plagiogramma constrictum, n. sp., Grev.—Valve constricted in the middle, then suddenly dilated and lyriform, with terminal and central vittæ ; striæ moniliform, interrupted, 11 in '001". Length about '0025" (fig. 8).

Hab. New Caledonia ; in a dredging communicated by Dr Roberts.

Allied to *P. lyratum*, but differing in the smaller size of the frustule, in the relatively much broader valve, more abrupt constriction, more approximated central vittæ, and much fewer striæ.

Plagiogramma Atomus, n. sp., Grev.—Very minute ; valve with terminal and central vittæ ; the intermediate spaces spherically dilated ; striæ few, pervious. Length, '0005" (fig. 9).

Hab. Woodlark Island ; in a dredging communicated by Dr Roberts.

The smallest of the genus hitherto discovered, distinguished by the very deep constriction and spherical form of the dilated portions, each of which contains about half a dozen striæ.

OMPHALOPSIS, nov. gen., Grev.

Frustules united into a filament ; lateral view cruciform, with central nodule, terminal vittæ, and interrupted transverse striæ.

This genus is allied on the one hand to *Plagiogramma*, on the other to *Glyphodesmis*. From the former it differs in the absence of central vittæ ; from the latter, in the presence of terminal vittæ, as well as in difference of structure, the remarkable clathrate cellulation being wholly wanting. In the cruciform lateral view it resembles *Odontidium*, (?) *Harrisonii* of Smith, and *O. speciosum* of Brightwell (both placed by Ralfs in his genus *Dimere-*

gramma), but is far removed in structure by its vittæ and central nodule.

Omphalopsis australis, n. sp., Grev.—(Figs. 10, 11).

Hab. Woodlark Island ; in a dredging communicated by Dr Roberts. Not unfrequent.

Frustules varying considerably in size, but generally about '0022" in length, rectangular, the angles slightly rounded, and the striæ forming a very narrow marginal border ; valve cruciform, about '0012" in breadth in the middle ; nodule large, prominent, circular ; striæ 11 in '001", strong, submoniliform, somewhat oblique, radiating in the centre, where a short one usually occurs at the outer margin, interrupted, leaving a narrow blank median space which terminates at the vittæ which cut off the rounded ends. At first sight, the large boss-like nodule and robust striæ convey an impression that the valve may belong to a species of *Glyphodesmis*, but, as already remarked, the terminal vittæ and different structure essentially separate it.

AMPHITETRAS.

Amphitetras parvula, n. sp., Grev.—Lateral view with the sides slightly convex, the angles rounded, containing a large pseudo-opening ; centre marked by two cruciform lines of cellules which intersect each other in the middle and give off radiating lines of minute puncta. Breadth between the opposite angles '0015" (fig. 12).

Hab.—New Caledonia ; in a dredging communicated by Dr Roberts. Very rare.

Frustule very small, quadrangular, the sides very slightly convex, the rounded angles being formed by the pseudo-openings which are very conspicuous, the true margin, continued on the inner side of the openings, strong and dark, outside of which the edge of the connecting zone appears constituting a secondary margin, which passes to the apices of the pseudo-openings. The internal space is equally divided by two lines crossing in the middle, composed of two contiguous rows of cellules, which are mostly visible in consequence of the shadow they produce, while from each side of these rows pass inclined lines of very minute puncta.

This exceedingly distinct little species is about the same size as *A. crucifera* of Kitton, and resembles it to a certain extent in the internal cruciform arrangement. But in the last-named species there are no pseudo-openings, the produced rounded angles being simply an extension of the internal surface. There is a difference also in the cruciform appearance, which in *A. crucifera* is produced by the position and larger size of puncta, similar to those which fill up the rest of the space, and which do not cause the line of shadow above mentioned. Although the form is exceedingly rare, I have seen and examined several specimens.

AMPHIPRORA.

Amphiprora eximia, n. sp., Grev.—Front view elongated, oblong, rounded at the ends, deeply constricted at the middle, one supplementary wing convex and widely overlapping the constriction, a second passing over the nodule where it is somewhat constricted, and within the nodule a short elevated longitudinal ridge. Length about .0060" (fig. 13).

Hab.—Curteis Straits, Queensland; in a dredging communicated by Dr Roberts.

A singularly graceful species. Nothing can be more beautiful than the harmony of the various intersecting lines. As far as I can make them out, there are two supplementary wings, the largest very broad, arising at the terminal angles of the valve, and with a gentle curve overlapping the constriction so much as to bring the convexity of the supplementary wing almost into a line with the widest part of the frustule. The second supplementary wings (if they be truly wings) also arise from the same point, soon curve outwards, and then, becoming constricted at the nodule, have a somewhat lyrate appearance. Lastly, there is just within the nodule a little ridge (or abortive wing?) which is parallel to, and follows the curve of the second supplementary wing, and disappears at less than half way between the nodule and the ends. I have not been able to resolve the striæ.

Amphiprora lata, n. sp., Grev.—Front view very broad (breadth more than half the length), truncate at the ends,

with the corners very widely rounded, rather deeply and acutely constricted; supplementary wing narrow, convex, not reaching the constriction; central portion with faint lines or folds. Length about $\cdot 0040''$, breadth $\cdot 0028''$ (fig. 14).

Hab.—Curteis Straits, Queensland; in a dredging communicated by Dr Roberts.

It was not until I had seen a number of examples of this diatom that I began to regard it with any confidence. There can be no doubt that some of the *Amphiprora* are liable to considerable variation; and the very broad, squat appearance of the frustules now before me made me suspect that they might turn out to be a variety of some large species. All the specimens, however, present precisely the same characters, and I am consequently prepared to offer it as a genuine species. From *A. maxima* of Gregory it differs not only in form, and in the much narrower supplementary wings, but very strikingly in the more truncated ends. The striæ are readily brought out.

Amphiprora delicatula, n. sp., Grev.—Minute, hyaline; front view oblong, with rotundato-truncate ends and deep lateral constriction; supplementary wing very narrow, slightly overlapping the nodule; lateral view lanceolate, the apices slightly produced and minutely truncate. Length about $\cdot 0025''$ (figs. 15, 16).

Hab.—Woodlark Island; in a dredging communicated by Dr Roberts. Not uncommon.

A most delicate and graceful species, easily overlooked on account of its small size and hyaline character. I have been unable to make out the striæ.

Amphiprora? superba, n. sp., Grev.—Very large; front view elongated oblong, slightly constricted, widest part about half way between the ends and middle, each extremity gradually converging to the angle of the connecting zone, which is truncate; supplementary wings rather broad, linear, directed inwards with two longitudinal lines between them and the margin. Length $\cdot 0108''$ (fig. 17).

Hab.—New Caledonia; in a dredging communicated by Dr Roberts. Extremely rare.

This fine diatom may perhaps be regarded as belonging

doubtfully to the present genus. In some respects it is not unlike a gigantic *Amphora* ; but the position of the nodules, and the apparent existence of what I have called supplementary wings, are opposed to its being referred to that genus. There are, however, some very aberrant forms discovered by my friend the late Professor Gregory, and several more recently observed by myself, which render the generic discrimination of *Amphiprora*, in the absence of the valvular view, somewhat difficult. The panduriform outline in some instances disappears altogether, the constriction is occasionally scarcely perceptible, and the wing inconspicuous. The striæ in the present diatom are transverse, 27 in '001", and sufficiently evident.

Amphiprora nitida, n. sp., Grev.—Small ; front view panduriform, broadly rotundato-truncate at the ends, rather deeply and sharply constricted, margin with a row of minute puncta ; supplementary wings linear, forming a nearly straight line, which passes within the constriction ; another ridge-like line converging towards the middle at each end. Striæ conspicuous. Length from '0020" to '0035" (fig. 18).

Hab.—Curteis Straits, Queensland ; in a dredging communicated by Dr Roberts.

A beautiful and apparently very distinct small species, characterised at once by the lines of the supplementary wings and the superficial ridges. The supplementary wing is linear, passing on each side from end to end in a very slight curve just within the nodules. At the ends it turns almost at a right angle to join the middle portion, and thus a sort of parallelogram is produced, which, when focussed for itself, is very striking. Another very characteristic line seems to be caused by an elevated ridge, commencing on each side at the angle of the middle portion or connecting zone, curves outwards until it reaches the margin of the supplementary wing, which it then follows for about a third of its length, when it leaves it and curves in a similar way towards the middle portion at the opposite end. The surface is thus much undulated, as is shown by the different directions taken by the striæ.

Amphiprora lineata, n. sp., Grev.—Front view oblong,

broadly rounded at the ends, and moderately constricted at the middle, with a marginal row of very minute puncta; surface marked with straight longitudinal lines or folds (about seven); striation obscure. Length about '0038" (fig. 19).

Hab.—Woodlark Island; in a dredging communicated by Dr Roberts. Rare.

In this species we find no distinct trace of supplementary wings, but in their place a few equidistant longitudinal lines or folds, which cannot be prominent as they cast very little shadow.

Amphiprora? Clepsydra, n. sp., Grev.—Front view oblong, rotundato-truncate at the ends, much, but not sharply constricted at the middle; striæ coarse, forming a narrow intra-marginal band, and two inner ones still narrower, the two outermost following the curve of the margin, and converging at the ends. Length '0034" (fig. 20).

Hab.—Curteis Straits, Queensland; in a dredging communicated by Dr Roberts. Very rare.

Here, again, we have a diatom very different indeed from what has been regarded as typical of *Amphiprora*. It will be perceived that the system of striation is quite unlike, the outer band being distinctly radiate. The inner bands may be regarded as supplementary wings. The constriction is not sharp, as in most of the preceding species, but rather deeply concave.

Amphiprora? paradoxa, n. sp., Grev.—Front view quadrangular, with truncated ends, and rounded corners very slightly and gradually constricted at the middle; supplementary wings broader than the primary ones, and also constricted opposite the nodule; striæ robust, moniliform, 10 in '001". Length '0035" (fig. 21).

Hab.—Curteis Straits, Queensland; in a dredging communicated by Dr Roberts.

In the robust and distinctly moniliform striæ of this diatom we have another deviation from the ordinary structure of *Amphiprora*, so strongly marked as to render it evident, that although it may be convenient in the meantime to refer some of these forms to that genus, a re-arrangement will soon become necessary. The possession of the



valve might serve to show more exactly their relations; but the probability of obtaining a side view of the frustule is so remote, that I prefer giving figures of these interesting objects at once, to laying them aside for an indefinite period. It only requires to be understood that their position in the genus is provisional. There is no reason why the same latitude should not be allowed in the publication of Diatoms as in that of other plants; and especially of other algæ. For example, while engaged in the description of these *Amphiproræ*, the 49th Part of my friend Professor Harvey's admirable "Phycologia Australica" has appeared, in which, under *Cryptonemia decipiens*, he remarks, "In now referring it to *Cryptonemia*, I must be understood to do so provisionally, until the discovery of its fruit enable us to assign it its proper place in the system." This is a most reasonable privilege. It may be observed, with regard to the frustules now before me, that there is all the appearance of a distinct wing, which becomes narrower as it approaches the constriction, as in the known *Amphiproræ*.

Amphiproræ? undulata, n. sp., Grev.—Small; front view quadrangular, with rounded angles, and nearly straight sides, and a slight, somewhat notch-like constriction; surface rather coarsely striated, and undulated with longitudinal ridges, the two principal ones curving outwards, and passing just within the constriction. Length about '0030" (fig. 22).

Hab.—Curteis Straits; in a dredging communicated by Dr Roberts.

This species is as rectangular in its outline as *A. pusilla* of Gregory. The two outer prominent ridges or folds evidently occupy the place of supplementary wings, while the inner ones are merely undulations, sufficiently decided, however, to throw the striæ which cross them out of focus. I have examined a number of examples, but have not succeeded in finding a valve. The latter would no doubt assist in determining the genus in the case of aberrant forms; but I apprehend that in most instances the specific character will be best obtained from the front view.

DESCRIPTION OF PLATE I.

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|---------|---------------------------------|-------------|
| Fig. 1. | <i>Stictodesmis australis</i> , | front view. |
| 2. & 3. | " " | side view. |
| 4. | " " | valve. |
| 5. | <i>Plagiogramma costatum</i> , | front view. |
| 6. | " " | side view. |
| 7. | " <i>spectabile</i> , | side view. |
| 8. | " <i>constrictum</i> , | side view. |
| 9. | " <i>Atomus</i> , | side view. |
| 10. | <i>Omphalopsis australis</i> , | front view. |
| 11. | " " | side view. |
| 12. | <i>Amphiteras parvula</i> , | side view. |
| 13. | <i>Amphiprora azimia</i> , | front view. |
| 14. | " <i>lata</i> , | front view. |
| 15. | " <i>delicatula</i> , | front view. |
| 16. | " " | side view. |
| 17. | " ? <i>superba</i> , | front view. |
| 18. | " <i>nitida</i> , | front view. |
| 19. | " <i>lineata</i> , | front view. |
| 20. | " ? <i>Clepydra</i> , | front view. |
| 21. | " ? <i>paradoxa</i> , | front view. |
| 22. | " ? <i>undulata</i> , | front view. |

All the figures are x 400 diameters.

On the question, Is Oxide of Arsenic, long used in a very small quantity, injurious to Man? By JOHN DAVY, M.D., F.R.SS. Lond. & Edin.*

The facts which led me to propose the above question were the following:—In Cumberland, within a stone's cast of the Coast's Railway, between Whitehaven and Broughton, is the little church of the parish of Whitbeck, and also the farm-house of Whitbeck-head,—names these derived from the small mountain stream which descends from Black Comb, and so rapidly as to be an almost white line of foam. The hamlet, situated at the foot of the hill, consists of the farm-house just mentioned, and of five cottages, each occupied by a family dependent for water on the rivulet, "the Beck," their inmates using no other. The same water is drunk by the cattle of the farm, and by the poultry, fowls, geese, and ducks; and, as regards all, with one excep-

* Read at the Meeting of the British Association in 1862.

tion, its wholesomeness has never been called in question. The one exception has been in the instance of ducks, which, according to report, never could be reared there.

No attempt that I am aware of was made to account for this exceptional effect until about two or three years ago, when the water was examined by Mr Arthur H. Church, and was found by him, it was said, to contain arsenic,—an alkaline arseniate, and in a determinate quantity. The first notice of this gentleman's results that I saw was in an extract from the *Whitehaven Herald*, in which it was stated that "the arsenical water is habitually used for every purpose by the inhabitants of the little village of Whitbeck, and with beneficial results so apparent that one might be justified in paradoxically characterising it as a very wholesome poison, the deadly elements in solution being productive of the most sanatory effects." It is immediately added, "It is true ducks will not live if confined to Whitbeck; and whilst trout abound in the neighbouring rivulets, no fins are ever found in this arseniated stream." Further, it is said, that "when the railway was being carried past Whitbeck, the first use of the water produced the usual marked effects on the throats both of the men and horses employed on the works. The soreness of mouth from which they first suffered soon however disappeared, and in the horses gave rise to that sleekness of coat assigned as one of the principal effects produced by the administration of minute but repeated doses of arsenic."

This notice, which I have since found is nearly a travesty of Mr Church's, published in the "*Chemical News*" for August 25, 1860, exciting my curiosity, I paid a visit to the stream on the 27th August of last year, ascending to one of its sources, probably 700 feet above the level of the sea, where it issues from the gallery of a forsaken mine, and thence followed it in its descent to the village. In this its course, of about a mile, it was joined by other small streams, and by one from a lateral valley equal in size to that which it meets. The temperature of the water close to the mine-gallery was 48° at the time that the open air was 60° ; lower down, just before entering the village, it was 58° . I then collected a portion of water for examination, and had other

portions sent me; one in October, when the stream was of moderate size; another, in January, when it was lower than usual, after a frost of many days continuance; and a fourth I collected myself when I last visited the stream, viz. on the 3d of August of this year, when it was of its average height, and when the water of the mine-branch was $51^{\circ}5$, and of the lateral branch $50^{\circ}5$.

Before mentioning the results of my trial of these specimens of water, I shall give a brief summary of the information which I obtained from the inhabitants of the village.

The statement about the ducks was verified; and also that no fish had ever been seen in the stream. Nothing was known of its having had any of the effects attributed to it when first used by the labourers and horses employed on the railway; indeed, there was a positive contradiction given by the mother of a young family of children who had come to reside at the village after the line was opened. Nor could I learn that any of the inhabitants thought that any special good effect on their health was due to their habitual use of the water. My chief informant was Mrs K., a widow of about 60, of Whitbeck-head, residing in the farm-house, the mother of twelve children, all of whom were born and brought up in the same place, and had good health. She was not a native of the spot, but had lived there uninterruptedly about forty years—that is, from the time of her marriage; and when I saw her she was active for her age, was in good health, and said she always had been healthy. According to her, so fruitless had been all her attempts to rear ducks that she had given them up as hopeless. Their death, she stated, was preceded by a wasting; and that, on examination after death, their stomachs were found “unusually crammed” or distended; all other poultry doing well.

I shall now speak of the water of the Beck. It had the general character of the mountain-streams of the Lake District, was perfectly clear, and colourless and tasteless. Of the several specimens obtained, I found the specific gravity the same, and the same as that of distilled or rain-water. When evaporated to dryness the residue was very small, a pint yielding about $\cdot 25$ of a grain—and, from the different

specimens tried, not varying more than one-tenth of a grain. In each instance this residue was found to consist chiefly of common salt; it tasted of this salt, and in solution was copiously precipitated by nitrate of silver. Mixed with the common salt was a little magnesia and lime, both probably in combination with sulphuric acid, as sulphate of lime and of magnesia, the presence of the acid being denoted by nitrate of barytes; a trace, too, of oxide of arsenic was detected in each, and it may be inferred, in combination with potash, a trace of which also was obtained. The arsenic was detected not only by the test of the ammoniaco-nitrate of silver, but also by reduction to its metallic state by sublimation, after mixture with ferro-cyanide of potassium. Of the several specimens of water tried, that procured in October, when the stream was about its ordinary size, afforded a somewhat stronger trace of the metal than either the earlier or later, the one in August 1861, taken when the stream was swollen after rain; the other in January 1862, during a frost of several days' duration, when probably the water was frozen at its sources,—the stream then lower than common. In noticing, however, the later, I should except the last, that taken in August last, when the stream was of about its average volume, and the indications of arsenic nearly the same as those of October. In the instance that the trace was strongest, judging from comparative experiments with oxide of arsenic,—experiments of reduction by sublimation,—the quantity of oxide contained in the pint was only about '008 grain, or '064 grain to the gallon.

Before concluding this part of my subject I may mention that in the second visit which I made to the stream, then in company with Dr Robinson of Newcastle, we paid particular attention to the herbage and plants growing close to or in the water, and were unable to observe anything in their appearance indicative of a noxious influence on them, with the exception—if it be an exception—of a single tree, a maple (*Acer campestre*), growing where its roots were washed by the descending stream; of its branches many were dead, and such was its general aspect that it might well represent a poisoned tree; yet, that it was poisoned, is

very doubtful, judging from the circumstance that its fructifying power was not apparently impaired, seed-vessels being abundant on it. At my last visit I saw sheep browsing on the grass that grew at the edge of the stream : and then I was struck not only by the variety of the kinds of plants, but also by the freshness and healthy appearance of those so near as to be in the way of being sprinkled with the spray of the little torrent. At the same time that I had the advantage of the company of Dr Robinson, we sought for larvæ in the sediments of a little pond, which had been formed as a reservoir for the purpose of working a mill belonging to the farm, but without success. The only living things we saw, and these were not in the water, were some small spiders, which were actively skimming its surface. In a portion of the sediment which I brought away, subjected to the microscope, I found a few remains of Infusoria. It may be worth mentioning, that when the bottom of the pond was stirred much air was disengaged. Dr Robinson was so good as to have a portion of it collected and sent to me. I found it to be almost entirely azote ; it was not diminished by agitation with milk of lime, and hardly perceptibly by phosphorus ; nor was it inflammable. As the deposit had an ochrey hue, perhaps the separation of the oxygen, which most waters contain, was owing to the action of a protoxide of iron in process of conversion into the peroxide.

To revert to the arsenical impregnation. If it be asked whence it was derived, we have not far to seek : the obvious inference is, that the very minute quantity of arsenic in the water is owing to the water flowing over mineral substances of which arsenic is a constituent part, such as arsenical pyrites, which I am informed was abundant in the mine,—(a vein that was worked for cobalt), and is of common occurrence in the side of Black Comb. Dr Robinson, who had collected some specimens of it, favoured me with a portion of them ; and from water, in which they had been immersed a few hours, traces were detected of oxide of arsenic, and also of iron and copper ; and in confirmation, I may state, that when, at my last visit, I tested on the spot the two streams before adverted to, which by their junction form

the Beck, I found the indications of arsenic in the one, of about the same degree of distinctness as in the other.

As regards the question proposed,—Is arsenic in very small quantity, long continued in use, injurious to man?—do not the results described and the experience related justify an answer in the negative? To me they seem to warrant this conclusion, and also the conclusion equally, that no appreciable good effect is produced in the instance of man, or in that of the other animals mentioned, by its long-continued use; and in confirmation of this I may state, that one of the inhabitants of the village, a blacksmith, who on taking up his abode there some years ago, was subject to attacks of asthma, assured me that his complaint had from year to year been becoming worse rather than better.

In a recent number of the *Journal de Médecine et Chirurgie Pratiques*,* there is a notice of a thermal spring (its temperature that of 106° Fahr.) in the regency of Tunis, called Bon-Chater, the water of which, from the analysis made of it by M. Guyon, contains a large proportion of arsenic in the state of arseniate of potash and of soda, larger indeed than any other yet known, being as much as four grains to the gallon; and yet we are assured, that it was used not only by the natives with impunity, but also by M. Guyon and his party.

If, then, not noxious, used as above described, is there not another inference to be drawn,—viz., that arsenic, especially in combination with either of the alkalies, is not one of those substances which accumulate in the system; is, on the contrary, as commonly admitted, one of those which are got rid of by excretion,—its elimination varying, probably, in different persons and in different animals as to rate, and varying, it may be, proportionally in the measure of its tolerance? Analogy seems to favour this view. The tolerance of certain substances taken in slowly increasing doses is very remarkable. I was assured by the medical man under whose care “The Opium-Eater,” the late Mr De Quincey placed himself, that at the height of his indulgence he took half a pint of laudanum daily. The same practitioner assured me that another patient of his, a lady whom I knew,

* Tom. xxxii. 869.

took daily two drams of solid opium, but not without injury to her fine mental faculties. I may mention another instance, and one more extraordinary, which came to my knowledge at Constantinople, of a Turk who took daily, and for fifteen years, two drams of solid opium, with half that quantity of corrosive sublimate. My informant knew the man well,—he was a porter at the arsenal (of which my friend was an official), and though in shattered health, was capable of his easy duties. I obtained a specimen of what was used as corrosive sublimate, and, testing it, found it to be this compound, of ordinary purity. The main motive for its use, he said was, that it improved—increased, the effects of the opium. This instance, according to the apothecaries in Pera, is not a solitary one of its kind; their experience having taught them that the opium-eater has recourse to it when the narcotic has ceased to have its original effect.

The tolerance of certain substances, in connection with their elimination, has not, I am disposed to think, had all the attention which it deserves. Why is it that individuals living under the same circumstances, using the same kind of diet and the same drinks, are some of them subject to gout, whilst others are free from it? Is it not because the latter possess in their organisation a greater power of eliminating the *causa mali*—lithic acid? Why is it that nitrate of silver long used, in some persons (the very few) becomes—that is the metal—lodged in the cutis vera, occasioning there its peculiar discolouring effect, whilst in the majority, after long use, no effect of the kind is witnessed? Is not the rationale of the difference the same?

We have had it confirmed that the water of Whitbeck is, in the solitary instance of the duck, injurious; ultimately it would appear to be fatal. This may be owing, it may be conjectured, to the duck seeking its food so much more in water, and to a delicacy, an idiosyncrasy rendering it peculiarly susceptible of the effect of arsenic; a peculiarity itself which may be connected with a feeble eliminating power, especially of the kidneys, its urine, like that of birds in general, approaching to a solid.

A somewhat similar instance of great susceptibility of the effects of arsenic occurs, I believe, in the charr, one of the

most delicate, the most sensitive of noxious influences of all the Salmonidæ. It is an established fact, that in the two or three instances in the Lake District, that mines have been opened in the vicinity of lakes, their drainage entering the lakes, the charr has either entirely disappeared, as at Ulswater, or has become very scarce, as at Coniston-water, the trout, a hardier fish, remaining. I have examined the water flowing from the Coniston copper mines into the lake of the same name, and have detected in it distinct traces of arsenic; and I have obtained a like result from the examination of the water flowing into Ulswater, which receives the drainage of an adjoining lead mine.

Recently, much alarm has been felt from apprehension of arsenic-poisoning, owing to the great use made of some of its compounds, especially of Scheele's green—the cuprio-arsenate—in colouring paper and articles of dress. Probably the apprehension is an exaggerated one; but yet there seems some foundation for it, especially in the case of women employed in making artificial flowers, and in that of ladies wearing those flowers in ball-dresses, coloured by the arsenic compound. If there were only a risk of injury, and that there certainly is in these instances, of a poisonous effect, ought it not to act powerfully as a prohibition to the further use of articles so coloured? Even the most insensate votaries of fashion who may defend crinoline, asserting that, with caution, they are safe from combustion, will hardly venture to defend the use of a poisoned dress, from the slow effects of which, undermining health and spoiling beauty, no ordinary precautions can be effectual.

When we reflect on the abundance of arsenical pyrites in most of our mining districts, and the numerous springs issuing from mineral strata, the water of which is used by the inhabitants, it may be asked how is it that arsenicated water is not of common occurrence, and its poisonous qualities well established? The answer to this, I apprehend, is not difficult, resting on two facts—one, the slight solubility of the oxide of the metal in cold water; the other, its harmlessness in very minute quantities. As to the first, so slowly is it soluble at ordinary temperatures, that we learn, on good authority, it may be digested for many days with

100,000 times its weight of water, and yet not be entirely dissolved. This, its slowness of solution, is probably owing to its comparatively high specific gravity, in consequence of which the oxide falls to the bottom, and to the strong coherence of its particles, resisting its disintegration. Whatever the explanation may be, the property is a fortunate one for animal life; for were it readily soluble, were it more than very slightly soluble, how direful might have been the results! May we not view it as one of the many happy adaptations which are so common in the economy of Nature, and an instance of the limitation of the noxious, or its neutralization, or even more, of its transition into positive good? this last, on the supposition that arsenic used in a very minute quantity may really be beneficial.

Illustrations of the Significance of certain Ancient British Skull Forms. By DANIEL WILSON, LL.D., Professor of History and English Literature, University College, Toronto.

During a recent visit to Washington, I availed myself of the facilities afforded me by Professor Henry, the learned secretary of the Smithsonian Institution, to examine with minute care the ethnological collections preserved there, including those formed by the United States Exploring Expedition; and especially a highly interesting collection of human crania. The latter includes those of Esquimaux and Tchuktchi, a number of compressed and greatly distorted Chinook and other Flathead skulls, as well as examples of those of other Indian tribes, both of North and South America; and of Fiji, Kanaka, and other Pacific islanders. On my return I spent a short time in Philadelphia, chiefly for the purpose of renewed study of the valuable materials of the Mortonian collection; and while there enjoyed the opportunity of examining, in company with Dr Aitken Meigs, a series of one hundred and twenty-five Esquimaux crania obtained by Dr Hayes in his Arctic journey of 1854.

The materials for craniological investigation which such collections supply can scarcely be surpassed in some of their

departments, and invite to very diverse researches by the illustrations they are calculated to afford. It chanced, however, that my attention had been recently recalled to an old subject of speculation, relative to the possible modification of the forms of ancient British crania by some of the very causes which so materially alter those of many American tribes; and this accordingly influenced me in part, in the notes I made of the collections both at Washington and Philadelphia, and will now give direction to some remarks bearing on the same inquiry.

Among the most prized crania in the collection of the Academy of Natural Sciences at Philadelphia is the celebrated Scioto Mound skull. But though on a former visit I made the ancient mound crania an object of special study, this most remarkable example of the series was not then included among them; and I now examined the original for the first time. The result of this examination was to satisfy me that the remarkable form and proportions of that skull are much more due to artificial influences than I had been led to suppose from the views published in the "Smithsonian Contributions to Knowledge."* The vertical view, especially, is very inaccurate. In the original it presents the peculiar characteristics of what I have before designated as the truncated form; passing abruptly from a broad flattened occiput to its extreme parietal breadth, and then tapering with slight lateral swell, until it reaches its least breadth immediately behind the external angular process of the frontal bone. The occiput has been subjected to the flattening process to a much greater extent than is apparent from the drawings; but at the same time it is accompanied by no corresponding affection of the frontal bone, such as inevitably results from the procedure of the Chinooks and other Flathead tribes; among whom the desired cranial deformation is effected by bandages crossing the forehead, and consequently modifying the frontal as much as the parietal and occipital bones. On this account, great as is the amount of flattening in this remarkable skull, it is probably due solely to the undesigned pressure of the cradle-board acting

* Ancient Monuments of the Mississippi Valley, pl. xlvii. and xlviii.

on a head of remarkably brachycephalic proportions and great natural posterior breadth. The forehead is fully arched, the glabella prominent, and the whole character of the frontal bone is essentially different from the Indian type. The sutures are very much ossified, and even to some extent obliterated. So early as 1857, when discussing Dr Morton's theory of one uniform cranial type pervading the whole ancient and modern tribes of North and South America, with the single exception of the Esquimaux, I remarked, "I think it extremely probable that further investigation will tend to the conclusion that the vertical or flattened occiput, instead of being a typical characteristic, pertains entirely to the class of artificial modifications of the natural cranium familiar to the American ethnologist alike in the disclosures of ancient graves, and in the customs of widely separated living tribes."*

This idea received further confirmation from noticing the almost invariable accompaniment of such traces of artificial modification, with more or less inequality in the two sides of the head. In the extremely transformed skulls of the Flathead Indians, and of the Natchez, Peruvians, and other ancient nations by whom the same barbarous practice was encouraged, the extent of this deformity is frequently such as to excite surprise that it could have proved compatible with the healthful exercise of any vital functions. But the aspect in which it is now purposed to review the subject of artificial modifications of the human cranium, in relation to ancient British skull forms, was suggested, in the same paper above referred to, when pointing out the mistaken idea adopted by Dr Morton, that such unsymmetrical conformation, or irregularity of form, is peculiar to American crania.† The latter remark, I then observed, is too wide a generalisation. I have repeatedly noted the like unsymmetrical characteristics in the brachycephalic crania of Scottish barrows, and it has occurred to my mind, on more than one occasion, whether such may not furnish an indication of some partial compression, dependent, it may be,

* *Edinburgh Philosophical Journal*, n.s., vol. vii. p. 24. *Canadian Journal*, vol. ii. p. 406.

† *Crania Americana*, p. 115. *Types of Mankind*, p. 444.

on the mode of nurture in infancy, having tended, in their case also, if not to produce, to exaggerate the short longitudinal diameter, which constitutes one of their most remarkable characteristics.

- The idea thus expressed, in a paper read before the American Scientific Association at Montreal, as well as at the Dublin meeting of the British Association in 1857, was the result of observations made before leaving Scotland in 1853. One chapter of the "*Prehistoric Annals of Scotland*" is devoted to a discussion as to the ethnological significance of the crania of Scottish tumuli; and after its publication I availed myself of every favourable opportunity for adding to the rare materials illustrative of that interesting department. In pursuing such researches my attention was repeatedly drawn to the unsymmetrical proportions of ancient brachycephalic skulls, and to their peculiar truncated form, accompanied, as in the mound skull of the Scioto Valley, by an abrupt flattening of the occiput, which I soon began to suspect was due to artificial causes. Since then the facilities derived from repeated examinations of American collections have familiarised me, not only with the extreme varieties of form of which the human head is susceptible under the influence of artificial compression, but also with the less marked changes undesignedly resulting from such seemingly slight causes as the constant pressure of the Indian cradle-board. The examination and measurement of several hundred specimens of American crania, as well as of the living head in representatives of various Indian tribes, have also satisfied me not only of the existence of dolichocephalic and brachycephalic heads as tribal or national characteristics, but of the common occurrence of the same exaggerated brachycephalic form, accompanied with the vertical or obliquely flattened occiput, which had seemed to be characteristic of the crania of the Scottish tumuli. There are indeed ethnical differences apparent, as in the frontal and malar bones, but so far as the posterior region of the head is concerned, both appear to exhibit the same undesigned deformation resulting from the process of nursing still practised among many Indian tribes.

The light thus thrown on the habits of the British

mother of prehistoric times, by the skull-forms found in ancient barrows, is replete with interest, from the suggestions it furnishes of ancient customs hitherto undreamt of. But it has also another and higher value to the craniologist, from its thus showing that some, at least, of the peculiar forms hitherto accepted as ethnical distinctions, may be more correctly traced to causes operating after birth.

The first example of this peculiar cranial conformation which attracted my attention, as possibly traceable to other causes than the inherited characteristics, or natural deviations from the typical skull-form of an extinct race, occurred on the opening of a stone cist at Juniper Green, near Edinburgh, on the 17th of May 1851. Soon after the publication of the "*Prehistoric Annals of Scotland*," in which the special characteristics of the crania of the Scottish tumuli were first discussed, I heard of the accidental discovery of an ancient tomb in a garden on the Lanark road, a few miles to the north-west of Edinburgh, and immediately proceeded to the spot. The cist occupied a slightly elevated site, distant only a few yards from the road; and as this had long been under cultivation as a garden, if any mound originally marked the spot it had disappeared, and no external indication distinguished it as a place of sepulture. A shallow cist formed of unhewn slabs of sandstone enclosed a space measuring three feet eleven inches in length, by two feet one inch in breadth at the head, and one foot eleven inches at foot. The joints fitted to each other with sufficient regularity to admit of their being closed by a few stone chips inserted at the junction, after which they appeared to have been carefully cemented with wet loam or clay. The slab which covered the whole projected over the sides, so as effectually to protect the sepulchral chamber from any infiltration of earth. It lay in a sandy soil, within little more than two feet of the surface; but it had probably been covered until a comparatively recent period by a greater depth of earth, as its site was higher than the surrounding surface, and possibly thus marked the traces of the nearly levelled tumulus. Slight as this elevation was it had proved sufficient to prevent the lodgment of water, and hence the cist was found perfectly free from damp. With-

in this a male skeleton lay on its left side. The arms appeared to have been folded over the breast, and the knees drawn up so as to touch the elbows. The head had been supported by a flat water-worn stone for its pillow; but from this it had fallen to the bottom of the cist, on its being detached by the decomposition of the fleshy ligatures; and, as is common in crania discovered under similar circumstances, it had completely decayed at the part in contact with the ground. A portion of the left side is thus wanting; but with this exception the skull was not only nearly perfect when found, but the bones were solid and heavy; and the whole skeleton appeared to me so well preserved as to have admitted of articulation. Above the right shoulder, a neat earthen vase had been placed, probably with food or drink. It contained only a little sand and black dust when recovered, uninjured, from the spot where it had been deposited by affectionate hands many centuries before, and is now preserved along with the skull in the Scottish Museum of Antiquities.

As the peculiar forms of certain skulls, such as are described by Dr Thurnam, from an Anglo-Saxon cemetery at Stone, in Buckinghamshire,* and another, described by me, from an Indian cemetery at Montreal,† as well as those of numerous distorted crania, from the Roman site of Uriconium and other ancient cemeteries, have been ascribed to posthumous compression, the precise circumstances attendant on the discovery of the Juniper Green cist are important, from the proof they afford that the body originally deposited within it had lain there undisturbed and entirely unaffected by any superincumbent pressure from the day of its interment. Two, if not three, classes of skulls have been recovered from early British graves. One with a predominant longitudinal diameter, in the most marked examples differs so essentially in its elongated and narrow forehead and occiput from the modern dolichocephalic head, that I was led to assign it to a separate class under the title kumbecephalic.‡ Another has the longitudinal diameter little

* *Crania Britannica*, Dec. i. p. 88.

† *Edin. Phil. Journal*, n. s., vol. xvi. p. 269.

‡ *Prehistoric Annals of Scotland*, p. 177.

in excess of the greatest parietal breadth, and is no less strikingly distinguished from the prevailing modern head, whether of Celtic or Saxon areas, by its shortness, than the other is by its length, when viewed either in profile or vertically. The Anglo-Saxon type of skull appears to be intermediate between those two forms, with a more symmetrical oval, such as is of common occurrence in modern English heads.

If cranial conformation has any ethnical significance, it appears to me inconceivable that the two extreme forms above referred to can both pertain to the same race; and the circumstances under which the most characteristic examples of the opposite types have been found, confirm me in the belief which I advocated when the evidence was much less conclusive, that the older dolichocephalic or kumbecephalic skull illustrates the physical characteristics of a race which preceded the advent of the Celtæ in Britain, and gradually disappeared before their aggressions. As, however, the opposite opinion is maintained by so high an authority as Dr J. Barnard Davis, the comparison of the following measurements, illustrative of the three types of head, will best exhibit the amount of deviation in opposite directions from the intermediate form. The measurements are taken from those furnished in the "*Crania Britannica*," and include the *longitudinal diameter, frontal, parietal, and occipital breadth, parietal height, and horizontal circumference*. No. 1, like the majority of the same class, is derived from a megalithic chambered barrow, and has been selected by Dr Davis as a characteristic example of the class to which it belongs;* though, according to him, that is one of aberrant deviations from the typical British form. No. 2, obtained from a barrow at Codford, in Wiltshire, has also been selected by Dr Davis as one of three typical British crania. It is of the same type as the Juniper Green skull, and its strongly marked characteristics are thus defined by him: "Its most interesting peculiarities are its small size, and its decidedly brachycephalic conformation. This latter character, which commonly appertains to the ancient British cranium, and even to that form which we regard as typical,

* Proceedings of the Acad. Nat. Sciences, Philadelphia, 1857, p. 42.



is seldom met with expressed in so marked a manner.* No. 3, is a skull from an Anglo-Saxon cemetery near Litlington, Sussex, one of two of which Dr Davis remarks,—“There is a general indication of good form in these fine capacious skulls, which is apparent in every aspect. . . . On a review of the whole series of Anglo-Saxon crania which have come under our notice, we are led to conclude that this pleasing oval, rather dolichocephalic form, may best be deserving the epithet of typical among them.”† All the three examples are male skulls:—

	L. D.	F. B.	P. B.	O. B.	P. H.	H. C.
1. Uley Chambered Barrow Skull....	8·1	4 7	5 7	5·	5·1	21·7
2. Codford Skull.....	6·8	4 6	5 7	5·1	4·7	20·
3. Litlington Skull.....	7 5	4 7	5·3	4·6	4 9	20·9

Each of the above examples presents the features of the type to which it belongs with more than usual prominence, so that if the mean of a large series were taken, the elements of difference between the three would be less strongly defined. The differences are, however, those on which their separate classification depends, and they thus illustrate the special points on which any craniological comparison for ethnological purposes must be based. Of the three skulls, the era and race of one of them (No. 3) are well determined. It is that of a Saxon, probably of the seventh or eighth century, of the race of the South Saxons, descended from Ælla and his followers, and recovered in a district where the permanency of the same ethnic type is illustrated by its predominance among the rural population at the present day. Another of the selected examples (No. 2) is assumed by Dr Davis, probably on satisfactory grounds, to be an ancient British, *i.e.*, Celtic skull. It is indeed a difficulty, which has still to be satisfactorily explained, how it is, that if this brachycephalic type be the true British head-form, no such prevalence of the form is to be found, on modern Celtic areas, as in the case of Saxon

* *Crania Britannica*, Dec. ii. pl. 14.

† *Ibid.* Dec. iv. pls. 39, 40.

Sussex connects the race of its ancient pagan and Christian cemeteries with the Anglo-Saxon population of the present day. The historical race and era with which Dr Davis conceives the barrow-builders of Wiltshire to be connected is thus indicated in the "*Crania Britannica*," "Region of the Belgæ, Temp. Ptolemæi, A.D. 120." The Belgæ of that era—apparently then comparatively recent intruders, and by some regarded as not Celtic, but Germanic—were displaced, if not exterminated; but the modern Britons of Wales are undoubted descendants of British Celts of Ptolemy's age. Though mingling both Saxon and Norman with pure British blood, they probably preserve the native type as little modified by such foreign admixture as that of its supplanters in the most thoroughly Saxon or English districts of England. It is therefore a question of some importance, how far the extreme brachycephalic proportions of the so-called British type may be traceable to other than inherited ethnical characteristics. Meanwhile, turning from this supposed British skull of Roman times to the one derived from Uley chambered barrow, No. 1, the most ancient of the series, and assuming their chronological order to be undisputed, as it appears to be, we find no gradation from an abbreviated to an elongated form, but, on the contrary, an extreme brachycephalic type interposed between the ovoid dolichocephalic Anglo-Saxon of the Christian era and the extreme dolichocephalic, or kumbecephalic one belonging to a period seemingly so remote, that Dr Thurnam, when noting the recurrence of the same type in another chambered barrow at Littleton Drew, Wiltshire, remarked, "It is not necessary to admit the existence of any pre-Celtic race, as the skulls described may be those of Gaelic, as distinguished from Cymric, Celts; or the long-headed builders of these long, chambered, stone barrows, may have been an intrusive people, who entered Britain from the south-west. Can they have been some ancient Iberian or Ibero-Phœnician settlers?"*

Among the rarer crania of the Morton collection is one to which a peculiar interest attaches, and which may possibly have some significance in reference to this inquiry. Its history is narrated in Dr Henry S. Paterson's

* *Crania Britannica*, Dec. iii. pl. 24 (4).

Memoir of Dr Morton. During a visit of Mr Gliddon to Paris in 1856, he presented a copy of the "*Crania Ægyptiaca*" to the celebrated oriental scholar M. Fresnel, and excited his interest in the labours of its author. Upwards of a year after he received at Philadelphia a box containing a skull, forwarded from Naples, but without any information relative to it. "It was handed over to Morton," says Dr Paterson, "who at once perceived its dissimilarity to any in his possession. It was evidently very old, the animal matter having almost entirely disappeared. Day after day would Morton be found absorbed in its contemplation. At last he announced his conclusion. He had never seen a Phœnician skull, and he had no idea where this one came from, but it was what he conceived a Phœnician skull should be, and it could be no other."* Six months afterwards, Mr Gliddon received, along with other letters and papers forwarded to him from Naples, a slip of paper, in the handwriting of M. Fresnel, containing the history of the skull, which had been discovered by him during his exploration of an ancient rock-tomb at Malta. Dr Meigs refers to this in his catalogue of the collection, No. 1352, as an illustration of "the wonderful power of discrimination, the *tactus visus* acquired by Dr Morton in his long and critical study of craniology." Such was my own impression on first reading it; but I confess the longer I reflect on it, the more am I puzzled to guess by what classical or other data, or process short of absolute intuition, the ideal type of the Phœnician head could be determined. I suspect, therefore, if we had the statement in Dr Morton's own words, it would fall short of such an absolute craniological induction. The following is the sole entry made by him in his catalogue: "Ancient Phœnician? I received this highly interesting relic from M. F. Fresnel, the distinguished French archæologist and traveller, with the following memorandum, A.D. 1847:—Crâne provenant des caves sépulchrales de Ben-Djemma, dans l'île de Malte. Ce crâne paraît avoir appartenu à un individu de la race qui, dans les temps les plus anciens, occupait la côté septentrionale de l'Afrique, et les îles adjacentes." The skull is not im-

* Memoir of Dr S. G. Morton; *Types of Mankind*, pl. xl.

probably what it has been assumed to be, and it is in many respects a remarkable one. A deep indentation at the nasal suture gives the idea of an overhanging forehead; but the superciliary ridges are not prominent, and the peculiar character of the frontal bone is most strikingly apparent in the vertical view, where it is seen to retreat on either side almost in a straight line from the centre of the glabella to the external angular process of the frontal bone. The contour of the coronal region is described by Dr Meigs as "a long oval, which recalls to mind the kumbecephalic form of Wilson."* Whatever, indeed, be the ultimate conclusion of ethnologists as to the evidences which led me to adopt that name to distinguish the characteristics of a pre-Celtic British race, the necessity appears to be acknowledged for some such term to distinguish the form in question from the ordinary dolichocephalic type. The head is narrow throughout, with its greatest breadth a little behind the coronal suture, from whence it narrows gradually towards front and rear. The lower jaw is large and massive, but with less of the prognathous development than in the superior maxillary. The skull is, no doubt, that of a woman. The nose has been prominent; but the zygomatic arches are delicate, and the whole face is long, narrow, and tapering towards the chin. The parietals meet at an angle, with a bulging of the sagittal suture, and a slight but distinctly defined pyramidal form running into the frontal bone. The occiput is full, round, and projecting a little more on the left side than the right. The measurements of the skull are as follows:—

Longitudinal diameter, ...	7·4
Parietal diameter, ...	5·1
Frontal diameter, ...	4·
Vertical diameter, ...	5·3
Intermeatoid arch, ...	12·3
Intermastoid arch, ...	15· (?)
Intermastoid line, ...	4·3 (?)
Occipito-frontal arch, ...	14·2
Horizontal circumference, ...	20·2

* Catalogue of Human Crania in the Academy of Nat. Science of Philadelphia, p. 29.

I have been thus particular in describing this interesting skull, because it furnishes some points of comparison with British kumbecephalic crania, bearing on the inquiry, whether we may not thus recover traces of the Phœnician explorers of the Cassiterides in the long-headed builders of the chambered barrows. When contrasting the wide and nearly virgin area with which Dr Morton had to deal, with that embraced in the scheme of the "*Crania Britannica*," I remarked in 1857:—Compared with such a wide field of investigation, the little island home of the Saxons may well seem narrow ground for exploration; but to the ethnologist it is not so. There, amid the rudest traces of primeval arts, he seeks, and probably not in vain, for the remains of primitive European allophyliæ. There it is not improbable that both Phœnicians and early Greek navigators have left behind them evidences of their presence, such as he alone can discriminate.*

Before, however, we can abandon ourselves to the temptations of so seductive a theory,—which, after all, finds only such support as may be deduced from a certain general analogy of cranial form, and derives no confirmation from the works of art accompanying the remains of the long-headed barrow-builders,—it has to be borne in remembrance that the question is still disputed with reference to this class of British dolichocephalic crania: are they examples of an essentially distinct type, preserving evidence of the characteristics of a different race, or are they mere exceptional aberrant deviations from the supposed brachycephalic Celtic or British type? Much stress is laid on the fact that the two forms of skull have occasionally been recovered from the same barrow; from which it may be inferred that the two races to which I conceive them to have belonged, were for a more or less limited period contemporaneous. More than this I cannot regard as a legitimate induction from such premises, in relation to crania of such extremely diverse types. But this amounts to little; for the same is undoubtedly true of the ancient British and the modern Anglo-Saxon race; and the discovery of Celtic and Saxon

* Canadian Journal, vol. ii. p. 445.

skulls in a common barrow or tumulus of the sixth century is no proof that the latter race was not preceded by many centuries in the occupation of the country by the Britons, among whom they then mingled as conquerors and supplanters. But the elongated skulls are no rare and exceptional forms. They have been most frequently found in tombs of a peculiar character, and of great antiquity. Many have been recovered in too imperfect a state to admit of more being deduced from the fragments than that these conform to the more perfect examples of the peculiar type. Nevertheless the number already obtained in a sufficiently perfect state to admit of detailed measurement is remarkable, when their great age and the circumstances of their recovery are fully considered. Of this the following enumeration will afford satisfactory proof. Only two perfect crania from the chambered tumulus of Uley, in Gloucestershire,—of which the proportions of one are cited above,—have been preserved. But in the later search of Mr Freeman and Dr Thurnam, in 1854, the fragments of eight or nine other skulls were recovered, and of these the latter remarks: "The fragments are interesting, as proving that the characters observed in the more perfect crania were common to the individuals interred in this tumulus. Three or four calvaria are sufficiently complete to show that in them likewise the length of the skulls had been great in proportion to the breadth."* Again, in the megalithic tumulus of Littleton Drew, North Wilts, at least twenty-six skeletons appear to have been found, from several of which imperfect crania were recovered, and of those Dr Thurnam remarks: "Eight or nine crania were sufficiently perfect for comparison. With one exception, in which a lengthened oval form is not marked, they are of the dolichocephalic class."† So also the four nearly perfect skulls from West Kennet are described as "more or less of the lengthened oval form, with the occiput expanded and projecting, and presenting a strong contrast to skulls from the circular barrows of Wilts and Dorset."‡ To these may be added those of Stoney Little-

* *Archæol. Journal*, vol. xi. p. 318. *Crania Britannica*, Dec. i. pl. 5.

† *Crania Britannica*, Dec. iii. pl. 24 (8).

‡ *Ibid.* Dec. v. pl. 50 (4).

ton, Somersetshire, first pointed out by Sir R. C. Hoare;* and examples from barrows in Derby, Stafford, and Yorkshire, described by Mr Thomas Bateman in his "Ten Years' Diggings in Celtic and Saxon Grave Hills;" including those from Bolehill, Longlow, and Ringham Low, Derbyshire; from the galleries of the tumulus on Five-Wells Hill, and from the Yorkshire barrow near Heslerton-on-the-wolds. Several of the above contained a number of skulls; and of the last, in which fifteen human skeletons lay heaped together, Mr Bateman remarks: "The crania that have been preserved are all more or less mutilated; but about six remain sufficiently entire to indicate the prevailing conformation to be of the long or kumbecephalic type of Dr Wilson."† The crania occurring in graves of this class mentioned by Mr Bateman alone, exceed fifty in number, of which the majority are either of the elongated type, or too imperfect to be determined. The others include between thirty and forty well-determined examples, besides a greater number in too imperfect a state to supply more than indications of their correspondence to the same characteristic form. Alongside of some of these are also found brachycephalic crania; but in the most ancient barrows the elongated skull appears to be the predominant, and in some cases the sole type; and of the examples found in Scotland, two have been recovered from peat bogs, and others under circumstances more definitely marking their great antiquity.

The variations of cranial form are thus, it appears, no gradual transition, or partial modification, but an abrupt change from an extreme dolichocephalic to an extreme brachycephalic type; which, on the intrusion of the new and essentially distinct Anglo-Saxon race, gives place once more to a dolichocephalic form of medium proportions.

Leaving, meanwhile, the consideration of the question of distinct races indicated by such evidence, it will be well to determine first if such variations of skull-form can be traced to other than a transmitted ethnical source. The Juniper Green skull, already referred to, presents in profile, as shown in the full-sized view in the "*Crania Britannica*,"

* *Archæologia*, vol. xix. p. 47.

† *Ten Years' Diggings in Celtic and Saxon Grave Hills*, p. 230.

the square and compact proportions characteristic of British brachycephalic crania. It also exhibits, in the vertical outline, the truncated wedge form of the type. In the most strongly marked examples of this form, the vertical or flattened occiput is a prominent feature, accompanied generally with parietal breadth, from which it abruptly narrows at the occiput. The proportions of this class of crania were already familiar to me before the discovery of the Juniper Green example; but it had not before occurred to me to ascribe any of their features to other than natural causes. But the circumstances attending its discovery gave peculiar interest to whatever was characteristic in the skull and its accompanying relics, handled for the first time as evidences of the race and age of the freshly-opened cist, discovered almost within sight of the Scottish capital, and yet pertaining to prehistoric times. The skull was carried home in my hand, a distance of several miles, and its truncated outline, and still more, its flattened occiput, attracted special attention, and gave rise to conversation with my friend Mr Robert Chambers, who had accompanied me on this exploratory excursion. With the temptation of a novel discovery, I was at first disposed to recognise the traces of art in this abbreviated form, not only as exaggerating the natural characteristics, but as a possible source of their production. But a comparison with examples of the true dolichocephalic skull, to which I had already assigned priority in point of time, sufficed to dispel that illusion. At a subsequent meeting of the Society of Antiquaries of Scotland, I accompanied the presentation of the cranium and urn with an account of the circumstances of their discovery, and some remarks on the novel features noticeable in the skull. Unfortunately the printing of the Society's Proceedings, which had been suspended for some time, was not resumed till the following season; and no record of this communication was preserved beyond the title.

Another skull in the same collection, found under somewhat similar circumstances in a cist at Lesmurdie, Banffshire, has the vertical occiput accompanied by an unusual parietal expansion and want of height, suggestive of the

idea of a combined coronal and occipital compression.* A third Scottish skull, procured from one of a group of cists near Kinaldie, Aberdeenshire, also exhibits the posterior vertical flattening. But a more striking example than any of those appears in the one from Codford, South Wiltshire, selected above to illustrate this type.† Dr Davis remarks in his description of it:—"The zygomatic arches are short, a character which appertains to the entire calvarium, but is most concentrated in the parietals, to which the abruptly ascending portion of the occipital lends its influence. The widest part of the calvarium is about an inch behind, and as much above the auditory foramen, and when we view it in front we perceive it gradually to expand from the outer angular process of the frontal to the point now indicated." The entire parieto-occipital region presents in profile an abrupt vertical line; but when viewed vertically it tapers considerably more towards the occiput than is usual in crania of the same class.

The cause of the vertical occiput, as well as the oblique parieto-occipital flattening in this class of British crania, I feel no hesitation in believing to be traceable to the same kind of rigid cradle-board as is in constant use among many of the Indian tribes of America, and which produces precisely similar results. Its mode of operation, in effecting the various forms of oblique and vertical occiputs, will be considered, when describing some of the phenomena of compressed Indian crania; but another feature of the Juniper Green skull, which is even more obvious in that from Lesmurdie, in the same collection, is an irregularity amounting to a marked inequality in the development of the two sides. This occurs in skulls which have been altered by posthumous compression; but the recovery of both the examples referred to from stone cists precludes the idea of their having been affected by the latter cause; and since I was first led to suspect the modification of the occiput, and the exaggeration of the characteristic proportions of British brachycephalic crania by artificial means, familiarity with those of the Flathead Indians, as well as other ancient and modern artificially distorted American crania, has led me

* *Crania Britannica*, Dec. ii. pl. 16.

† *Ibid.* Dec. ii. pl. 14.

to recognise in their parietal conformation the constant occurrence of the same unsymmetrical inequality.

But besides the well-known distortions produced in many American crania by protracted compression purposely applied with a view to change the form, others of a less marked character closely correspond to those of the British brachycephalic crania. The normal human head may be assumed to present a perfect correspondence in its two hemispheres; but very slight investigation will suffice to convince the observer that few living examples satisfy the requirements of such a theoretical standard. Not only is inequality in the two sides frequent, but a perfectly symmetrical head is the exception rather than the rule. The plastic condition of the cranial bones in infancy, which admits of all the strange malformations of ancient Macrocephali and modern Flatheads, also renders the infant head liable to many undesigned changes. From minute personal examination I have satisfied myself of the repeated occurrence of inequality in the two sides of the head, arising from the mother being able to suckle her child only at one breast, so that the head was subjected to a slight but constantly renewed pressure in the same direction. It is surprising, indeed, to how great an extent such unsymmetrical irregularity is found to prevail, when once the attention has been drawn to it. The only example of the Greek head possessed by Dr Morton, was a cast presented to him by Dr Retzius, and which, from its selection by the distinguished Swedish craniologist for such a purpose, might reasonably be assumed to illustrate the Greek type. It is accordingly described by Dr J. Aitken Meigs, in his "*Cranial Characteristics of the Race of Man*," as very much resembling that of Constantine Demetriades, a Greek native of Corfu, and long a teacher of the modern Greek language at Oxford, as engraved in Dr Prichard's *Researches*. Its cranial characteristics are thus defined in the Catalogue of the Mortonian Collection (No. 1354): "The calvarial region is well developed, the frontal line expansive and prominent, the facial line departs but slightly from the perpendicular." On recently visiting Philadelphia for the purpose of renewed examination of its valuable collections, I was surprised to

find this head, instead of being either oval, or, as Blumenbach describes the example selected by him, subglobular: presenting the truncated form, with extreme breadth at the parietal protuberances, and then abruptly passing to a flattened occiput. It measures 6·5 longitudinal diameter; 5·7 parietal diameter; and 19·2 horizontal circumference. But the most noticeable feature is the great inequality of the two sides, the right side is less tumid than the left, while it projects more to the rear, and the whole is fully as unsymmetrical as many American crania. Were it not that this feature appears to have wholly escaped Dr Morton's attention, as he merely enters it in his catalogue as a "Cast of the skull of a young Greek. Professor Retzius," I should be tempted to suppose it had been purposely sent to him to illustrate the phenomena of unsymmetrical development, and of the influence of undesigned artificial causes on skull-forms. Nor was Dr Morton unobservant of such indications. When first noticing the probable origin of the flattened occiput of certain British skulls, I drew attention to the fact, that he had already recognised undesigned artificial compression as one source of abnormal cranial conformation, and accompanied its demonstration with a reference to the predominant unsymmetrical form in all such skulls. "This irregularity," he added, "chiefly consists in the greater projection of the occiput to one side than the other," and "is not to be attributed to the intentional application of mechanical force." Such want of uniformity in the two sides of the head is much more strongly marked in Flathead skulls, which have been subjected to great compression. It is clearly traceable to the difficulty of subjecting the living and growing head to a perfectly uniform and equable pressure, and to the cerebral mass forcing the skull to expand with it in the direction of least resistance. Hence the unsymmetrical form accompanying the vertical occiput in the Lesmurdie and Juniper Green skulls, and, as I conceive also, in the Greek skull of Retzius. The study of the latter skull-form has tended strongly to confirm me in the belief, that the extreme abbreviated proportions of many naturally brachycephalic crania are due to artificial causes. Wherever a very noticeable inequality exists between the two sides,

it may be ascribed with much probability to the indirect results of designed or accidental compression in infancy ; and by its frequent occurrence in any uniform aspect, may, quite as much as the flattened occiput, furnish a clue to customs or modes of nurture among the people to whom it pertains.

Dr Struthers of Edinburgh has in his collection an interesting example of a modern skull, measuring 7·5 longitudinal diameter, 6·5 parietal diameter, 21·4 horizontal circumference, in which the truncated form is even more strongly marked by the abrupt flattening, immediately behind the parietal protuberances, accompanied with inequality in the two sides of the head. It was obtained from a grave-digger in Dundee, who stated it to be that of a middle-aged female, whom he had known during life. There was nothing particular about her mental development.

I have also drawn attention in former papers to the fact that such peculiar forms and examples of inequality in the development of the two sides of the head are familiar to hat-manufacturers. Occasionally the eye is attracted by very unusual cranial forms revealed by baldness ; but the hair suffices generally to conceal abnormal irregularities, some of which, as illustrated by hatters' shapes, are extremely odd and fantastical. My attention was directed to this familiar test by a remark of the late Dr Kombst, that he had never been able to obtain an English-made hat that would fit his head. He added, that he believed such was the general experience of Germans, owing to the greater length of the English head. I subsequently found the shapes of a Yorkshire hatter to be shorter than some furnished me from Dublin ; while English and Anglo-Canadian ones are decidedly longer than those of the French Canadians of Lower Canada ; and I believe that comparisons of the shapes most in demand in different parts of the British Islands, and on the Continent, will supply important craniological results.

The novel forms thus occurring in modern heads, though chiefly traceable, as I believe, to artificial causes, are not the result of design. But the same is true of the prevalent vertical and obliquely flattened occiput of many ancient and

modern American crania, as well as of the British brachycephalic class already described. Nor are such changes of the natural form necessarily limited to skulls of short longitudinal diameter, in which this typical characteristic is exaggerated by the pressure of the cradle-board in infancy. Now that this source of modification begins to receive general recognition among craniologists, its influence is assumed as a probable source of the most diverse aberrant forms. Dr Thurnam, when referring to two skulls of different shapes, recovered from the same group of British barrows, of "a somewhat late though pre-Roman period," on Roundway Hill, North Wiltshire, thus indicates their contrasting characteristics, and suggests the probable source of such divergence from the supposed British type: "The general form of the cranium (plate 43) differs greatly from that from the adjoining barrow (plate 42). That approaches an acrocephalic, this a platycephalic form; that is eminently brachycephalic, this more nearly of a dolichocephalic character. As the eye at once detects, the difference is much greater than would be inferred from a mere comparison of the measurements. The respective peculiarities of form in the two skulls may possibly be explained by supposing that both have been subject to artificial deformation, though of a different kind,—the one appearing to have been flattened on the occiput, the other showing a depression immediately behind the coronal suture over the parietal bones, which seems to indicate that this part of the skull was subject to some habitual pressure and constriction, perhaps from the use of a bandage or ligature tightly bound across the head and tied under the chin, such as to this day is employed in certain parts of the west of France, producing that form of distortion named by Dr Gosse the *sincipital*, or *tête bilobée*."*

The influence of the recognition of this source of change is, indeed, very manifest throughout the fifth Decade of the "Crania Britannica." An extremely brachycephalic skull of a youth, obtained from a barrow on Ballard Down, Isle of Purbeck, is described as unsymmetrical, and as affording "tolerably clear evidence that this form, if not always produced, was at least liable to be exaggerated by an artificial

* *Crania Britannica*, Dec. v. pl. 48.

flattening of the occiput, such as is practised by many American and Polynesian tribes."* In the same Decade another skull of the type most dissimilar to this is described and illustrated. It was recovered in fragments from the remarkable chambered barrow at West Kennet, Wiltshire, and its most characteristic features are thus defined by Dr Thurnam:—"It is decidedly dolichocephalic, narrow, and very flat at the sides, and realises more nearly than any we have yet had to figure the kumbecephalic or boat-shaped form described by Dr D. Wilson. The frontal region is narrow, moderately arched, and elevated at the vertex, but slopes away on each side. The parietal region is long, and marked by a prominent ridge or *carina* in the line of the sagittal suture, which is far advanced towards obliteration, whilst the other sutures are quite as perfect as usual. The occiput is full and prominent; the supra-occipital ridges only moderately marked. There is a deep digastric groove, and a slight paroccipital process on each side. The external auditory openings are somewhat behind the middle of the skull, and very much behind a vertical line drawn from the junction of the coronal and sagittal sutures." Its extreme length and breadth are 7·7 and 5·1, and an inequality in the development of the two sides is obvious in the vertical view. As the brachycephalic skull recalls certain American and Polynesian forms, so such examples of the opposite type suggest the narrow and elongated skulls of the Australians and Esquimaux; and he thus proceeds:—"The Ballard Down skull bears marks of artificial flattening of the occiput; this calls to mind the artificial lateral flattening of the skull, characteristic of the ancient people called Macrocephali, or long-heads, of whom Hippocrates tells us, that 'while the head of the child is still tender, they fashion it with their hands, and constrain it to assume a lengthened shape by applying bandages and other suitable contrivances, whereby the spherical form of the head is destroyed, and it is made to increase in length.' This mode of distortion is called by Dr Gosse the *temporo-parietal*, or '*tête aplatie sur les côtés*.' It appears to have been practised by various people, both of the ancient and modern world, and in Europe as well as the

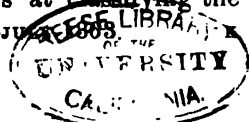
* Crania Britannica, Dec. v. pl. 45.

East. The so-called Moors, or Arabs of North Africa, affected this form of skull; and even in modern times, the women of Belgium and Hamburg are both described as compressing the heads of their infants into an elongate form. Our own observations lead at least to a presumption that this form of artificial distortion may have been practised by certain primeval British tribes, particularly those who buried their distinguished dead in long chambered tumuli." Accordingly, Dr Thurnam draws attention to the obliteration of the sagittal suture, both in the skull in question, and to a still greater extent in one figured by Blumenbach, under the name of "*Asiatic Macrocephali*," and expresses his belief that this "has been produced by pressure or manipulations of the sides of the head in infancy, by which it was sought to favour the development of a lengthened form of skull; to which, however, there was probably, in the present instance, at least, a natural and inherent tendency."

It thus appears that a class of variations of the form of the human skull, which becomes more comprehensive as attention is directed to it, is wholly independent of congenital transmitted characteristics: kumbecephalic, acrocephalic, and platycephalic, unsymmetrical, truncated, or elongated heads, may be so common as apparently to furnish distinctive ethnical forms, and yet, after all, each may be traceable to artificial causes, arising from an adherence to certain customs and usages in the nursery. It is in this direction, I conceive, that the importance of the truths resulting from the recognition of artificial causes affecting the forms of British brachycephalic or other crania chiefly lies. The contents of early British cists and barrows prove that the race with whom they originated was a rude people, ignorant, for the most part, of the very knowledge of metals, or at best in the earliest rudimentary stage of metallurgic arts. They were, in fact, in as uncivilised a condition as the rudest forest Indians of America. To prove, therefore, that, like the Red Indian squaw, the British allophylian or Celtic mother formed the cradle for her babe of a flat board, to which she bound it, for safety and facility of nursing, in the vicissitudes of her nomade life,—though interesting, like every other recovered glimpse of a long-forgotten past,—

is not in itself a discovery of much significance. But it reminds us how essentially man, even in the most degraded state of wandering savage life, differs from all other animals. The germs of an artificial life are there. External appliances, and the conditions which we designate as domestication in the lower animals, appear to be inseparable from him. The most untutored nomades subject their offspring to many artificial influences, such as have no analogy among the marvellous instinctive operations of the lower animals. It is not even unworthy of notice that man is the only animal to whom a supine position is natural for repose; and with him more than any other animal, when recumbent, the head is necessarily placed so as to throw the greatest pressure on the brain-case, and not on the malar or maxillary bones. Without, therefore, running to the extreme of Dr Morton, who denied, for the American Continent, at least, the existence of any true dolichocephalic crania, or, indeed, any essential variation from one assumed typical form, it becomes an important point for the craniologist to determine, if possible, to what extent certain characteristic diversities may be relied upon as the inherited features of a tribe or race; or whether they are not the mere result of artificial causes originating in long perpetuated national customs and nursery usages. If the latter is, indeed, the case, then they pertain to the materials of archæological rather than of ethnological deduction, and can no longer be employed as elements of ethnical classification.

Every scheme of the craniologist for systematising ethnical variations of cranial configuration, and every process of induction pursued by the ethnologist from such data, proceed on the assumption that such varieties in the form of cranium are constant within certain determinate limits, and originate in like natural causes with the features by which we distinguish one nation from another. By like means the comparative anatomist discriminates between the remains of the *Bos primigenius*, the *Bos longifrons*, and other kindred animal remains, frequently found alongside of the human skeleton, in the barrow; and, by a similar crucial comparison, the craniologist aims at classifying the crania



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of the ancient Briton, Roman, Saxon, and Scandinavian, apart from any aid derived from the evidence of accompanying works of art. But if it be no longer disputable that the human head is liable to modification from external causes, so that one skull may have been subjected to lateral compression, resulting in the elongation and narrowing of its form; while another, under the influence of occipital pressure, may exhibit a consequent abbreviation in its length, accompanied by parietal expansion; it becomes indispensable to determine some data whereby to eliminate this perturbing element before we can ascertain the actual significance of national skull-forms. If, for example,—as appears to be the case,—the crania from British graves of Roman times reveal a different form from that of the modern Celtic Briton, the cause may be an intermixture of races, like that which is clearly traceable among the mingled descendants of Celtic and Scandinavian blood in the north of Scotland; but it may also be, in part or wholly, the mere result of a change of national customs following naturally on conquest, civilisation, and the abandonment of Paganism for Christianity.

It is in this respect that the artificial causes tending to alter the natural conformation of the human head invite our special study. They appear at present purely as disturbing elements in the application of craniological tests of classification. It is far from improbable, however, that, when fully understood, they may greatly extend our means of classification, so that when we have traced to such causes certain changes in form, in which modern races are known to differ from their ethnical precursors, we shall be able to turn the present element of disturbance to account, as an additional confirmation of truths established by inductive craniology. Certain it is, however, whatever value may attach to the systematising of such artificial forms, that they are of frequent occurrence, apart altogether from such configuration as is clearly referrible to the application of mechanical pressure in infancy with that express object in view; or again, as is no less obviously the result of post-humous compression. But though the deforming processes designedly practised among ancient and modern savage

nations lie beyond the direct aim of the present inquiry, they are calculated to throw important light on the approximate results of undesigned compression and arrested development.

Among the flathead Indian tribes of Oregon and the Columbia River, where malformation of the skull is purposely aimed at, the infant's head is tightly bound in a fixed position, and maintained under continuous pressure for months. But it is a mistake to suppose that in the ordinary use of the cradle-board the Indian pappoose is subject to any such extreme restraint. The objects in view are facility of nursing and transport, and perfect safety for the child. But those being secured, it is nurtured with a tenderness of maternal instinct surpassing that of many savage nations. The infant is invariably laid on its back, but the head rests on a pillow or mat of moss or frayed bark, and is not further restrained in a fixed position than necessarily results from the posture in which the body is retained by the bandages securing it in the cradle. This fact I have satisfied myself of from repeated observations. But the consequence necessarily is, that the soft and pliant bones of the infant's head are subjected to a slight but constant pressure on the occiput during the whole protracted period of nursing, when they are peculiarly sensitive to external influences. Experiments have shown that at that period the bones specially affected by the action of the cradle-board are not only susceptible of changes, but liable to morbid affections dependent on the nature of the infant's food. Lehmann supposes the *Craniotabes* of *Elsässer* to be a form of *rachitis*, which affects the occipital and parietal bones during the period of suckling; and *Schlossberger* ascertained by a series of analyses of such bones, that the 63 per cent. of mineral constituents found in the normal occipital bones of healthy children during the first year diminished to 51 per cent. in the thickened and spongy bone.* The fluctuations in proportion of the mineral constituents of bones are considerable, and vary in the different bones, but in the osseous tissue they may be stated at from

* *Schlossberger*, *Arch. l. phys. Heilk.* *Lehmann*, *Physiol. Chem.*, vol. iii. p. 28.

67 to 70 per cent. It is obvious, therefore, that under the peculiar physiological condition of the cranial bones during the period of nursing, such constant mechanical action as the occiput of the Indian pappoose is subjected to must be productive of permanent change. The child is not removed from the cradle-board when sucking, and is not therefore liable to any counteracting lateral pressure against its mother's breast. One effect of such continuous pressure must be to bring the edges of the bones together, and thereby to retard or arrest the growth of the bone in certain directions. The result of this is apparent in the premature ossification of the sutures of artificially deformed crania.

At Washington I had an opportunity of minutely examining thirty-four Flathead skulls brought home by the United States Exploring Expedition, some of them presenting the most diverse forms of distortion. In the majority of those, the premature ossification of the sutures is apparent, and in some they are almost entirely obliterated. The same is no less obvious among the corresponding class in the collection of the Academy of Natural Sciences of Philadelphia, and especially in skulls of the Chinooks, who carry the process of deformation to the greatest extent. But I have also been struck, not only with the frequent occurrence of wormian bones in such altered skulls, but also with the distinct definition of a true supraoccipital bone.

It is marvellous to see the extraordinary amount of distortion to which the skull and brain may be subjected without seemingly affecting either health or intellect. The coveted deformity is produced partly by actual compression, and partly by the growth of the brain and skull being thereby limited to certain directions. Hales, the ethnographer of the exploring expedition, after describing the process as practised among the Chinooks, remarks: "The appearance of the child when just released from this confinement is truly hideous. The transverse diameter of the head above the ears is nearly twice as great as the longitudinal from the forehead to the occiput. The eyes, which are naturally deep set, become protruding, and appear as if squeezed partially out of the head."* Mr Paul Kane, in

* *Ethnography of the U. S. Exploring Expedition*, p. 216.

describing to me the same appearance, as witnessed by him on the Columbia River, compared the eyes to those of a mouse strangled in a trap. The appearance is little less singular for some time after the child has been freed from the constricting bandages, as shown in an engraving from one of Mr Kane's sketches of a Chinook child seen by him at Fort Astoria.* In after years, the brain, as it increases, partially recovers its shape; and in some of the deformed adult skulls, one suture gapes while all the rest are ossified, and occasionally a fracture or false suture remains open. An adult skull, of the same extremely deformed shape, among those brought home by the Exploring Expedition, illustrates the great extent to which the brain may be subjected to compression and malformation without affecting the intellect. It is that of a Nasqually chief, procured from his canoe-bier in Washington Territory. (No. 4549.) The internal capacity, and consequent volume of brain, is 95 cubic inches. The head is compressed into a flattened disc, with the forehead receding in a straight line from the nasal suture to the crown of the head, while the lambdoidal suture is on the same plane with the *foramen magnum*. The sutures are nearly all completely ossified, and the teeth ground quite flat, as is common with many of the tribes in the same region, and especially with the Wallawalla Indians on the Columbia River, who live chiefly on salmon dried in the sun, and invariably impregnated with the sand which abounds in the barren waste they occupy. I assume the unimpaired intellect of the Nasqually chief from his rank. The Flathead tribes are in the constant habit of making slaves of the round-headed Indians; but no slave is allowed to flatten or otherwise modify the form of her child's head, that being the badge of Flathead aristocracy. As this has been systematically pursued since ever the tribes of the Pacific coast were brought under the notice of Europeans, it is obvious that if such superinduced deformity developed any general tendency to cerebral disease, or materially affected the intellect, the result would be apparent in the degeneracy or extirpation of the Flathead tribes. But so far is this from being the case, that

* Prehistoric Man, vol. ii. p. 820.

they are described by traders and voyagers as acute and intelligent. They are, moreover, an object of dread to neighbouring tribes who retain the normal form of head, and look on the latter with contempt, as thus bearing the hereditary badge of slaves.

The child born to such strange honours is laid, soon after its birth, upon the cradle-board, an oblong piece of wood, sometimes slightly hollowed, and with a cross-board projecting beyond the head to protect it from injury. A small pad of leather, stuffed with moss or frayed cedar bark, is placed on the forehead and tightly fastened on either side to the board, and this is rarely loosed until its final removal before the end of the first year. The skull has then received a form which is only slightly modified during the subsequent growth of the brain. But the very same kind of cradle is in use among all the Indian tribes. It is indeed varied as to its ornamental adjuncts and non-essential details; but practically it resolves itself, in every case, into a straight board to which the infant is bound; and as it is retained in a recumbent position, the pressure of its own weight, during the period when, as has been shown, the occipital and parietal bones are peculiarly soft and compressible, is made to act constantly in one direction. This I assume to have been the cause of the vertical or otherwise flattened occiput in the ancient British brachycephalic crania. The same cause must tend to increase the characteristic shortness in the longitudinal diameter, and to shorten the zygoma, with probably also some tendency to make the arch bulge out in its effort at subsequent full growth, and so to widen the face.

Dr J. Barnard Davis has applied the term "parieto-occipital flatness," where the results of artificial compression in certain British skulls extend over the parietals with the upper portion of the occipital; and he appears to regard this as something essentially distinct from the vertical occiput. But it is a form of common occurrence in Indian skulls, and is in reality the most inartificial of all the results of the undesigned pressure of the cradle-board. This will be understood by a very simple experiment. If the observer lie down on the floor, without a pillow, and then ascertain

what part of the back of the head touches the ground, he will find that it is the portion of the occiput immediately above the lambdoidal suture, and not the occipital bone. When the Indian mother places a sufficiently high pillow for her infant, the tendency of the constant pressure will be to produce the vertical occiput; but where, as is more frequently the case, the board has a mere cover of moss or soft leather, then the result will be just such an oblique parietal flattening as is shown on a British skull from the remarkable tumulus near Littleton Drew, Wiltshire, figured in the "*Crania Britannica*," Decade iii., plate 24.

But there are other sources of modification of the human skull in infancy, even more common than the cradle-board. More than one of the predominant head-forms in Normandy and Belgium are now traced to artificial changes, and by many apparently trifling and unheeded causes, consequent on national customs, nursing usages, or the caprices of dress and fashion, the form of the head may be modified in the nursery. The constant laying of the infant to rest on its side, the pressure in the same direction in nursing it, along with the fashion of cap, hat, or wrappage, may all influence the shape of head among civilised nations, and in certain cases tend as much to exaggerate the naturally dolichocephalic skull, as the Indian cradle-board increases the short diameter of the opposite type. Such artificial cranial forms as that designated by M. Foville the *Tête annulaire*, may have predominated for many centuries throughout certain rural districts of France, solely from the unreasoning conformity with which the rustic nurse adhered to the traditional or prescriptive bandages to which he ascribes that distortion. All experience shows that such usages are among the least eradicable, and long survive the shock of revolutions that change dynasties and efface more important national characteristics.

But now that attention has been directed to the subject of undesigned changes thus effected on the human head, its full bearings begin to be appreciated; and there is even, perhaps, a danger that more may be ascribed to them than is legitimate. Such was undoubtedly the effect on Dr Morton's mind from his familiarity with the results of artificial de-

formation on American crania ; and were we to follow his example, we should be tempted to designate all the extreme varieties of the elongated dolichocephalic, acrocephalic, and brachycephalic skulls of British barrows, as mere modifications of the same ethnical form. In his latest recorded opinions, when commenting on some of the abnormal forms of Peruvian crania, he remarks : " I at first found it difficult to conceive that the original rounded skull of the Indian could be changed into this fantastic form, and was led to suppose that the latter was an artificial elongation of a head remarkable for its length and narrowness. I even supposed that the long-headed Peruvians were a more ancient people than the Inca tribes, and distinguished from them by their cranial configuration. In this opinion I was mistaken. Abundant means of observation and comparison have since convinced me that all these variously-formed heads were originally of the same shape, which is characteristic of the aboriginal race from Cape Horn to Canada, and that art alone has caused the diversities among them."* The repeated opportunities I have enjoyed of examining the Mortonian and other American collections, have satisfied me of the occurrence of both dolichocephalic and brachycephalic crania, not only as the characteristics of distinct tribes, but also among the contents of the same Peruvian cemeteries,—not as examples of extreme latitudes of form in a common race, but as the results of the admixture either of conquering and subject races, or of distinct classes of nobles and serfs, most generally resulting from the predominance of conquerors.† Among the Peruvians the elongated cranium pertained to the dominant race, and some of the results of later researches in primitive British cemeteries, and especially the disclosures of the remarkable class of chambered barrows, seem to point to an analogous condition of races. That the Uley and West Kennet skulls may have been laterally compressed, while the Codford barrow and other brachycephalic skulls have been affected in the opposite direction, appears equally probable. But such artificial influences only very partially account for the great diversity

* Physical Type of the American Indian. Schoolcraft, p. 326.

† Prehistoric Man, vol. ii. p. 225.

of type; and no such causes, even if brought to bear in infancy, could possibly convert the one into the other form.

But as the cranial forms, both of the Old and New World, betray evidences of modification by such artificial means; so also we find in ancient Africa a diverse form of head, to which art may have contributed, solely by leaving it more than usually free from all extraneous influences. Such at least is the conclusion suggested to my mind from the examination of a considerable number of Egyptian skulls. Among familiar relics of domestic usages of the ancient Egyptians is the pillow designed for the neck, and not the head, to rest upon. Such pillows are found of miniature sizes, indicating that the Egyptian passed from earliest infancy without his head being subjected even to so slight a pressure as the pillow, while he rested recumbent. The Egyptian skull is long, with great breadth and fulness in the posterior region. In its prominent, rounded parieto-occipital conformation, an equally striking contrast is presented to the British brachycephalic skull with truncated occiput, and to the opposite extreme characteristic of the primitive dolichocephalic skull; though exceptional examples are not rare. This characteristic did not escape Dr Morton's observant eye, and is repeatedly indicated in the "*Crania Ægyptiaca*" under the designation "tumid occiput." It also appears to me, after careful examination of the fine collection formed by him, and now in the Academy of Natural Sciences of Philadelphia, that the Egyptian crania are generally characterised by considerable symmetrical uniformity, as was to be anticipated, if there is any truth in the idea of undesigned artificial compression and deformation resulting from such simple causes as the mode of nurture in infancy.

The heads of the Fiji Islanders supply a means of testing the same cause, operating on a brachycephalic form of cranium; as most of the islanders of the Fiji group employ a neck-pillow nearly similar to that of the ancient Egyptians, and with the same purpose in view, that of preserving their elaborately dressed hair from dishevelment. In their case, judging from an example in the collection of the Royal College of Surgeons of London, the occipital region is

broad, and presents in profile a uniform, rounded conformation passing almost imperceptibly into the coronal region. Indeed the broad, well rounded occiput is considered by the Fijians a great beauty. The bearing of this, however, in relation to the present argument, depends on whether or not the Fiji neck-pillow is used in infancy. From the unreasoning uniformity of adherence to any national custom, common to all rude people, it is probable that the same pillow is used for the infant and the adult as among the Egyptians; but I have failed to obtain definite information on this point. In one male Fiji skull brought home by the United States Exploring Expedition (No. 4581), the occiput exhibits the characteristic full, rounded form, with a large and well-defined supra-occipital bone. But in another skull in the same collection that of Veindovi, Chief of Kantavu, who was taken prisoner by the U. S. ship Peacock in 1840, and died at New York in 1842, the occiput, though full, is slightly vertical. The occipital development of the Fiji cranium is the more interesting as we are now familiar with the fact that the artificially flattened occiput is of common occurrence among the islanders of the Pacific Ocean. "In the Malay race," says Dr Pickering, "a more marked peculiarity, and one very generally observable, is the elevated occiput, and its slight projection beyond the line of the neck. The Mongolian traits are heightened artificially in the Chinooks, but it is less generally known that a slight pressure is often applied to the occiput by the Polynesians, in conformity with the Malay standard."* Dr Nott, in describing the skull of a Kanaka of the Sandwich Islands who died at the Marine Hospital at Mobile, mentions his being struck by its singular occipital formation; and this he learned was due to an artificial flattening which the islander had stated to his medical attendants in the hospital was habitually practised in his family.† According to Dr Davis, it is traceable to so simple a cause as the Kanaka mother's habit of supporting the head of her nursling in the palm of her hand.‡ Whatever be the cause, the fact is now well established. The

* Pickering's Races of Man, p. 45.

† Types of Mankind, p. 436.

‡ Crania Britannica, Dec. III. pl. 24 (4).

occipital flattening is clearly defined in at least three of the Kanaka skulls in the Mortonian Collection; No. 1300, a male native of the Sandwich Islands, aged about forty; No. 1308, apparently that of a woman, from the same locality; and in No. 695, a girl of Oahu, of probably twelve years of age, which is markedly unsymmetrical, and with the flattening on the left side of the parietal and occipital bones. The Washington Collection includes fourteen Kanaka skulls; besides others from various islands of the Pacific, among which several examples of the same artificial formation occur: *e.g.*, No. 4587, a large male skull, distorted and unsymmetrical; and No. 4367, female? from an ancient cemetery at Wailuka, Mani, in which the flattened occiput is very obvious.

The traces of purposed deformation of the head among the islanders of the Pacific have an additional interest in its relation to one possible source of South American population by oceanic migration, suggested by philological and other independent evidence. But for our present purpose, the peculiar value of those modified skulls lies in the disclosures of influences operating alike undesignedly, and with a well-defined purpose, in producing the very same cranial conformation among races occupying the British Islands in ages long anterior to earliest history; and among the savage tribes of America and the simple islanders of the Pacific in the present day. They illustrate with even greater force than the rude implements of flint and stone found in early British graves, the exceedingly primitive condition of the British Islanders of prehistoric times.

On Variation in the Number of Fingers and Toes, and in the Number of Phalanges, in Man. By JOHN STRUTHERS, M.D., F.R.C.S., Lecturer on Anatomy in the Edinburgh School of Medicine. (Plate II.)

At the present time when the subject of variation is attracting so much attention, the following illustrations will, perhaps, be read with additional interest. I have arranged the cases of increase in the number of digits into

two groups, those which illustrate original variation, and those which illustrate the phenomena of hereditary transmission. The cases of variation in the number of the phalanges are given separately, as belonging to a different order of variation. In regard to those cases for which I am indebted to the kindness of various medical friends, whose names will appear, I may mention that they have been most carefully described to me in reply to a series of inquiries relating both to the anatomical condition and the hereditary history, and that what is stated may be relied on both for accuracy and for all possible completeness. A few remarks are added at the end on some of the points in variation and inheritance which the cases illustrate, and on the special point of the absence of a bone in the thumb and great toe, as compared with the other digits.

PART I.—VARIATION IN THE NUMBER OF DIGITS.

SECTION 1.—INCREASE IN THE NUMBER.

Group 1.—CASES WHICH COULD NOT BE TRACED TO HAVE HEREDITARY ORIGIN.

1. Case of Six Digits in three members of a family, occurring Symmetrically.

In this family of seven children, one of the daughters was born with six digits on each foot, and two of the sons with six digits on each foot and on each hand. I am indebted to the kindness of Dr John Alexander Smith for a full account of the case, and for affording me the opportunity of seeing the persons who are the subjects of the variety.

(a.) M—O—, female, æt. 22, Edinburgh, was born with a

EXPLANATION OF PLATE II.

- Fig. 1. R. and L. Hands of J—O—. Case 1. From a photograph.
 Fig. 2. Feet of J—O—. Case 1. From a photograph.
 Fig. 3. Feet of C—O—, younger brother to the last—Case 1. From a photograph.
 Fig. 4. Hand of G—S—. Case 14. From a sketch.
 Fig. 5. Hand of J—M—. Case 24. From a cast.
 Fig. 6. Hand of J—J—. Case 26. From a photograph.

Fig 1. L.



Fig.1. R.



Fig 2.



Fig 3



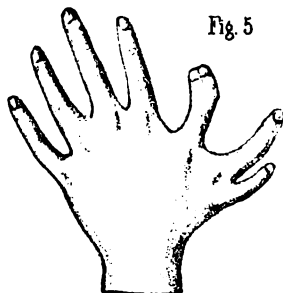
Fig. 4.



Fig 6.



Fig 5





sixth toe on the outer side of each foot. That on the right foot, being loosely attached, was removed by the surgeon a fortnight after birth, and the cicatrix is visible. The sixth toe on the left foot is fully equal in size to the fifth, and is supported on the fifth metatarsal bone. The hands are normal.

(b.) The elder brother, J— O—, æt. 19, has six fingers on each hand and six toes on each foot. The additional digits are placed on the outer side of the limb. Figs. 1 and 2 are taken from photographs of the hands and feet. Each sixth finger diverges considerably, reaches to a little beyond the joint between the proximal and middle phalanges of the fifth finger, and contains two phalanges, the proximal $1\frac{1}{2}$, the distal $\frac{3}{4}$ inch in length, the distal phalanx being somewhat longer on the left hand. The fifth and sixth fingers rest on one metacarpal bone, which broadens considerably in its distal third, where it is first grooved and then deeply notched, so that the bifurcated ends are as wide apart as the other knuckles. The left sixth finger looks shorter than the right, owing to the metacarpal head which supports it being farther back. The other fingers have the usual proportionate length. Each sixth finger moves with strength and quite as independently as the other fingers, without the others being held, and there is no tendency to move the fifth when he moves the sixth. The flexor and extensor tendons are, therefore, separate up to the muscles.

The sixth toe is well formed on each foot, lying parallel in the series. The fifth is of normal size, and the sixth rather larger than it, although, from commencing farther back, it does not project so much as the fifth. The fifth metatarsal broadens and then bifurcates to support the fifth and sixth toes, the bifurcation being greatest on the right side. Although it is not very easy to make this out, the sixth toe appears to have only two phalanges on each foot. The fifth and sixth toes are slightly webbed, but less so than the second and third, which are, as is common, webbed for $\frac{1}{2}$ to $\frac{1}{3}$ of an inch.

(c.) The younger brother, C— O—, æt. 10, was born with twenty-four digits, like his brother. The toes, as seen in fig. 3, also from a photograph, exactly resemble those of the elder brother (fig. 2), making allowance for the latter having corns. I can make out three phalanges in all the lesser toes except the sixth, which appears to have but two. The fifth metatarsal bones bifurcate as in the brother. The sixth fingers projected outwards so inconveniently, that they were removed, together with their supporting head of the bifurcated fifth metacarpal bone, three years ago by Dr Smith, and a cicatrix and slight bony eminence remain. On examination of one of the removed fingers I find it to contain two phalanges, which appear to be entirely destitute of flexor tendons, the proximal phalanx alone having an extensor tendon.

History of the O— Family.—The father and mother have the fingers and toes normal, and the variety was previously unknown in the family on either side. Both grandmothers were present at the birth of the daughter, the first child which presented the

variety, and both stated that they had never known or heard of it in their ancestors, neither having ever heard of such a thing before except in the case of the son of the giant in Gath, which they called to recollection in discussing the wonder. The first child of this family was a boy, without any digital peculiarity. The mother next had four miscarriages, between the 4th and 7th months, two boys and two girls, also it is stated with the hands and feet quite natural. The sixth child, the daughter M—, was the first to present the variety; she had six toes as above described. The family had lived in Forfarshire, and now went to Glasgow.

When again pregnant, the mother's thoughts must have been strongly turned in this direction, as she relates that she heard as it were a distinct voice from the wall of the room say that the child would have more than the last. This was about the second or third month, and made a deep impression on her mind. The child was the boy James O— above described as presenting six fingers and six toes. Three daughters, successively, were the next children born, whose hands and feet were quite natural. Lastly came the boy Charles O—, above described as having been born with an additional digit both on the feet and hands.

Dr Smith has attended two of the daughters in their confinement; one has had three children all of whom have the fingers and toes normal; the other, the daughter who was born with six toes, has had one child, a male, who lived only a few days, in whom the toes and fingers were normal. M—, J—, and C— O— have two paternal uncles and two paternal aunts, and one maternal uncle and five maternal aunts, all of whom have families, and in none of them—uncles, aunts, or children—do the fingers or toes present any variety. The mother, Mrs O—, was a twin, and the twin sister has had three children.

The mother, Mrs O—, mentions that she used to work beside a girl who had a double thumb, but this was years before she was married. The father mentions that after the variety had occurred in his own family, he saw a man who was exhibited as a giant with six fingers and six toes. The O— family mention the case of a railway guard (George B—), known to them after the variety had appeared among them, who had an additional finger on the ulnar side of his right hand, and an additional toe, also said to be on the right foot. He died a few months ago and leaves five or six children, none of whom have additional fingers or toes.

The following Seven Cases present an Additional Thumb on one Hand.

2. J — S —, æt. 25, Berwickshire, has an additional thumb on the right hand. The metacarpal bone bifurcates to support the two thumbs, which are of nearly equal length and thickness, that next the forefinger being slightly thicker and more powerful than the other. Each contains two phalanges, the proximal $1\frac{1}{2}$ inch, the distal 1 inch in length, being as long as those of the thumb of the left hand, except the distal phalanges, which are $\frac{1}{2}$ inch shorter.

In girth, each is only a little less than the left-hand thumb. The twin thumbs diverge from each other by their proximal phalanges, and converge by their distal phalanges, and are webbed half-way. In extension, the distal phalanges become parallel; in flexion, their points come together by their edges, and give a firm nip. Each can be flexed and extended separately when the other is held, but not otherwise. Flexion at the metacarpo-phalangeal joint is less extensive than usual. Motion at the carpo-metacarpal joint is free, and, in the movements of opposition, both of the thumbs move together across the hand. In writing, the pen lies between the approximated points of the two thumbs. He is right-handed as usual.

He states that variation in the number of the fingers or toes was previously unknown in the family.

3. E—— N——, æt. 13, Edinburgh (brought to me by my pupil Mr George Dickson), has an additional thumb on the left hand. The metacarpal bone is bifurcated for $\frac{1}{2}$ to $\frac{1}{3}$ inch, and forms two diverging heads for the two thumbs. The thumb next the forefinger is the larger of the two, and has nearly the usual length, but is not thicker than the last two divisions of the forefinger. The lesser thumb at first diverges to the radial side, and then curves forwards at the metacarpo-phalangeal joint, which is somewhat loose. It has only one phalanx, which is $\frac{1}{8}$ inch in length, the two phalanges of the greater thumb measuring together $1\frac{1}{2}$ inch. The two thumbs move together in opposing the fingers. The lesser thumb is flexed and extended in common with the other, and has considerable power in flexion.

Her mother states that variety in the number of fingers or toes was previously unknown in the family.

4. W—— G——, æt. 3 months, Edinburgh (kindly sent to me by Mr Edwards), has the thumb of the right hand double. The thumb next the forefinger is of good size, and has the usual number of bones. The lesser thumb consists of one phalanx, which is attached, firmly but quite moveably, to the inner side of the proximal phalanx of the greater thumb beyond its middle. It is about half the thickness of the greater thumb, is half the nail's length shorter, and they are webbed together as far as opposite the beginning of the two nails.

The mother states that the variety was previously unknown in the family.

5. I am indebted to Dr Edward Robertson of Otterburn for a note of the case of T—— H——, æt. about 6 months, Northumberland, who had an additional thumb on the right hand. It was rather loosely attached at the radial side of the metacarpo-phalangeal articulation of the greater thumb, which, again, was rather smaller than the thumb of the left hand. Dr Robertson lately removed the lesser thumb, and I find that it contains one phalanx only.

The father, mother, and maternal grandmother, all state that nothing of the kind was before known in the family.

6. My pupil, Mr Purves of Dryburgh, has procured for me a note, by his father, of the case of J—— W——, Berwickshire, a

man who has a small-sized additional thumb on the left hand. It contains one phalanx, which is attached rather loosely at the end of the metacarpal bone, as if by dense tissue and skin, without connection with the joint. He has no voluntary power over it, but it is more sensitive to pain than the greater thumb is. It is over an inch in length, and has a well-formed nail.

He states that "there is no hereditary tendency in his family to such formations."

7. For this (and for case 10. of this group) I am indebted to Dr Gibson of Campbeltown. — M'M——, æt. 14, Argyleshire, has a double thumb on the right hand. The metacarpal bone is much larger than its fellow in the left hand. The proximal phalanx is double, the two being wrapped in a common investment of skin, presenting a broad flattened appearance externally. The distal phalanges are separate, with a little space between them. They converge again at the points, giving the thumb a lobster's claw appearance. He is able to pick up small objects between the two points, and thinks such a thumb rather an acquisition, as in picking small things out of his vest pocket. He has the full use of the thumb in opposing the fingers.

He has three brothers and four sisters, none of whom have any digital variety. Neither have the father nor mother, nor do they know of any such variety in past generations of their families. The mother accounts for it from her seeing a man with a double thumb when she was pregnant, but says it was not the same kind of double thumb.

8. I am indebted to Dr Henderson of Fordoun for the particulars of the case of H—— K——, Kincardineshire, æt. 32, who has an additional thumb on the right hand. The thumb to the radial side, constituting the sixth digit, is considerably smaller and shorter than the other, and they are partially webbed. She can move the thumbs together so as to hold a pen between them.

A former child of the mother's, by a first marriage, had a sixth finger on the ulnar side of one hand, but lived only three weeks. No previous case is known in the family on either side. The mother has no story as to the cause.

Three Cases (two of them Brothers) presenting an Additional Little Finger on one Hand.

9. Dr Edward Robertson mentions to me also the case of John B——, æt. 5 years, Northumberland, from whom immediately after birth he removed a small supernumerary little finger from the left hand. A brother, who is now dead, had a similar supernumerary finger also on the outer side of the left hand.

The father and mother have never heard of any such variety in their ancestors, or in any relative of the family.

10. J—— G——, æt. 20 months, Argyleshire, has a sixth finger loosely attached to the outer side of the little finger of the left hand, near the middle of the proximal phalanx, and set at a right

angle to the little finger. It is $\frac{1}{2}$ of an inch in length, and has two phalanges. The little finger (fifth finger) is two inches in length. He is an only child. The father and mother have no such variety, and never heard of it occurring in the family before.

Three Cases presenting Six Toes on one Foot.

11. J—— S——, Liverpool, æt. 39, brother to one of my students, has six toes on the right foot. The external metatarsal bone is not broader at its middle than in the other foot, but becomes grooved and then bifurcated for a short distance at the head, supporting the fifth and sixth toes. I can make out three phalanges in the fifth toe, but the sixth seems to have only two. The long extensor tendon is felt and seen to bifurcate for the fifth and sixth toes. The five lesser toes form a regular series retiring outwards, with nothing to attract notice except the number. It was some months, he mentions, until his mother made the discovery that he had a toe more on one foot than on the other. There is no trace of any attempt to throw out a sixth digit on the other foot, or on the hands.

He states that variety of the toes or fingers was previously unknown in the family, and he can trace his ancestors for several generations back. He is one of six brothers, none of whom are married but himself, and he has no family, although there have been several miscarriages. He has five sisters, four of whom are married and have families, but none present any digital variety. He is a very tall man, standing six feet four inches in his shoes. The brothers are rather tall, but under six feet. The mother and sisters are not tall. The father was under six feet.

12. I am indebted to Dr Finlay of Trinity, Edinburgh, for the opportunity of examining this case, and also the two next:—

J—— L——, æt. 40, Newhaven, has six toes on the left foot. The fifth metatarsal bone is broader than the corresponding bone in the right foot, and supports the fifth and sixth toes. The fifth toe is narrower than the sixth, and has three phalanges, while the sixth seems to have only two phalanges. The extensor tendons of the fifth and sixth toes are seen to come forward together. The fourth and fifth toes are partially webbed.

No instance of digital variety is known to have occurred among his ancestors on the father's side, who have lived in Newhaven from time immemorial. The grandmother came from Edinburgh, and said that it was equally unknown among her ancestors. J—— L—— has a brother and four sisters; he himself has a family of four boys and one girl; his brother has one son; and three of his four sisters are married, and have families of sons and daughters; but no member or relative of the family has any digital variety except himself.

13. R—— L——, æt. 15, Newhaven, has six toes on the left foot. The fifth and sixth toes are supported on one metatarsal bone. The fifth is a small toe, the sixth thicker than the little toe

of the other foot. The fifth and sixth toes have each only two phalanges; the proximal phalanges are close together, but can be made to move past each other. Digital variety is unknown in the family on either side. R— L— is one of nine children, three boys and six girls. The father's family have, as usual with the Newhaven fishermen, always been in Newhaven. The grandmother came from the Highlands, and never knew of such a thing on her side of the family. Both the father and mother have brothers and sisters in Newhaven, all with families of sons and daughters. The mother states that her father's left little toe grew very awkwardly across the others, not in consequence of the pressure of the shoe, but naturally.

A friend showed me the other day a peculiar curve of his little finger, which two of his brothers also have. His father's mother had exactly the same peculiarity; also some of the children of a paternal uncle.

14. *Case in which one Hand presents Seven or Eight Digits, forming an approach to the condition of Double Hand.*

G— S—, æt. 5, has seven digits on the left hand, and the one corresponding to the thumb double at its distal segment, constituting so far an eighth digit. In every other respect the boy is well formed. The appearance presented by the hand is seen in fig. 4. There are seven distinct metacarpal bones. The four fingers on the outer side present the usual form and proportionate length, each with its three phalanges. The fifth has the position and opposing action of the thumb. Besides the metacarpal bone, it has two segments, the distal of which contains two phalanges placed side by side, while the proximal phalanx is single. The twin distal phalanges can be made to move a little past each other, and the one to the inner or radial side ends partly by a prominent non-articular angle, as if its proximal phalanx were wanting. They are closely enveloped in a common integument, and their nails join at their contiguous edges. He moves the thumb independently and freely, so that all its tendons must be separate from those of the digits on either side. The sixth and seventh digits are like the ring and little fingers of a right hand, except that the little finger is proportionately small. Each has three phalanges, and they are webbed most of the way between the proximal phalanges. Their metacarpal bones are quite separate and moveable, and, as they pass up, have a direction forwards to the palmar aspect of the carpus. These two fingers are associated together in their movements, and can be moved independently of the rest of the hand, but there is a tendency to flex the other fingers at the same time. In general grasping, the thumb and sixth and seventh digits oppose the other four and clasp down upon them. The hand is flat where the ball of the thumb should be.*

* A somewhat similar case, in which the duplicity of the hand was more complete, with a rudimentary condition of the thumbs, by Mr J. Jardine

A grandfather and grandmother are still alive. The father has three brothers and three sisters, besides two sisters who died. Two of the brothers and the three sisters are married, all of whom have families of sons and daughters, except one of the brothers whose children are all daughters, and the father himself has three daughters and another boy besides G—. But no other case of such, or of any, digital variety has been or is known in the family, either on the father's or on the mother's side.

Group 2.—CASES OF INCREASE IN THE NUMBER OF DIGITS, WITH HEREDITARY ORIGIN.

15. Case in which the Variety has been transmitted through at least Four Generations.

I am indebted to Dr Hamilton of Falkirk for the following case (and for case 16), and have to thank him for the great trouble and interest he has taken in ascertaining the genealogy of the family.

The great-great-grandmother, Esther P— (who married A— L—), had a sixth little finger on one hand. Of their eighteen children (twelve daughters and six sons), only one (Charles) is known to have had digital variety. We have the history of the descendants of three of the sons, Andrew, Charles, and James.

(1.) Andrew L— had two sons, Thomas and Andrew; and Thomas had two sons; all without digital variety. Here we have three successive generations without the variety possessed by the great-grandmother showing itself.

(2.) James L—, who was normal, had two sons and seven daughters, also normal. One of the daughters became Mrs J— (one of the informants), and had three daughters and five sons, all normal except one of the sons James J—, now æt. 17, who had six fingers on each hand. The additional fingers in this boy's case were loosely attached at the metacarpo-phalangeal joint of the little finger, and were removed by Dr Hamilton a few days after birth.

In this branch of the descendants of Esther, we see it passing over two generations and reappearing in one member of the third generation, and now on both hands.

(3.) Charles L—, the only child of Esther who had digital variety, had six fingers on each hand. He had three sons, James, Thomas, and John, all of whom were born with six fingers on each hand, while John has also a sixth toe on one foot. He had also five other sons and four daughters, all of whom were normal.

(a) Of the normal children of this, the third generation, the five sons have had twelve sons and twelve daughters, and the four

Murray of Brighton, was lately communicated to the Royal Medico-Chirurgical Society of London. Noticed in "The Lancet," Dec. 20, 1862. I am aware of a case of a child with complete double foot, at least at the digital and metacarpal part, but have as yet seen only a sketch of it.

daughters have had four sons and four daughters, being the fourth generation, all of whom were normal. A fifth generation in this sub-group consists as yet of only two boys and two girls, who are also normal.

In this sub-branch, we see the variety of the first generation present in the second, passing over the third and fourth, and also the fifth as far as it has yet gone.

(b) James, had three sons and two daughters who are normal.

(c) Thomas, had four sons and five daughters who are normal; and has two grandsons, also normal.

In this sub-branch of the descent, we see the variety of the first generation, showing itself in the second and third, and passing over the fourth, and (as far as it as yet exists) the fifth generation.

(d) John L—— (one of the informants) had six fingers, the additional finger being attached on the outer side, as in the case of his brothers James and Thomas. All of them had the additional digits removed. John has also a sixth toe on one foot, situated on the outer side. The fifth and sixth toes have a common proximal phalanx, and a common integument invests the middle and distal phalanges, each having a separate nail.

John L—— has a son who is normal, and a daughter, Jane, who was born with six fingers on each hand and six toes on each foot. The sixth fingers were removed. The sixth toes are not wrapped with the fifth, as in her father's case, but are distinct from them. The son has a son and daughter, who, like himself, are normal.

In this, the most interesting sub-branch of the descent, we see digital increase, which appeared in the first generation on one limb, appearing in the second on two limbs, the hands; in the third on three limbs, the hands and one foot; in the fourth on all the four limbs. There is as yet no fifth generation in uninterrupted transmission of the variety. The variety does not yet occur in any member of the fifth generation of Esther's descendants, which consists, as yet, only of three boys and one girl, whose parents were normal, and of two boys and two girls whose grand-parents were normal. It is not known whether, in the case of the great-great-grandmother, Esther P——, the variety was original or inherited.

16. *Case of Additional Thumb, with distant Hereditary Origin.*

—— B—— æt. 3, Linlithgowshire, was born with an additional thumb on the left hand, which Dr Hamilton removed when she was a few months old. The upper end of the proximal phalanx was left, for security to the metacarpo-phalangeal joint, and has since grown to some extent. It was not much smaller than the thumb which was left. The only other child, also a daughter, is normal.

The mother's maternal uncle (W——) had a thumb of the same kind, which Dr Hamilton has seen. His five sons and five daughters, however, were normal. The mother of the girl B—— is one of a family of four brothers and five sisters, all of whom are normal, and

their children, twenty-four sons and twenty-seven daughters, were likewise all normal, except one of Mrs B——'s daughters, being the case under description.

The occurrence of it in the maternal grand-uncle, indicates the variety to have existed in some generation previous to his. The case shows it to have passed over at least two generations, the grandmother and the mother, and to have reappeared in the third generation in the case of the girl B——.

For this and the five following cases 17, 17 (a), 18, 18 (a), 19, 19 (a), I am indebted to Dr Gibson of Campbelltown, who has most kindly taken much interest and trouble in obtaining and sending me the information.

17. Case of Additional Digit on one Hand, with Three Phalanges and a Metacarpal Bone ; and Additional Digit on each Foot. Direct Hereditary Origin.

W—— S——, æt. 73, Argyleshire, has, on his left hand, a sixth finger, placed midway between the thumb and fore-finger. It has three phalanges and a metacarpal bone, all clearly felt. It hangs pendulous, not having the power of extension. Its length is $3\frac{1}{2}$ inches, that of the thumb being $2\frac{1}{2}$, that of the fore-finger 4 inches. The thumb and fore-finger are each 3 inches in circumference, the intermediate digit $2\frac{1}{2}$.

He states that he has six toes on each foot, and that the additional toe is placed and formed in the very same way as in the hand. His children, four in number, are without digital variety, but it is hereditary, as fully given with the next case, that of a sister.

17 (a). Case of Two Thumbs, one of them with Three Phalanges on each Hand ; and Two Great Toes on each Foot, with an Additional Metatarsal Bone on one Foot. Direct Hereditary Origin.

A—— S——, Argyleshire, has six digits on each hand and on each foot. The additional digit is on the inner side. One of the thumbs presents three phalanges, on each hand, and one of the additional great toes has a separate metatarsal bone.

The two thumbs are supported on one unbifurcated metacarpal bone. The thumb next the fore-finger is clearly felt to have three phalanges, while the lesser thumb has two phalanges. It is the same on both hands. The greater thumb is $2\frac{3}{4}$ inches in length on the left hand, and $2\frac{1}{2}$ on the right ; the lesser thumb is 2 inches in length on both hands. The circumference of the greater thumbs on the left and right sides, respectively, is $2\frac{1}{2}$ and $2\frac{1}{4}$, that of the lesser thumbs being 2 inches. The two thumbs are webbed at their base. In each hand, it is the thumb next the fore-finger which is used.

In the *left foot* there are two great toes growing from one metatarsal bone, each having two phalanges, which are distinctly felt. The inner toe is the greatest, is $2\frac{1}{2}$ inches in length and $3\frac{1}{2}$ in circumference; the outer is 2 inches in length and 2 inches in circumference.

On the *right foot* there are six toes very regularly set, and six metatarsal bones. The great inner toe has two phalanges, is 2 inches in length, and $3\frac{1}{2}$ in circumference; the second is $1\frac{1}{2}$ in length, and $2\frac{1}{2}$ in circumference, and has two phalanges. The outer of the two great toes—that next the other toes—is therefore the lesser on both feet.

The variety is in the family. It came into the family through the paternal grandmother, who was a relative of the — family, in which digital variation exists. This grandmother herself is not stated to have had the variety, but the father of A— S— had six toes on each foot and “very long thumbs.” Of his seven children, a daughter and three sons have no digital variety; the other daughter has, as above described, six digits on each hand and foot; a son has the great toes double; and the remaining son, whose case is last described, has six digits on each foot and on one hand. This son has four children, who are all normal. Of the other sons, two, who are normal, are married and have fifteen children, all of whom are likewise normal.

18. *Case of Double Distal Phalanx of Thumb, with distant Hereditary Origin.*

J— H—, æt. 70, Argyleshire, has the thumb on the right hand double at the distal phalanx, while the metacarpal bone, and the proximal phalanx, are single. The distal segment is at first much flattened, and then bifurcated for about half an inch; each has a separate phalanx and a nail. The distal divided segment remains constantly extended.

Neither of his six brothers, or eight sisters, or father or mother, had any digital variety. His maternal grandmother, herself normal, was a member of the family already alluded to, among whom such variety exists; and the next case is that of a sister's grand-daughter.

18 (a). *Case of Additional Thumb on each Hand, with Additional Metacarpal Bones, and Double Great Toe on one Foot. Distant Hereditary Origin, connected with the preceding case.*

J— D—, æt. 4, Argyleshire, has two thumbs of equal size on each hand, and a double great toe on the left foot. The two thumbs on each hand have each two phalanges, and also each a metacarpal bone. The two thumbs of each hand possess flexion and extension and other motions perfect, and are equally useful.

The metatarsal bone of the left great toe appears to be grooved

but not bifurcated. Each of the great toes which it supports has two phalanges. They are wrapped in a common integument, except near the point, where they separate. Each has a nail. The phalanges of the outer lie partly upon those of the inner great toe. She has a brother and two sisters, but no member of the family, later than the brother of her maternal grandmother (case of J—H—, last related), has any digital variety. She is, through the same channel, a distant cousin of the family, already alluded to, among whom digital variety exists.

19. *Case of Additional Thumb on one Hand, with distant Hereditary Origin.*

J—F—, æt. 8, Argyleshire, has an additional thumb on the left hand. The distal end of the metacarpal bone is bifurcated. The lesser thumb is situated on the inner side, and has two phalanges. It is $1\frac{1}{2}$ inch in length, the larger thumb being 2 inches. The distal phalanx of the lesser thumb remains in the flexed position, and cannot be extended voluntarily. The thumb of the right hand is longer than usual, and is somewhat finger-like.

He has one brother and two sisters, neither of whom have digital variety, nor have the father or mother. The father's grandfather is said to have had some digital variety. The thumb on the right hand is longer than usual, and somewhat finger-like.

19 (a). *Case of Additional Thumb on one Hand, with Additional Metacarpal Bone. Distant Hereditary Origin.*

S—M—, æt. 7, Argyleshire, has an additional thumb on the right hand. It has two phalanges and a metacarpal bone, which articulates with the inner side of the metacarpal bone of the greater thumb, near the carpal extremity. The larger thumb is 2 inches in length, the lesser $1\frac{1}{2}$. It begins to leave the greater thumb opposite about the middle of the metacarpal bone of the latter, and its point reaches to the last joint of the greater thumb, along the side of which it lies. All the bones are clearly felt.

She has four brothers and five sisters, who, as well as the father and mother, have no digital variety. A first cousin, daughter of a maternal uncle, had a double thumb.

20. *Case of Six Fingers and Toes, with Interrupted Hereditary Origin.*

I am indebted to Mr J. Jardine Murray, F.R.C.S. Edinburgh, of Brighton, for a note of the case of C—G—, æt. 12 months, who was born with six fingers on each hand, the additional finger being on the ulnar side, and six toes on the right foot. The sixth toe is on the outer side of the foot, and lies more upon the dorsum

than to the outer side of the fifth toe. Mr Murray removed the supernumerary fingers in July 1862.

A brother has the same variety. The grandmother, on the father's side, had the same variety; also a sister of the father's.

SECTION 3.—DIMINUTION IN THE NUMBER OF THE DIGITS.

21. *Dissection of Three Limbs of a Child, presenting Diminution in the Number of the Digits.*

The child was born with the right hand presenting only two fingers, webbed together; the right foot presenting but three toes; and the left foot with the fourth and fifth toes united at their base. I was indebted to the kindness of Dr Keiller for obtaining the limbs. I have no history to the case, but have thought it worthy of notice from the opportunity of ascertaining by dissection how far the variation affected the deeper parts—the bones, muscles, and nerves.

(1.) **DISSECTION OF HAND.**—Externally there are two digits, one on the radial side considerably thicker than the other. It will be convenient to speak of the first as the pollex, and of the other as the little finger. They are webbed to the end, presenting a notch at the end on the palmar aspect. The nails are distinct, but close together. The palm is the same breadth as the forearm, and gradually tapers into the fingers. Both palm and fingers have the usual length in proportion to the forearm.

BONES.—Each digit has only two phalanges, the proximal about twice the length of the distal. There are two metacarpal bones. The four bones of the proximal carpal row are present, the second, or semilunar, small. The second row of carpals is represented by two bones, which are coalesced with, or prolonged from, the first row. The piece supporting the ulnar metacarpal, corresponds in position to the unciform, and is fused with the cuneiform. The piece supporting the radial metacarpal is fused behind with the scaphoid, and may represent the os magnum, trapezoid, or trapezium, or all three fused together. The bones and joints of the forearm are fully developed.

MUSCLES.—All the muscles of the forearm are present except one. The pronators and supinators are unusually large, as is also the Flexor Carpi Ulnaris. The *Flexor Sublimis Digitorum* is small, wants its radial origin, and ends in two tendons; one joins the tendon of the deep flexor, which goes to the little finger, the other ends in the annular ligament. The *Flexor Profundus Digitorum* is full sized, and ends in two tendons; one, the larger, ends in the annular ligament, the other forms a strong flexor tendon, going on to the distal phalanx of the little finger. The latter is joined at the wrist by the tendon of the superficial flexor, which is here pierced by the deep tendon, after which the two are incorporated.

The absence of a *tendo perforatus* proceeding separately to the finger corresponds to the fact of the absence of one of the phalanges. The *Flexor Longus Pollicis* is large, arising also from the edge of the ulna. It proceeds entirely to the distal phalanx of the pollex. It is not joined by any slip, except that, above, the fleshy slip which usually descends to the muscle from the condyloid origin of the flexor sublimis is unusually large.

The short muscles of the little finger are all present with the usual attachments. So also the short muscles of the thumb, with some modification. The Abductor is large. The Adductor arises from the annular ligament. The Opponens is small, much smaller than the opponens (flexor ossis meta-carpi) minimi digiti. The *Flexor Brevis Pollicis*, undersized, arises from the two sides of the metacarpal bone; the ulnar head appearing like a second interosseous. There is one *Interosseus* muscle for the little finger, along its radial side.

Extensor Muscles.—The *Extensor Carpi Radialis Longior* sends a small tendon to the base of the metacarpal bone of the pollex, and a large tendon to between the bases of both metacarpals, chiefly to the ulnar. The *Extensor Carpi Radialis Brevior*, also large, goes to the carpus just above the ulnar metacarpal. The *Extensor Carpi Ulnaris* is entirely wanting. The *Extensor Communis Digitorum* gives a tendon to each finger, that to the pollex only a little the largest. The *Extensor Minimi Digiti* is quite distinct from the latter as a muscle. Its only tendon goes to the little finger, joining with the tendon to that finger from the common extensor, the latter being the larger. Two muscles form the deep layer. One has the fleshy attachments of the *Extensor Ossis Metacarpi Pollicis*, but its tendon goes to the first phalanx, and is therefore the *Extensor Primi Internodii*. The other muscle has the fleshy attachments of the long extensor of the thumb, and the extensor of the fore-finger. Besides two tendinous slips to the back of the carpus, it ends in two tendons which join the tendons of the long common extensor. One of them, therefore, represents the *Extensor Secundi Internodii Pollicis*; the other the *Extensor Indicis*, going to the only remaining finger.

NERVES.—The *Ulnar* supplies the ulnar side of the little finger, and unites with the median for the supply of the radial side of the same finger. The *Median* supplies both sides of the pollex, and is also the principal nerve for the ulnar side of the little finger. The interdigital space receives for its supply, as above described, a series of nerves, three from the median and two from the ulnar, as if the nerves of the wanting fingers had been crowded into the cleft.

(2.) *DISSECTION OF RIGHT FOOT.*—This foot presents three toes, the internal having the usual characters of the great toe.

BONES.—Each of the lesser toes has the three phalanges—the great toe the usual two. The *metatarsal* bones are three in number, the internal having its usual great size. The second *atarsal* row presents a bone for the support of each metatarsal—two cuneiform bones and the cuboid—but the latter is fused with the *os calcis*.

The part corresponding to the cuboid has a separate bony nucleus, smaller than the nucleus of the os calcis. Of the first row, the astragalus is fused with the os calcis, and the scaphoid is wanting or fused with the astragalus.

MUSCLES.—The *Flexor Brevis Digitorum* sends tendons to the two lesser toes, that to the external being much the largest. The *Flexor Longus Digitorum* and *Flexor Longus Pollicis* are separate muscles in the leg, but at the ankle form a common tendon, which at the middle of the foot, after receiving a large accessorius, divides into two, one for the great toe, the other for the second toe, which perforates the tendon of the short flexor. There is one *Lumbricalis*, arising from both sides of the long tendon to the second toe, and going to the tibial side of the same toe. Two muscular bundles arise from the abductor minimi digiti, besides the usual tendon of that muscle, and proceed one to the fibular side of the second toe, the other to the tibial side of the outer toe. They are like large lumbricales, or additional short flexors. The *Flexor Brevis Minimi Digiti* is wanting. There are two plantar *Interossei*, one for the tibial side of each of the two lesser toes; and one dorsal, in the outer space, for the middle toe.

Extensors.—The *Extensor Brevis Digitorum*, gives five distinct digital tendons, three to the middle toe, one of which is larger than the tendon to the outer or to the great toe. All of the five come, as usual with the tendons of this muscle, from separate portions of the muscle. A sixth portion and tendon passes to the external metatarsal bone, and here represents the peroneus tertius, which is wanting in its usual situation. The *Extensor Longus Digitorum* is small, and gives at the ankle a tendon to join the extensor longus pollicis, the rest of the tendon ending on the os calcis. It is just possible that an intra-uterine fracture of the tibia which had taken place, with angular union, may account for the atrophy of this muscle, but the other muscles of the leg are well formed. The *Extensor Longus Pollicis* is large and separate throughout, except that it receives a tendon from the extensor longus digitorum.

The muscles not alluded to in these notes present their usual arrangement.

NERVES.—The distribution of the plantar digital nerves is somewhat remarkable. The *external plantar* sends a nerve to the fibular side of the outer toe, and a second to both sides of the outer interdigital cleft. The *internal plantar*, which has the usual preponderance in size, sends, first, a nerve for the tibial side of the great toe; second, a branch to the internal cleft which gives off first one and then a second set of subdivisions to the sides of the cleft, the double nerve on each side keeping a plantar course; third, a branch to join with the external plantar in supplying both sides of the outer cleft. Also there is a twig from the deep or muscular division of the external plantar, through below the flexor brevis digitorum muscle, to join one of the branches of the internal plantar to the fibular side of the great toe. A similarly derived and similarly

placed connecting twig passed from the ulnar nerve to the median in the right hand.

The distribution of the internal plantar nerve, taken alone, would indicate that the wanting toes are the two outer; while the distribution of the external plantar, taken alone, would indicate that the second and third are the wanting toes; but the double apparatus of nerves to each cleft, and their source, is exactly explained by supposing the second and fourth to be the wanting toes.

(3.) **DISSECTION OF LEFT FOOT.**—The fourth and fifth toes are united at their bases. The foot is in the condition of talipes varus.

BONES.—The bases of the proximal phalanges of the fourth and fifth toes are united in a single piece, and in front of this they are held together by a strong transverse ligament, with an anterior concave edge. Each of the lesser toes has three phalanges, except the fifth, in which the distinction between the second and third phalanges is not evident, but the softness and smallness of the parts render it difficult to pronounce as to this. It has, however, a tendon from the flexor brevis digitorum muscle. The fifth *metatarsal* bone is wanting. The fourth is double sized, broader but not bulkier than that of the hallux, but it presents no trace of duplicity externally, and a section shows one large medullary canal. The *tarsal* bones present no variety, except that the cuboid is narrower than usual, having only one metatarsal bone to support, and that the astragalus is fused with the os calcis.

MUSCLES.—The fourth and fifth toes receive no tendon from the *Extensor Brevis Digitorum*, and but one tendon from the *Extensor Longus Digitorum*, which divides at the metatarso-phalangeal joint into a tendon for each of the partially united toes. The external tendon of the *Flexor Brevis Digitorum* divides in the same manner at the head of the metatarsal bone to supply these two toes. So does the external tendon of the *Flexor Longus Digitorum*, after crossing the metatarso-phalangeal joint.

The most external of three *Lumbricales* is double-sized and goes to the tibial side of the fourth toe. The first lumbricalis, besides going to the tibial side of the second toe, sends a slip to the fibular side of the great toe. A muscle arises partly with, partly behind, the adductor pollicis, and goes to the neighbouring sides of the second and third toes. Of the two plantar *Interossei*, the external is very large and goes to the fourth toe. The *Flexor Longus Digitorum* and *Flexor Longus Pollicis* form a common muscle and tendon. The muscle splits naturally enough into two, but the outer portion comes mainly from the tibia and partly from the upper part of the fibula, the usual place of origin of the flexor longus pollicis from the fibula being unoccupied. The tendons of these two portions form one indivisible tendon above the ankle, which after receiving the accessorius, divides into four tendons, the internal for the hallux being the greatest, the external, as already noticed, for the two partially united outer toes.*

* It is an error to regard the so-called "*Flexor Longus Pollicis*" as a flexor of the great toe only, or to consider the presence of a tendon from it to

PART II.—VARIATION IN THE NUMBER OF PHALANGES.

SECTION 1.—DIMINUTION IN THE NUMBER.

22. *Case in which all the Fingers and Toes want a Phalanx, in several Members of a Family.*

I am indebted to Dr Oswald H. Bell of St Andrews for the description and history of this case, and for affording me an opportunity of seeing the boy.

David M.—, æt. 18, St Andrews, wants a phalanx in each of his fingers and toes, and has a brother and sister similarly formed. The two hands are precisely similar. The thumb consists of a short metacarpal bone ($\frac{1}{2}$ inch in length) and of one phalanx, $1\frac{1}{2}$ inch in length, the joint between them being loose, as if composed of some soft intermediate tissue. The fore finger is so much longer than the others as to suggest the appearance of a hand in the act of pointing. This is due to the greater length of its metacarpal bone, which is 3 inches in length, while the next two metacarpals are under half that length. The metacarpal of the little finger is just $1\frac{1}{2}$ inch in length, but, from its obliquity, does not project so far as the fourth. The proximal phalanx of the fingers measures, in the index $1\frac{1}{2}$, in the middle $1\frac{1}{2}$, in the ring 1, in the little finger $1\frac{1}{2}$; the distal phalanx, in the index and middle, $\frac{1}{2}$, in the ring and little fingers $\frac{1}{2}$ inch. On the left side, the distal phalanx of the index finger is proportionately shorter. Except in the case of the fore finger, the five digits present their usual relative projection. The metacarpo-phalangeal joints, especially of the index and middle fingers, are considerably sunk behind the web, and are loose, while the joint between the two phalanges does not bend down with the usual degree of angularity.

He can easily seize and retain minute articles as a needle or pin, between the thumb and index finger, and can write with comparative ease. Being a groom, he can drive tolerably well, though he is apt to let the reins slip, being unable in the usual way to form the digital hook which the third phalanx naturally completes.

The feet are well formed as far forward as the distal ends of the metatarsal bones. The toes are short, pulpy, and very loosely articulated. The lesser toes have two phalanges each, and are much turned up at the interphalangeal joint; the great toe has its usual proportionate greatness, but, like the thumb, has only one phalanx. The pads below the anterior end of the metatarsal bones, behind both the great and lesser toes, are more developed than usual. No the second toe as an occasional occurrence only. The tendinous slip commonly described as passing between the Flexor Longus Pollicis and the Flexor Longus Digitorum, in the sole of the foot, is nothing less than a tendon from the former to at least the second toe, of good size when the proportionate size of the two toes is considered. It is, normally, the principal flexor tendon of the second toe, and the first lumbricalis muscle is attached chiefly to it. (See communication by the author to the Edin. Medico-Chirurgical Society, "Edin. Medical Journal," July 1868.)

one would suspect from his gait any deficiency in the feet. He is 5 feet 2 inches in height, healthy and active.

Family history.—He is one of a family numbering ten in all, who were born in the following order. First, a son, and then successively three daughters, all normal. Fifth, a son, the first member of the family who presented the digital variety. Sixth and seventh twin girls, normal. Eighth and ninth, twin boys, one normal, the other whose case is above described. Tenth, a girl, with fingers and toes as in this boy, and, in addition, the feet turned in. Neither Dr Bell nor I have seen the brother and sister who have the variety, but the boy states that their fingers and toes are the same as his, the hands so like that when the arms are covered and the hands presented promiscuously, the mother cannot say to which brother they belong. The brother is a clerk and is said to write an excellent "hand." We have no information as to the ancestors, but neither of the parents, uncles, aunts, or cousins, had or have any deformity.

23. *Case in which four Fingers of one Hand possess but one Phalanx each, the Thumb having two Phalanges.*

Dr Henderson of Fordoun sends me a note and sketch also of the case of A—W—, æt. 3½ years. The four outer digits of the left hand are short nipple-like processes containing one phalanx each, so loosely attached to the metacarpals that they can be easily twisted round. Each has its little nail. The thumb contains two phalanges, is as large as that of the other hand, projects a long way beyond the fingers, and she makes great use of it.

Her father has an aunt with two thumbs on the left hand, similar to the case of H—K— (No. 8).

24. *Case in which the Fingers are formed so as to give the Hand a resemblance to a Foot.*

I am indebted to Dr Grierson of Thornhill for the description, and for sketches of the hand, of this case, a cast of which I had previously seen, obtained through Dr A. Mitchell and Dr A. Simpson.

J—M—, æt. 16, Dumfriesshire. The hand may be described, generally, as having a remarkable general resemblance to a foot, in the size and straight direction of the thumb, and the little development and outward lessening of the fingers. The sketch (fig. 5) will give a correct idea of the form of the hand. The thumb is the longest and thickest of the digits, and lies parallel to the fingers. It can be abducted, and only half crossed over the palm. It has two phalanges, besides the metacarpal bone. The distal end of the latter extends a little way beyond the separation between the thumb and next finger. The thumb alone has a nail. The next two fingers project for about an inch, the index more, the middle less. The ring and little fingers appear only as fleshy finger points. There is a bone projecting some way into the index and also into the

middle finger, either a phalanx or a prolongation of the metacarpal bone. The fourth and fifth fingers have no phalanges. The fingers have no voluntary motion. Viewed on the palm, the "ball" of the thumb is flat and narrow, but the eminence formed by the muscles of the little finger is prominent.

The other hand and the feet are normal. She is the third child in a family consisting of six daughters and three sons, all the rest of whom, as well as the father and mother, are normal. No such formation is known to have been presented by any ancestor. The mother's story is that in her third month of pregnancy she had a vivid dream that she saw a man killed and his fingers cut off, at her own door; that she told this at the time to her husband and neighbours, and could not for long get rid of the idea of the cut-off fingers.

25. *Case of Five Rudimentary Digits on one Hand.*

Mr James R. Crease of Gateshead, sends me the particulars and a sketch of the case of a female child, M— J— T—, ten days old, whose right hand presents a very rudimentary condition. The four fingers are present as mere rudiments, without nails; the thumb is about $\frac{1}{4}$ inch in length and has a nail. After the radius and ulna there are no bones to be felt in the hand, except one supporting the thumb. The rest of the limb is well formed.

Digital variety was previously unknown in the family history. There are three other children, boys, all well formed. An aunt by the father's side, had all the fingers, but not the thumb, of the right hand amputated, but the mother had not seen her for many years. When the mother was three months pregnant, her attention was attracted by a woman singing in the street who had a deformed right hand. For a time she always thought she saw this woman, but had no idea that anything would be wrong with the child.

SECTION 2.—INCREASE IN THE NUMBER OF PHALANGES.

26. *Case of Additional Phalanx in the Thumb, on both Hands. Probably of Hereditary Origin.*

J— J— æt. 21, Roxburghshire, now in Edinburgh, has the thumb on both hands of unusual length and form, as represented in Fig. 6, which is taken from a photograph. In examining the thumbs I was surprised to find an additional joint, giving three phalanges besides the metacarpal bone, the additional bone being placed in the position of a middle phalanx. A case so unusual deserves careful examination.

The metacarpal bone is $2\frac{1}{4}$ inches in length and appears to be quite normal. The first phalanx is $1\frac{3}{4}$ inch in length, and is, therefore, longer than usual. The *additional bone*, occupying the position of a middle phalanx, is broader on the inner, or radial, side than on the side next the index finger, having a triangular or wedge-shaped figure, which gives the distal phalanx an inclination towards the index. It measures along the radial side $\frac{3}{8}$ inch, along the ulnar

side $\frac{1}{4}$ inch, and $\frac{1}{2}$ an inch along the middle. The position of the base of this wedge-shaped additional bone is shown in the sketch by the double protuberance on the radial side. Between these a depression may be felt corresponding to the short shaft between the two articular ends. The distal phalanx is an inch in length. The measurements were made exactly from the joints, avoiding the error of including the knuckle twice.

When the thumb is straightened, its point passes a sixth of an inch beyond the joint between the proximal and middle phalanges of the index finger. The thumb of the right hand scarcely advances so far as that joint. The unusual length of the thumbs in this case is gained by the proportionately greater length of the proximal phalanx and by the presence of the additional bone, while the distal phalanx is a little shorter than usual. At the joint on the proximal side of the additional phalanx there is the usual extent of flexion and extension. Between it and the distal phalanx passive motion is free and readily felt in both thumbs, although it cannot be carried so far as to cause an angle or additional knuckle. Some motion in the lateral direction also can be made at this joint. The utility of the thumb is not impaired. The fingers are longer than usual (their lengths, from the metacarpo-phalangeal articulation, are, fore-finger $3\frac{1}{8}$, middle $4\frac{3}{8}$, ring $4\frac{3}{8}$, little $3\frac{1}{4}$, inches. The whole hand is 8 inches in length), present the usual proportionate length, and have three phalanges each. The great toes present the usual length, size, and number of phalanges. He is of full average stature, his height being 5 feet $9\frac{1}{4}$ inches.

A maternal aunt had the same kind of thumb on the right hand, being the only instance of the occurrence of the peculiarity in any relative of the family. This aunt has a son and three daughters. The mother had other three sisters and two brothers, all of whom, except one of the sons, have families of sons and daughters. The mother never heard that any of her ancestors had it.

REMARKS.

As some of the cases in the hereditary group show the tendency to have passed over at least two generations and then to have reappeared, it is possible that some of the cases recorded as original, may be cases of distant inheritance. But it must have had a beginning in these too. Careful inquiry was made in all the cases, and the previous occurrence of such a thing in the family is pretty sure to be brought up when a child is born with it. The cases one would feel most inclined to doubt on this score, are those in which it appeared in more than one child of the family, as in cases 1, 8, and 9.* In such cases we may suppose the

* I am indebted to Dr Strachan, of Dollar, for a case (which has arrived too late for detailed insertion at present) closely resembling Case 1. In a

cause which determined the occurrence in one child might equally determine it in another, or it might be held that the first case having occurred spontaneously, the circumstance had operated by an impression on the mind of the mother.

In all the cases of original variation, except in Case 1, and in one of the children in Dr Strachan's case, it appeared on one limb only.

This was not to any marked extent on one side of the body more frequently than on the other. The greater acquired strength of the right hand and foot might be supposed to operate on the offspring, but in the above cases it so happens that it appeared rather more frequently on the left side. Throughout the animal kingdom, when one side is more developed than the other, the right side is not selected more frequently than the left.*

The variation begins more frequently on the hand than on the foot. In the non-hereditary group of cases in which it affected either a hand or a foot only, it appeared on the hand in eleven of the cases, on the foot in four cases. The hereditary cases, by multiplying the fact, show a still higher proportion affecting the hand than the foot; and in the cases in which it occurs on three of the limbs, it is on a foot more often than on a hand that the sixth digit is wanting. None of the cases show hereditary transmission of the peculiarity on the feet alone, while the cases of hereditary transmission on the hands only are more numerous than those showing it on the hands and feet. This greater frequency of its appearance on the hand may be connected with the zoological fact that in those mammals in which the digits differ in number on the two feet, the greater number is on the fore foot; or, more generally, with the fact that greater separate use is made of the digits on the hand than on the foot.

The additional digit appears nearly equally on the outer or on the inner side of the limb. In the non-hereditary group, reckoning the first case as one, it appeared in 7 cases on the outer side, and in 7 cases on the inner side, as an

family of 8 brothers and 3 sisters, 3 brothers and one sister were born with digital increase. Two had an additional outer toe, one an additional outer finger, one had it on all the four limbs. It was unknown in the family as far back, at least, as the great-grand-parents.

* See a paper by the author in the "Edin. Medical Journal," June 1868.

additional thumb. Three of the seven external cases were on feet, on the outer side. In the hereditary group, taken as families, it is on the inner side in the majority of the cases. In one family only [cases 17, 17 (a), 18 (a)] does it affect the big toes. As far as these cases show, it would, therefore, seem to be more common on the outside of the foot, and, on the hand, more frequent on the inner than the outer side. Were we to connect these facts with the facts regarding the order in which the toes appear, or disappear, among the mammalia, we would require to consider not only the fact that the inner digit is the last added, but that the increase, or decrease, takes place alternately on the inner and outer sides.

On the hand, the additional digit was smaller than the next, always so in the case of an additional little finger, and nearly always so in the case of an additional thumb, but in case 18 (a) the two thumbs were equal, and in case 2 nearly equal. On the foot, in cases 12 and 13, the sixth toe is thicker than the fifth, although it has a phalanx less, but the thickness does not seem to depend on the bones. Among the hereditary cases, in case 17 (a) the internal of the two great toes is the larger.

Cases of non-hereditary increase in the number of the digits appear to be of not unfrequent occurrence. Most surgeons have met with cases, and it is a common practice to remove the additional digit early. The above cases show that the removal of the digit, in cases in which it is inherited, does not eradicate the atavic influence. But most of the cases of original variation do not appear to transmit the tendency, for, besides the facts mentioned in some of the cases, cases of original variation are much more common than hereditary cases, even reckoning each member of the existing family as a separate case.

Hereditary Transmission of the Variety.—The transmission of a newly acquired variety has to depend on whether the new influence or the prior normal influence proves the stronger; and, if the former prevails, it has, so long as only one parent presents the variety, farther to depend for its continuance on its chance of the fact that one parent may exert more influence than the other on some of the young. Thus if not itself at the beginning overcome by

atavism, it is liable to be lost at every new union, and, we would suppose, liable to be worn out at last. The case of the L—— family (case 15) illustrates various phenomena of variation and transmission. (1.) In the line of descent through Andrew L——, we see three successive generations without the variety, and it is apparently extinguished. (2.) In the line through James L——, he and his children were normal, but it re-appears in his grandchildren, having passed over two generations. In case 19, it had passed over three, and in case 16, over at least two, generations. In case 18 (a) although the grandmother's brother had it, the great-grandparents had not, so that it had passed over three generations in the direct line and reappeared in the fourth.

(3.) We see uninterrupted transmission in the line of descent through John L——, from Esther in the first (if she was the first), to Jane in the fourth generation. The six-fingered tendency had here maintained itself against three successive unions with five-fingered persons. In the first offspring it succeeded, as far as we know, in only one out of 18. In the offspring of this one (Charles L——) it succeeded in 3 out of 12. In two of these three it did not appear in the next generation; in the third of them it succeeded in one out of two children, thus being directly transmitted to the fourth generation.

It is interesting to notice, too, how the variety so far from being weakened had gathered force in each new generation, even although it had not the advantage of the greater development attending utility, to enable it to increase its hold on the organism. In the first known ancestor (case 15) it occurred on one hand; in the second generation on both hands; in the third on both hands of two brothers, and on both hands and one foot of a third brother; and in the fourth generation on all the four limbs—thus, so far from becoming weaker, making its way to an additional limb in each successive generation.

Besides thus extending itself symmetrically and serially, it may extend in depth, beginning as one phalanx and extending at length to the metacarpal or metatarsal region. In some of the cases of original variation, there was only

one phalanx, in most of them two phalanges. Among the hereditary cases, in case 19 (a) there is a metacarpal bone besides two phalanges; and, in case 18 (a), the same occurs in both hands, while on the six-toed foot it has not extended to the metatarsal bone. In case 17 (a) one of the thumbs on each hand presents three phalanges without an additional metacarpal, while the two great toes have, on both feet, two phalanges each, and one foot has an additional metatarsal bone. In case 17 there is an additional metacarpal bone and three phalanges. We do not know how it was with the ancestors of these cases, but in none of the cases of non-hereditary variation did it extend so deeply into the limb.*

When the variety is transmitted it is on the same side of the limb. In case 15, the additional digit was external, both on the hands and feet, in all the ramifications of the descent. Also in case 20. In cases 17, 17 (a) 18, and 18 (a), in which the variety is derived from a common ancestor by different lines of descent, it is on the inside of the hand or foot, or on the inside of both hand and foot. Also in case 16. In none of the cases was it external on one limb and internal on another limb of the same person, or in the same family. In case 8, of two children in the same family, one had an additional little finger, the other an additional thumb, but the case is one of the non-hereditary group, and there is the curious fact that they were the children of different fathers, though of the same mother. The influence of sex does not appear to any marked extent in the hereditary transmission in the above cases.

Diminution in the Number of the Digits.—Diminution in

* When the additional digit is supported, as it generally is when well formed, on one end of a more or less bifurcated metacarpal or metatarsal bone, it might be supposed that this indicated the original presence of an additional metacarpal or metatarsal bone, which had become more or less confluent with the next, as in the development of the metacarpus and metatarsus in the ruminant. But while this can be easily shown in the ruminant, there is no proof that here there is anything but a more or less extensive bifurcation of one bone. In the dissection of the left foot in case 21, in which the fourth and fifth toes were partially united and supported on one metatarsal bone, the fifth metatarsal being wanting, the fourth metatarsal was twice the normal thickness, but presented no trace of double origin, externally or internally, although the foot was that of a new-born child.

the number of the digits appears to be a much less frequent variation than increase, both in man and in mammals generally, although many mammals have normally less than five. Among these the occurrence of an additional digit is generally but the development of the concealed rudiment of a suppressed digit. In man, although he normally possesses the full mammalian number, the tendency is to farther increase rather than to decrease in the number.

Variation in the Number of Phalanges.—In this group of cases there was diminution of the number of phalanges in four cases, in none of which was it hereditary, although one of the cases embraces two brothers and a sister.*

From cases 23, 24 and 25, in which the fingers were more or less rudimentary, while the thumb was either not at all or less reduced, it would seem as if the thumb were the last to suffer reduction. Two cases have been mentioned to me, however, although I have not seen them, in which the thumb alone is wanting, in one of the cases on both hands. Homologically considered, we would expect

* See a case of hereditary transmission of deficient phalanx by Alf. Robert, mentioned in an able paper by Mr Sedgwick "On the Influence of Sex in the Hereditary Transmission of Disease" (*Brit. and Foreign Med. Chir. Review*, April 1863). Also a case by Dr Kellie of Leith (*Edin. Med. and Surg. Journal*, 1808, p. 252); but the mother's statement that it had been transmitted for ten generations must be received with caution, when we consider what ten generations implies. Among the more interesting cases of hereditary transmission of increased number of digits on record are—The case of the Maltese family, by Commander Godehew (*Mémoires de l'Académie Royale des Sciences*, 1751), more fully related by Reaumur (*L'Art de faire eclorre et d'élever des Oiseaux Domestiques*, tom. ii. p. 377): The case by Sir A. Carlisle (*Philosophical Transactions*, London 1814, p. 94): A case by Dr Crawford of Peebles (*Edinburgh Monthly Journal of Medical Science*, Oct. 1851, p. 356): A case by Mr J. B. Thomson of Perth (*Edin. Medical Journal*, 1858-59, p. 502); and a case noticed in "Medical Times and Gazette," Dec. 20, 1860, from the "American Medical Times," possibly a branch of the family whose case is related by Sir A. Carlisle. On this subject may be also consulted, Haller (*Elementa Physiologia*, t. viii. pp. 97, 98, 99, 1766): Morand (*Mém. de l'Acad. des Sciences*, 1770): Isidore Geoffroy St-Hilaire (*Histoire Générale et Particulière des Anomalies de l'Organisation*, 1832-36): and A. W. Otto (*Mons'rorum Sexcentorum Descriptio Anatomica*, 1841). The oldest recorded case is that of one of the sons of the giant of Gath (2 Samuel, ch. xxi., v. 20; and 1 Chronicles, ch. xx., v. 6) who had six fingers on each hand and six toes on each foot. Pliny notices two cases, among the Romans, of six fingers on each hand (XI. Book, ch. 48). Anne Boleyn is said to have had six fingers on each hand.

the thumb to be the first, teleologically considered, the last, to suffer reduction.

Increase in the number of phalanges occurs in the thumb only, or as in cases 17 and 17(a), in the digit serially corresponding to the thumb when six digits are present. None of the cases presented a higher number than three, the full number in the mammalian type. Case 26, in which, without any increase in the number of the digits, the thumb presents an additional phalanx, is a very remarkable one when we consider the mammalian law. Not unlikely, it may have been the same in the father of cases 17 and 17 (a), who is described as having had very long thumbs, while his son and daughter had three phalanges in the corresponding digit, and a thumb with two phalanges added by its side.

On the absence of a Bone in the Thumb and great Toe, as compared with the other Digits ; and on the Nature of the "Metacarpal" or "Metatarsal" Bone of the Inter-nal Digit.

The occurrence, normally, of a bone less in the thumb or great toe than in the other digits, is part of a law exemplified in the inner digit of all five-toed mammals,* and may be supposed to find its explanation in the fact that the internal is the small toe, and the one which has disappeared when the number is reduced to four. Whatever be its meaning, this law is maintained in the few cases in which the internal digit undergoes great teleological development, as in the seal and walrus,† in which the internal digit is longer and thicker than the three middle digits, and in

* This law is not without exception in cases of variation, as in case 26; and I have lately been presented by Mr Robertson of Kelso with a pig's foot, presenting five toes, in each of which there are three phalanges besides the metacarpal bone.

† I have seen a skeleton of the walrus with three phalanges in the pollex, but on trying with my penknife, I found one of them to be a piece of wood. In the skeleton of a seal I once saw three natural phalanges in the inner digit, but the first and fifth toes had been transposed. The phalanges and metacarpals and metatarsals of the seal are arranged and developed exactly as in man, and the presence of only two phalanges in the pollex and hallux is easily felt in the living seal.

man in whom the great toe and thumb are the most important of the digits. As the erect posture requires a great toe on the inner side of the foot, and a long one, the phalanges of the inner digit are, accordingly, developed in length as well as in thickness. The thumb, again, being better adapted for opposition by being shorter, is developed in thickness, and still retains its character as the shortest digit.*

It has long been a discussion in human anatomy, whether the wanting bone in the thumb and great toe is a phalanx or a metacarpal or metatarsal bone. The view that it is the middle phalanx which is wanting is supported by the arrangement of the muscles,—(a.) by the absence of the *tendo perforatus*; (b.) by the attachment to the first phalanx of the short muscles which correspond to the short muscles of the little finger, which are attached to its first phalanx; (c.) by the attachment to the metacarpal bone (with its resulting prismatic form) of the muscles which correspond to those which are attached to the metacarpal bone of the little finger; and (d.) by the position of its metacarpal bone in the metacarpal range.

Opposed to this view is the one fact, that the so called metacarpal bone of the thumb is developed like a phalanx, having its epiphysis at the proximal end, while the other metacarpal bones have their epiphyses at the distal end. The discussion stood there, the difficulty being to say what value should be given to the developmental fact. There might be some special reason for the changed position of the epiphysis, although it would be difficult indeed to suggest any such reason, either on the longitudinal growth, or on the elasticity, theories of the use of epiphyses, especially

* I have been in the habit of pointing out the interesting fact, that the relative length of the digits on the human hand indicates the order in which the digits disappear in the downward progression from the five to the one toed mammal,—the internal disappearing first, next the external or fifth, next the index or second, lastly the ring or fourth,—the extreme digit disappearing on alternate sides, beginning on the inner, until the middle digit alone remains in the foot of the horse, as demonstrated by Professor Owen in his work "On the Nature of Limbs." Nor need this correspondence be regarded as a mere coincidence, when we consider the relative function of the digits in an ordinary five-toed limb. The exceptions presented to this, in the human foot, and in the lateral toes of the pinnigrade carnivora, are special adaptive modifications of certain digits.

in the case of the great toe, the distal end of whose metatarsal bone is the most developed. Comparative anatomy, however, sets this question at rest. I find that the distal position of an epiphysis on a metacarpal and metatarsal bone, and its proximal position on the phalanges, belongs to other mammalia as well as to man, and is adhered to notwithstanding the most varied proportionate size which the two kinds of bones, or their two ends, attain.* In the horse and ruminant, in which the great metacarpals and metatarsals attain enormous size, they have still only one epiphysis, while the radius and humerus have an epiphysis at both ends; and the metacarpals and metatarsals have their epiphysis distal, while on the phalanges it is proximal. The short and flat thigh-bone of the seal has epiphyses at both ends, while the greatly elongated metacarpals, metatarsals, and phalanges, have but one epiphysis each, placed as on the corresponding bones in the human hand and foot. A still more striking example of this law is presented in the development of the rudimentary metacarpals and metatarsals of the horse; the large upper ends of which, serving an articular function, are developed without an epiphysis, the epiphysis being kept for the little "button" which terminates the tapering lower end. The more we compare the muscles of the two limbs, and in the limbs of different animals, the less dependence do we place on their attachments for the determination of homology; and the above facts in comparative osteogeny show, that the position of the epiphysis is decisive in establishing the view that the bone which is wanting in the human thumb and great toe, and in the internal digit of other five-toed mammals, is the metacarpal and metatarsal, although custom and convenience lead us to apply these terms to the bone which homologically is the proximal phalanx.

* The cetacea present an exception to the mammalian law of three phalanges, in the occurrence of more than three in the longer digits of the paddle, making an approach in this respect to the digital type of the fish. I have observed that the cetacean digits present another exception in the presence of epiphyses at both ends of each phalanx, and also at both ends of the metacarpal bones. In contrast with this, we have the great distal phalanx of the horse, ruminant, pig, and some others, developed without an epiphysis, while in man it is present on the distal phalanx as well.



Extract from Report of the Royal Commission (consisting of Professor LYON PLAYFAIR, C.B., Professor HUXLEY, and Lieut.-Colonel MAXWELL) on the Operation of the Acts relating to Trawling for Herring on the Coasts of Scotland.

Natural History of the Herring.

Before proceeding to sum up our conclusions from the preceding inquiries, it will aid us to take a general survey of the natural history of the herring, so far as it relates to the practical questions now under consideration.

The herring is found under four different conditions: 1st, *Fry* or *Sill*; 2d, *Maties* or *Fat Herring*; 3d, *Full Herring*; 4th, *Shotten* or *Spent Herring*.

The first term is applicable to all herring which are not larger than sprats, or, in other words, are under five or six inches in length. The milt and roe in fish of this size are so small as to be discoverable only by careful dissection.

The fry pass imperceptibly into *Maties*, which may have any length from six inches to thirteen, the last admeasurement being the extreme length of any British herring which has come under our notice. Internally, a matie is characterised by two peculiarities: the one being the great quantity of fat deposited about the alimentary canal; the other the small size of the roes or milts, which never quite fill the abdominal cavity, and, in herrings under ten inches, rarely exceed two or three inches in length, though they are always readily discernible.

A *Full herring* is one in which the milt or the roe is fully developed, so as to occupy the whole of the abdominal cavity, except the small space filled by the intestine; while the fat around the intestine has disappeared, having in all probability been applied to the nutrition of the reproductive organs.

The smallest full herring which has come under our own observation was a female, measuring $10\frac{1}{8}$ inches in extreme length, taken in Loch Fyne; but we have been assured by very competent and trustworthy informants, that full herrings not more than seven or eight inches long have been taken at several points of the west coast of Scotland. The largest British full herring which we have had under our direct observation was $12\frac{5}{16}$ inches long; but Mr Gibson, Fishery Officer at Broadford, Skye, through whose hands immense numbers of herrings passed when he was in the employment of the late Mr Methuen of Leith, assured us that it is by no means an unfrequent occurrence to find fish 14 or 15 inches long among the Orkney herrings, and that he once measured one which had attained the surprising length of 17 inches.*

* Mr Methuen himself states that Iceland herrings are generally 17 inches long.

We have measured two maties from Labrador, respectively $13\frac{1}{2}$ and $13\frac{2}{3}$ inches long; while Mr Mitchell, in his valuable essay on the herring, states that the Norwegians have a considerable fishery at Stadtland, where they generally get very large herrings, few being under a foot in length, and many 15 inches.

Hence it is quite safe to assume that adult full herrings may vary in length from 10 inches to 15, while it is possible that they may vary from 7 inches to 17 inches. There is nothing really remarkable about this, as other fish vary in dimensions in their adult state to fully the same extent,—a consequence of their continuing to grow long after they have reached the age at which they are capable of propagating their kind.

It has been frequently asserted, and evidence has been repeatedly laid before us to the effect, that particular lochs on the west coast of Scotland, such as Loch Broom and Loch Hourne, are inhabited only by herrings of a size smaller than the average; but, on the other hand, equally trustworthy evidence has directly negatived these assertions, the witnesses on this side affirming that the herrings of these lochs differ in no appreciable respect from those met with in other localities. We can give no opinion on this subject, as the time at our disposal was necessarily employed in investigating matters more directly relevant to the objects of the Commission.

When the herring first attain the full condition, the roe or the milt, as the case may be, is firm and hard; and pressure upon the belly of the fish will not give rise to the extrusion of ova, or seminal fluid, from the apertures of the reproductive organs. But the texture of the latter rapidly becomes softer, and soon acquires a semi-fluid consistency, so that the slightest pressure causes the reproductive elements to pass out of the body.

If a female in this state be squeezed, and the ova received into a vessel full of sea-water, they will all fall to the bottom; and the viscid substance, with which the ova are coated, causing them to adhere together, forms a thick cake, which, after a few hours' rest, constitutes a dense consistent mass, so firmly adherent to the bottom of the vessel that the latter may be suddenly inverted without detaching the ova. Indeed, to separate these either from one another or from the body to which they adhere, demands the exertion of a certain force, which as often ends in the destruction of the eggs as in their detachment.

It would seem as if all the superfluous nourishment in the body of the herring were applied to supply the rapid increase of the reproductive organs; for *shotten*, or *spent herrings*, so called because their roe and milt are completely emptied of their contents, not only have no fat about their intestines, but even their muscular substance is almost devoid of fat, and is consequently harsh, dry,

and insipid when cooked. They are distinguished from maties not only by this circumstance, but by the fact that the roe or milt, though greatly shrunken, may be restored to nearly the size of those of a full herring by inflating them with air, whereas the reproductive organs of a matie cannot be enlarged in any such manner.

It is extremely difficult to obtain any satisfactory evidence as to the length of time which the herring requires to pass from the embryonic to the adult or full condition. Of the fishermen who gave any opinion on this subject, some considered that a herring takes three, and others that it requires seven years, to attain the full or spawning condition; others frankly admit that they knew nothing about the matter; and it was not difficult, by a little cross examination, to satisfy ourselves that they were all really in this condition, however strongly they might hold by their triennial or their septennial theories. Mr Yarrell and Mr Mitchell suppose, with more reason, that herring attain to full size and maturity in about eighteen months.

It does not appear, however, that there is any good evidence against the supposition that the herring reaches its spawning condition in one year. There is much reason to believe that the eggs are hatched in, at most, from two to three weeks after deposition, and that in six or seven weeks more (that is, in, at most, ten weeks from the time of laying the egg), the young have attained three inches in length. Now it has been ascertained that a young smolt may leave a river and return to it again in a couple of months, increased in bulk eight or ten fold; and as a herring lives on very much the same food as a smolt, it appears possible that it should increase in the same rapid ratio. Under these circumstances, nine months would be ample time for it to enlarge from 3 to 10 or 11 inches in length. It may be fairly argued, however, that it is not very safe to reason analogically from the rate of growth of one species of fish to that of another; and it may be well to leave the question, whether the herring attains its maturity in twelve, fifteen, or eighteen months, open, in the tolerably firm assurance that the period last named is the maximum.

Yarrell, Valenciennes, and all the best authorities upon the herring, agree that the account of the migrations of the herring from and to the seas within the Arctic circle, promulgated by Anderson and Tennant, is wholly devoid of evidence. The herring fry frequent the lochs and shallows of the sea upon the British coasts in shoals, sometimes by themselves, at other times mingled with sprats, feeding upon the minute crustacea which throng the waters, and gradually growing into maties. Whether they betake themselves into deeper water on assuming the latter condition, or merely pass from a gregarious into a solitary state, does not

appear to be clearly made out. But the large shoals of herrings which appear upon our coasts in the early summer and in the late autumn months, and which seem to come from deeper into shallower water, are at first either entirely composed of maties, or contain a very large proportion of them.

The maties feed, develop their reproductive organs, and become full herring in the course of about three or four months, the full herring appearing, at first, only scattered here and there among the shoals, but gradually increasing in number, until they largely preponderate over the maties, or almost entirely constitute the shoal. The herrings then frequently aggregate for about a fortnight, in most prodigious numbers, in particular localities, like the Banks of Ballantrae, and the Traith or Fluke Hole, which are suitable, from the character of the bottom or from other circumstances, for the reception of their ova. Here they lie in tiers, covering square miles of sea bottom, and so close to the ground that the fishermen have to practise a peculiar mode of fishing in order to take them; while every net and line used in the fishing is thickly covered with the adhesive spawn which they are busily engaged in shedding. So intent are the fish on this great necessity of their existence, that they are not easily driven from their spawning ground; but when once their object has been attained, and they have become *spent fish*, the shoal rapidly disappears,—the universal and very probable opinion of fishermen being, that they withdraw into deep water, at no great distance from the coast. There is no positive evidence as to the ultimate fate of the spent herrings; but there is much to be said in favour of the current belief, that after a sojourn of greater or less duration in deep water, they return as *maties* to the shallows and lochs, there to run through the same changes as before, passing a second time from the condition of *matie* to full herring, and from *full* to *spent*. Of the manner in which the change from the *spent* to the second *full* state is effected we know nothing, nor have we any information respecting the number of times which one and the same herring may run through the cycle. However, the enemies of the fish are too numerous and too active to allow us to suppose the existence of any one individual to be prolonged beyond two or three reproductive epochs. It is probable that the difference of age, whatever it may be, is sufficient to account, in great measure, for the varying size of adult herrings.

Singularly contradictory statements are to be met with respecting the spawning season of the herring. We have obtained a very large body of valuable evidence upon this subject, derived partly from the examination of fishermen, and of others conversant with the herring fishery; partly from the inspection of the accurate

records kept by the Fishery Officers at different stations,* and partly from other sources; and our clear conclusion from all this evidence is, that the herring spawns at two seasons of the year, viz., in the spring and in the autumn. We have hitherto met with no case of full or spawning herring being found in any locality during what may be termed the solstitial months, namely, June and December; and it would appear that such herring are never (or very rarely) taken in May or the early part of July, in the latter part of November or the early part of January. But a spring spawning certainly occurs in the latter part of January, in February, in March, and in April; and an autumn spawning in the latter part of July, in August, September, October, and even as late as November. Taking all parts of the British coast together, February and March are the great months for the spring spawning, and August and September for the autumn spawning.

It is not at all likely that the same fish spawn twice in the year; on the contrary, the spring and the autumn shoals are probably perfectly distinct; and if the herring, according to the hypothesis advanced above, comes to maturity in a year, the shoals of each spawning season would be the fry of the twelve-month before.

However, no direct evidence can be adduced in favour of this supposition; and it would be extremely difficult to obtain such evidence.

The food of the herring consists of crustacea, varying in size from microscopic dimensions to those of a shrimp, and of small fish, particularly sand eels. While in the matie condition, they feed voraciously, and not unfrequently their stomachs are found immensely distended with crustacea and sand eels, in a more or less digested condition.

Herring thus abundantly fed are apt to have all their tissues so permeated with fat that they will not cure well, and their flesh is liable to break when handled. The Scotch fishermen style such as these "gut-pock" herring,† and consider them of very inferior quality.

The Possible Effects of Legislation on the Breed of Herring.

The herring is a notoriously variable fish, appearing in the most prodigious shoals, year after year, at a given point of the coast, and then suddenly diminishing in number, or even utterly

* The intelligent Fishery Officers might be made highly useful by keeping natural history registers on a systematic plan, and results important both to science and practice might thus soon be obtained.

† These seem to be the "Harengs à la bourse," or "Harengs aboutifs," of the French fishermen.

vanishing for a long period. The chief reason of these variations is probably to be sought in the variation, 1st, of the quantity of food of the fish; 2d, in the number and force of the destructive agencies at work upon it.

Any circumstance which increases or decreases the quantity of crustacea and of sand eels, for example, must influence in a direct ratio the chances of existence of a multitude of herrings; while any condition which increases or decreases the number or the activity of the enemies of the herring, must influence those chances in an inverse ratio.

Leaving out of consideration the animals which prey upon herring fry, and their mere accidental and unimportant enemies, their great destroyers are,—

a. *Fish.*

Cod, ling, coal-fish, hake, conger, dog-fish.

b. *Birds.*

Gulls and gannets.

c. *Marine Mammals.*

Porpoises and other cetacea.

d. *Man.*

The following considerations may help us to form an estimate of the relative importance of these different destructive agencies:—

In 1861, there were in Scotland, and that part of England over which the Fishery Board have jurisdiction, 42,751 fishermen and boys engaged in the herring fishery. The total take of the year would give about 20,000 herrings for each of these persons, or near upon 900,000,000 for the whole.

It seems, and indeed is, a most marvellous drain upon the power of multiplication of one animal, to destroy in one year a multitude exceeding in number the whole human population of the globe. But it may be readily demonstrated that this vast number sinks into insignificance if compared with the total destruction effected by agencies over which man has no control whatsoever.

Consider the destruction of large herring by cod and ling alone. It is a very common thing to find a cod-fish with six or seven large herrings in his stomach, of which not one has remained long enough to be digested. If, in order to be safe, we allow a cod-fish only two herrings *per diem*, and let him feed on herrings for

Herring. Days.

only seven months of the year, then $2 \times 210 = 420$ herring as his allowance during that time; and fifty cod-fish will equal one fisherman in destructive power. But the quantity of cod and ling taken in 1861, and registered by the Fishery Board, was over 80,000 cwt. On an average, 30 cod-fish go to 1 cwt. of the dried fish. Hence, at least, 2,400,000 cod-fish were caught in 1861.

But if 50 cod-fish equal 1 fisherman, 2,400,000 will equal 48,000 fishermen. In other words, the cod and ling caught on the Scotch coasts in 1861, if they had been left in the water, would have caught as many herring as a number of fishermen *equal to all those in Scotland, and six thousand more*, in the same year; and as the cod and ling caught were certainly not one tithe part of those left behind, we may fairly estimate the destruction of herring, by these voracious fish alone, as at least ten times as great as that effected by all the fishermen put together.

When it is further considered, that the conger and the dog-fish probably do as much mischief as the cod and ling, that the gulls and the gannets slay their millions, and that the porpoises and grampuses destroy additional untold multitudes, it will probably be thought no exaggerated under-estimate if we assume that our fishery operations, extensive as they are, do not effect 5 per cent. of the total destruction of maties and full herring that takes place every year. And when it is further considered, that sea-trout and innumerable other fish prey upon the herring fry, and that flat fish of all kinds resort in immense numbers to the spawning grounds of the herring, to prey upon the freshly-deposited ova, it would seem, as we have said, that the influence of man, whether conservative or destructive, upon herrings must be absolutely inappreciable; and, under these circumstances, it seems almost unavoidable that great fluctuations, wholly beyond human control, should occur in the abundance of herring in different years.

If the herrings in any given year multiply with great rapidity, owing to favourable circumstances, their enemies also will multiply in consequence of their better and more abundant supply of food; and if, as must sometimes happen, the ratio of multiplication of the enemies is greater than that of the herring, the latter will, in that or the succeeding year, be vastly diminished. But the very diminution of the herring necessarily tends to starve down the fish which destroy them, and to throw them open, in a weakened state, to the attacks of *their* enemies, who, by the same law, have necessarily multiplied in consequence of *their* multiplication. By such a concurrence of causes, the herring, relieved from their oppressors, will in a year or two appear again in immense numbers; and so the alternations of prosperity, over-production, and panic in the trade which they originate, will occur with as much regularity as if the herring were manufacturers.

CONCLUSIONS.

Having in the previous part of the Report given our reasons for the conclusions to which we have come, in regard to the sub-

jects of our investigations, it may now be useful to make a brief summary of the results :—

1. Drift-net fishing has been long pursued in this country, as the chief method for the capture of herring.
2. Drift-nets must have a mesh of not less than one inch from knot to knot (48 Geo. III., c. 110, s. 12). In consequence of this regulation, herring fry cannot be caught by this system of fishing.
3. The amount of capital required by the crew of a drift-net boat is considerable. The price of the boat varies, according to its size, from L.20 to L.100 or upwards, and the value of the nets may be equal to that of the boat.
4. The system of seining (improperly called 'trawling') for herring was introduced on the West Coast in 1838, but did not become general till 1846. Its operations are chiefly confined to Loch Fyne and its neighbourhood.
5. The system of seining requires much less capital than that of fishing by drift nets. The cost of each trawl skiff is about L.15, two skiffs being used in fishing, and the cost of the seine net is from L.15 to L.20.
6. The practice of using the seine net for herring was rendered illegal in 1851, by the Act 14 and 15 Vict., c. 26.
7. Under special circumstances, the risk of a rupture of the peace being imminent, the Government, but not at the instigation of the Fishery Board, obtained in 1860 increased powers for the repression of trawling by the Act of 23 and 24 Vict., c. 92, and still further powers in 1861, by the Act 24 and 25 Vict., c. 72.
8. These Acts have been effective as means of repression, and seining for herring is scarcely now practised on the West Coast of Scotland.
9. The Act of 1860 established a close time, during which herring are not allowed to be taken. This close time is limited to the West Coast of Scotland, and is fixed from the 1st January to 31st May as far as Point Ardnamurchan, and from thence to Cape Wrath from 1st January to 20th May.
10. There is no close time on the East Coast of Scotland; but the Fishery Board, by the Act of 1860, have power to introduce one,—a power which we recommend should be withdrawn.
11. The curers of herring and the drift-net fishermen approve of the operation of these repressive Acts, asserting that the method of fishing with the seine destroyed the supply

of herring, by scaring the shoals of fish; that immature fish were captured by it; that the spawn beds were injured; that the fish caught by the seine were unfit for curing; and that the seiners or 'trawlers' injured the fishing-gear of the drift-net men.

12. The main question involved in these allegations is the following: Is it an ascertained fact that the system of trawling has injured the fishery of Loch Fyne and its neighbourhood?
13. Our inquiries satisfy us that the fishery of Loch Fyne has suffered no diminution by the operations of the trawlers, but that, on the contrary, it is a steadily progressive fishery, when the periods of comparison are made sufficiently long to correct the annual fluctuations, which are always considerable in this as in all other herring fisheries. The approximate gross take of herring in Loch Fyne, according to the returns of the Fishery Board, are as follows:—

General annual average take from 1833 to 1843, 18,994 barrels.			
"	"	"	1844 to 1848, 15,427 "
"	"	"	1849 to 1853, 19,149 "
"	"	"	1854 to 1858, 25,744 "
"	"	"	1859 to 1862, 42,165 "

14. This steady increase of the fishery, during the period when trawling was practised, cannot be ascribed to an augmentation in the number of drift-net boats; for these, on an average of the same years, with the exception of 1862, show no increase, while the square yards of netting employed remain also comparatively stationary. Hence we are forced to the conclusion, that there have been no grounds for the alarm that the fishery of Loch Fyne was being destroyed by the operations of the trawlers.
14. This result established for Loch Fyne is found to apply to the west coast of Scotland as a whole—viz., that there is a steady increase in the fishery during the periods when trawling was prosecuted, and since it has been abandoned.
15. The selected years of bad fishing, brought as proofs that trawling was destroying the fishery, have, when examined, no application to the question, as an equal number of years of quite as bad fishing are found in every decennial period before the system of trawling had been discovered.
16. The selection of the prosperous year of fishing, 1862, is equally fallacious as bearing on the question. It is only

an instance of the singular fluctuations of the herring fishery. In a period of nearly forty years, the two most productive years have been 1860 and 1862. In the first of these years the second Act of repression was just passed, but trawling was still actively carried on, and had been in more or less active operation for fifteen years. It was only effectually put down by the Act of 1861. The good returns of these two years prove beyond a doubt that trawling has not injured the fishery of Loch Fyne.

17. These conclusions as to the steady increase of the fishery in Loch Fyne, negative the assertion that the trawlers scared away the shoals of fish when entering the lochs.
18. The evidence laid before us, and the experience of the Irish, Norwegian, Newfoundland, and Labrador fisheries, prove that trawled fish, when properly handled, are fit for the purposes of the curer. The reason of their occasional inferiority for this purpose is the reckless mode of pursuing a system of fishing in the constant apprehension of being caught in the violation of the law.
19. Trawling for herring has been an important means of cheapening fish to the consumer, by the large and sudden takes, and has thrown into the market an abundant supply of wholesome fresh fish, at prices which enable the poor to enjoy them without having to come into competition with the curer.
20. It is this circumstance which, in our opinion, has produced the demand for repressive legislation, for the gains of the drift-net fishermen are much affected by the sudden and great captures of the trawler, who, working with less capital, and with a more productive kind of labour, is able to undersell the drift-net fisherman, and to derange the market for the curers. It is to be observed, that as facilities of locomotion and transport increase, the market for fresh fish becomes more important than that for salt fish, and the former suffers if any special protection be afforded to the latter. As regards Loch Fyne, the number of barrels of herrings sold fresh already considerably exceeds that cured. From 1844 to 1853, 55,247 barrels were cured, and 77,137 barrels were consumed in a fresh state. From 1854 to 1862, 129,000 barrels were cured, and 168,530 barrels consumed fresh.
21. The demand for repressive legislation is only another form of that which always arises when a new and more pro-

ductive form of labour presses inconveniently upon those who prosecute and have embarked their capital in the old and less productive form of labour.

22. Although recent legislation has been in logical sequence of that which has long prevailed, and may, in the absence of new inquiries, have been requisite to prevent disturbance of the peace, still, as it proceeds on the assumption that it is justifiable for the conservation of the breed of herring, we are bound to state that, in this point of view, the repressive Acts of 1851, 1860, and 1861, were altogether unnecessary; that they are essentially Acts for protecting class interests, and interfere with the invention and application of new and more productive modes of industry.
23. We are of opinion, that if any legislation had been requisite, it should have only been in the same direction as that applied to the herring fishery generally, viz., to a regulation of the size of the mesh of the seines, which were frequently used under the legal standard. We express no opinion as to the necessity of restricting the size of the mesh generally, as this question is not referred to us under our Commission.
24. If the seine fishing be again rendered legal, increased powers for regulating the police of the fisheries should be given to the Fishery Board.
25. A discretionary power should be vested in that Board to prohibit seining from being prosecuted in waters which are too narrow for that and drift-net fishing being peaceably carried on simultaneously. We are of opinion that Upper Loch Fyne, i.e., above Otter Spit, is too narrow for this purpose, and that the Kyles of Bute offer another instance in which it would be desirable to give the Fishery Board discretionary powers to prohibit the practice of seining, but merely as a question of police.
26. Although we have been unable to satisfy ourselves that a close time is of any advantage for the protection of the herring, still, as it is universally approved of in the district round the Firth of Clyde, we do not take upon ourselves to recommend, against the wish of those who are practically concerned in the fisheries, any alteration as regards this district. It is true that close time presses heavily on the long-line fishermen, who are unable to obtain herring for bait to catch white fish during the close time from the 1st January to the 31st May; but it would be possible in this locality to allow licensed boats to catch

fish for bait; and the general desire of all witnesses examined by us is, that the Board of Fishery should be allowed to license such boats under proper regulations. We make this suggestion, however, purely in deference to the state of public feeling in the district in question. The establishment of any system of licensing boats for the capture of bait would, in the long run, involve the taxation of the community for the support of the officers by whom this fishery is superintended, and a further tax on the long-line fishermen, who could obtain their bait more cheaply if they were allowed to procure it for themselves. We do not pretend that it is strictly justifiable to tax the community in general, or one particular branch of industry, to foster another; but in the prevalent state of public feeling about the Firth of Clyde, the method proposed is the only practicable means of alleviating the injustice of close time, as regards the persons engaged in the long-line fisheries.

27. We are of opinion that there is not the slightest necessity for retaining a close time in any part of the coast north-west of the Mull of Cantire. The advocates for a close time in the extensive coast line between this and Cape Wrath desire a period different for each locality, and for reasons entirely dependent on class interests, and having no relation whatever to the protection of the herring. We recommend, therefore, that the coast line from the Mull of Cantire to Cape Wrath should be made as free as the whole east coast of Scotland.
28. In coming to this conclusion, we have taken into consideration the various destructive agencies to which the herring is subject, and of which man is certainly the least destructive. The effect of the recent legislation has been to promote an increase of the major destructive agencies in the sea.
29. By prohibiting the use of herring for bait during the close period from 1st Jan. to 31st May, the white fish, like cod and ling, have been allowed to multiply. A single herring used for bait is employed to catch three of these fish, each of which, if left in the sea, would have devoured annually at least between four and five hundred herring.
30. The cod and ling actually caught and cured on the Scotch coasts in 1861 would, if left in the sea, have destroyed more herring than 48,000 fishermen. As only 42,751 fishermen and boys were engaged in fishing in that year, the magnitude of this destructive agency will be readily per-

ceived. The close time which diminishes the capture of such fish must necessarily prove destructive to the herring.

31. When we further consider that the cod and ling caught represent only a small proportion of those which remain in the sea, and when we recollect that these fish are by no means the most voracious for herring, but that the latter have to struggle for existence, not only with larger fish, but with birds, such as gulls and gannets, and with porpoises, and other cetacea, man, as an element of either conservation or destruction, must produce an inappreciable effect.
32. Under such circumstances, the herring fishery should not be trammelled with repressive Acts, calculated only to protect class interests, and to disturb in an unknown and possibly injurious manner the balance existing between the conservative and destructive agencies at work upon the herring. If legislation could regulate the appetites of cod, conger and porpoise, it might be useful to pass laws regarding them ; but to prevent fishermen from catching their poor one or two per cent. of herring in any way they please, when the other 98 per cent., subject to destructive agencies, are poached in all sorts of unrecognised piscine methods, seems a wasteful employment of the force of law.
33. We conclude by expressing our strong conviction that the recent legislation on the subject of the herring fishery has unnecessarily restricted the operations of fishermen, —has repressed invention, by prohibiting new and more productive forms of labour,—is calculated to be destructive, rather than conservative, in relation to the future supply of herring ; and although it may be beneficial to certain class interests, is prejudicial to the consumer of fish, and to the public generally.

The Place and Power of Natural History in Colonisation ; with special reference to Otago (New Zealand).* By W. LAUDER LINDSAY, M.D., F.R.S. Edin., F.L.S. and F.R.G.S. London, &c. (Concluded from last Number.)

Geology of the Otago Lignites.

Nearly as many queries have been put to me regarding *coal* as regarding *gold*. What do I think, I am asked, of the Clutha coal, the Saddlehill coal, the Tokomairiro coal, and others of the so-called coals of Otago? In reply, I regret I cannot regard any of your so-called coal, *which I have seen*, in the same favourable light that Government and the settlers generally appear to do. I say so now, and frankly, because I fear great disappointment, and, very probably, great losses in speculation may be the result of exaggerated or erroneous notions of the value of the coals referred to. There are questions connected with the commercial value of your coals—(or let me call them by what appears to me to be their proper name, *Lignites*—I prefer using the true designation, for reasons immediately to be explained)—with which, at present, I have nothing to do; such are the distances of the respective coal fields or workings from Dunedin or other market, the difficulty and cost of transport, the scarcity and expensiveness of labour, and so forth,—all of which, however, are matters of moment to speculators and consumers. But I would enter on certain brief explanations as to the geological position and characters of your lignites. Without going into the strict scientific definition of “what is coal”—a theme of much greater intricacy than you may suppose, and the subject of one of the most interesting suits that has been tried before the Court of Session in Scotland of late years—the celebrated Torbanehill mineral case—it is enough for present purposes to say that the substance we at home call Coal, belongs to, or is contained in, a particular formation or system of rocks called by geologists the Carboniferous, or coal-bearing, system. The position and the components of this series of strata are well defined; geologists know precisely the rocks above and below. The character of the fossils especially it is, vegetable or animal, which a rock contains—a fern frond, a fish-tooth, a shell,—which enables the geologist with certainty to identify strata, and at once to fix the system to which they belong—their place in the geological series or chain—their comparative chronology. And it is fortunate we possess

* Extracts from a Lecture prepared for, and at the request of, the “Young Men’s Christian Association” of Dunedin (Otago, New Zealand). Dunedin, January 1862.

such an infallible, or at all events comparatively accurate, index ; for the mere mineral characters are often utterly insufficient. For instance, there is a red sandstone above and another below the Coal Measures, the one called the *New Red* and the other the *Old Red Sandstone*, in reference to their comparative ages,—alike in regard to mineral characters, that is as to colour, composition, and texture. The novice cannot tell the one from the other ; but the palæontologist—the expert who is conversant with the fossils which characterise the earth's strata—can at once say which is which. Knowing that the geological position of the Coal Measures is between these two series of rocks, the man of science would never be so stupid, nor so rash, as to be at the trouble and expense of sinking coal-pits through the Old Red Sandstone in the hope of finding coal ; while he would do so through the New Red with every probability of success. But, look at the position of the landowner or coal speculator ignorant of these elementary facts in geology. He may ruin himself by sinking shafts through the Old Red Sandstone, for a mineral which *cannot* be found below it ; or he may lose a fortune should he mistake the New Red for the Old Red Sandstone, by refraining from digging where coal may with every probability be found. This is no hypothetical case ; I have known landowners ruining and stultifying themselves by boring or sinking shafts for coal through the Old Red Sandstone on the Perthshire base of the Ochils in Scotland. I have already quoted the familiar adage—"All is not gold that glitters." Neither, may it be equally truly added, is every rock that is black coal ! There is no lack of black rocks in the earth's crust ; many shales are sufficiently black and carbonaceous: they contain more or less vegetable matter, yet are they not coal, nor do they belong to the Carboniferous system. The Silurian slates, for example, are frequently very black and carbonaceous, quite as much so as many shales of the true coal measures ; they are so where I have examined them in the neighbourhood of Dumfries. Now the Silurian system is inferior in the geological scale to the Old Red Sandstone ; and if no coal is to be found below the latter, still less will it be found below the former. Led astray, however, by the coal-like aspect of such shales—having confidence in their own judgment and cunning, ignorant altogether of geology, too parsimonious to spend a few guineas on a survey and report from a mining geologist—I have heard of Dumfriesshire lairds ruining themselves by expensive borings through these same Silurian slates. The late Mr Rose, consulting geologist, Edinburgh, I have heard tell many stories of the same kind ; sufficiently numerous instances occur, indeed, in the experience of all practical geologists.* Such cases, indeed, are the

* Let me here cite the testimony of what is at once the most recent and (according to the "Edinburgh New Philosophical Journal" for Oct. 1862, p.

commonest illustrations of the *loss of money by geological ignorance*. But not only are such cases frequent *at home*; they would appear to be equally so much nearer your own shores; so notoriously so indeed as to have found the following record in the pages of a popular topographical handbook (Fairfax's "Handbook to Australasia," 1857, p. 14, Victoria):—"Considerable sums, which might have been saved by a very small amount of geological knowledge and investigation, have been injudiciously and fruitlessly expended, both in the Cape Patterson, Cape Otway, and Barrabool Hills districts, in searching for workable seams of coal." True coal, then—the coal we use as fuel in Scotland and England—belongs to the "Carboniferous system" of geologists. A limited amount of coal, whether properly or improperly so-called I cannot stop here to inquire, occurs, *inter alia*, in the Permian, Triassic, Jurassic, and Cretaceous systems, all of which are superior in the geological scale to the Carboniferous system. Another substance called *Brown Coal* or *Lignite*—usually the latter—is peculiar to the Tertiary formation, a formation still more recent, still higher in

290) "the best Manual of Geology in the English language"—"The Student's Manual of Geology, by J. Beete Jukes, F.R.S., Director of the Geological Survey of Ireland: Edinburgh, 1862." "I have elsewhere stated my belief," says Mr Jukes, "that the amount of money fruitlessly expended in a ridiculous search after Coal, even within my own experience, would have paid the entire cost of the Government Geological Survey of the United Kingdom. It is a curious perversity of the human mind, that men prefer to take the advice of those whose interest it is to get them to spend money, rather than the warnings of those who can have no interest in inducing them not to spend it. . . . Within my own experience large sums of money have been absolutely thrown away, which the slightest acquaintance with Palæontology would have saved. I have known, even in the rich coal district of South Staffordshire, shafts continued down below the coal measures, deep into the Silurian shales, with crowds of fossils brought up in every bucket, and the sinker still expecting to find coal in beds below these Silurian fossils. I have known deep and expensive shafts sunk in beds too far above the coal measures for their ever being reached; and similar expensive shafts sunk in black shales and slates in the lower rocks far below the coal measures, where a pit might be sunk to the centre of the earth without ever meeting with coal. Nor are these fruitless enterprises a thing of the past. They are still going on in spite of the silent warnings of the fossils in the rocks around, and in spite of the loudly expressed warnings of the Geologists, who understand them, but who are supposed still to be vain theorists, and not to know so much as the 'practical man.' . . . All 'indications' are worthless as evidence of the presence of the 'Carboniferous formation,' except the occurrence of the 'Carboniferous fossils.' Even where the fossils occur, there may be no coal; but all sinking for coal in beds containing any other than the Carboniferous fossils is pure waste of labour and money. . . . The geologist . . . knows that not only do black and grey shales occur where there is no chance of coal being found; but that even thin seams of coal occur in formations where no coal worth working has ever been found in the British area or in western Europe. . . .

"The importance of the study of fossils . . . is not limited to the theoretical speculations or the philosophical conclusions that may be derived from them; for these, like many other scientific conclusions, may be coined into actual money, or money's worth, by their practical application."

the geological scale. It hardly occurs to any extent in Britain; but it is abundant in some parts of Germany, and in Iceland it constitutes the "Surturbrand," of which I have seen specimens very like those of the Otago lignites. To this class of rocks—to the *tertiary lignites*—belong, in my opinion, all the so-called *Otago coals* I have as yet seen. I have traced the formation of your lignite from the state simply of decayed wood to that of a black coal-like mineral, in the cliffs near the Coal Point at the mouth of the Clutha; and I have seen it in process of formation at the present day in a well sinking at a depth of some 40 or 50 feet in the swamp-clays of Dunedin. The origin of these lignites seems comparatively recent: they have not been subjected to the same enormous pressure as the Carboniferous system coals, occurring as the former do mostly on or near the surface. Nor do they seem composed of the same materials—at least in the same proportions: chemically, their constitution is not precisely the same; in heat-giving, gas-giving, and steam-giving power, they are likely to prove inferior to the true coals, with which they will not probably be able to compete.* Should all the coals of Otago belong to the family of the tertiary lignites, I fear you must be prepared to find their value inferior to that of the true coals of Scotland or England, or of coals belonging to the Carboniferous system, or to the other systems older than the Tertiary. I have been asked whether there is no chance of the *discovery of true coal in connection with any of the numerous Otago limestones*. This point I am not in a position to determine; it is an interesting problem for solution by your Provincial geologist. Should what is called by geologists the "Mountain Limestone" be found in Otago, true coal may reasonably be looked for. But the limestones I have met with, so far as I can at present venture an opinion, seem of a more recent age, and are not of a kind usually associated with coal.

Lignites you possess in abundance: for they are scattered in beds of greater or less extent very generally over the province. They occur in, and they are probably co-extensive with, the older tertiary auriferous "Drifts," which constitute one of the most marked features of Otago geology. One of the largest workings in the Dunedin district, perhaps in Otago, may be seen at Saddlehill, on Mr Jeffrey's property of Saddlehill Park, on the flank of the hill looking towards the Taieri Plain. Lignites crop out at various points on the slopes or base of this hill, or in its immediate

* This impression is so far borne out by the analyses, since my return home, by Dr Murray Thomson of Edinburgh, of a series of the Otago lignites. His analyses embraced 8 series of samples, including Saddlehill, Clutha, Abbott's Creek, and M'Coll's Creek, lignites. Among the results is the following—that they are comparatively poor both in *oil* and *gas*:—the highest proportion of *oil* per ton being 17 gallons (Saddlehill), and of *gas* 3981 cubic feet (M'Coll's Creek). I reserve details for future and separate publication.

neighbourhood; for instance, between Saddlehill and Scrogg's Hill on the Government Coal Reserve; in the gully in which M'Coll's and Taylor's Stations are situated, on the seaward aspect of the hill; on the farm of Fairfield, and in the adjoining Abbott's Creek. They are being worked also in the hills between the Tokomairiro Plain and the sea, on the slope of the range which looks towards the plain; and on the coast near the mouth of the Clutha. It is proposed further to work them on the Gold Fields—at Waitahuna and Wetherstone's Flats. My remarks must, I think, have sufficiently implied that I would recommend caution in any speculations as to working these Lignites—especially under the impression that they are true *Coals*. To be more specific, I would point to the circumstances under which the Clutha Coal-field is now being worked, and the Waitahuna and Wetherstone's Fields are proposed to be worked. In regard to the former I have to express a doubt whether the considerable expenditure of capital indicated by the railway and other appliances will be followed by adequate returns—especially seeing that lignite, apparently of the same kind and quality, in as great, perhaps greater, abundance, *is to be found within six to eight miles of Dunedin*, in what may be called the *Saddlehill Lignite Basin*. In regard to the lignite beds of Tuapeka Gold-field, I have to remark upon the inexpediency of “getting up” a company and proceeding to work a “coal-field” that has not been surveyed by competent persons. While on the gold-fields lately, I was told, on what I cannot but consider the best authority, that a company has been, or is being, formed at the diggings, for the purpose of working the lignite of Waitahuna and Wetherstone's Flats. Enormous profits are expected from its sale among the diggers. The whereabouts of the coal at Wetherstone's, at all events, and other matters therewith connected, seem to be kept a profound secret; the consequence whereof to me was, that though I searched diligently in the locality to which rumour pointed as its site, I failed in finding it. I was informed, further, that the extent of the field or seam is unknown; its value a matter of conjecture:—that neither locality has been visited by a geologist, nor reported upon by any competent authority. So far as I could learn from investigation on the spot, nobody seems to know anything precise regarding this so-called coal, which, as is the case with all latest discoveries, is said to be the finest coal yet found in New Zealand, but which, I fear, will prove neither better nor worse than the lignite of Saddlehill, the Clutha, and many other localities; and yet apparently steps are being taken to work this “coal.” This is surely a kind of speculation as unsafe as it is unsound. I am far from saying it will *necessarily* prove unsuccessful or unfortunate; but I do say it deserves to be,

if resting on no better basis than that described to me by the local authorities.

Museum of Local Natural History.

Having indicated, by a few illustrations, what fruits may be expected from a judicious cultivation or promotion of the study of the Natural Sciences,—how and to what extent these sciences, if rightly applied, may aid materially in the advancement of the State,—let me consider, in the second place, the most legitimate means—the best, the cheapest, and easiest methods of rendering these sciences available to the citizens at large, as well as of applying them to the specific requirements of Government or of individual settlers. The most obvious and easy means of recognising the place and power of natural history in your province, and of developing its applications to practical life, is the establishment of a *Museum of Local or Provincial Natural History*, or a collection of the products of this province, with such products only of other countries as will enable you to understand and appreciate the true position and value of the economical resources of your own province. Let such a Museum be essentially a collection of the rocks, minerals, plants, and animals of *Otago*;^{*} and let it possess an attached *Library*, containing such works on the natural history of *Otago* or of New Zealand, or such general works on natural history, as will enable the student duly to learn the lessons which the specimens displayed in the Museum-cases are ready to teach him. Of such a library there is great want, and of such works there is an astonishing dearth. I have not heard, for instance, of any person or corporation in the province possessing a copy of Dr Hooker's work on the "Flora of New Zealand," the most complete work on the subject yet published. I am told that the expense of such a work prevents its purchase by individuals; and this perhaps is a valid excuse for *individuals*. But, as a work of reference, it ought undoubtedly to have a place in some of your public libraries. Moreover, I know that the first British authorities in Botany—Sir William Hooker, and his equally distinguished son, Dr Joseph Hooker,—of Kew—are extremely desirous of issuing *cheap Colonial Botanical Manuals*, were there any demand for such works. Show that you desire

* I was, while in *Otago*, referred for the only good collections of *Otago* rocks, fossils, and minerals, to two private geologists at *Wellington*, and of *Otago* plants to a lady at *Sydney*; and I had the opportunity of examining a better suite of illustrations of *New Zealand* geology and mineralogy in *Sydney* (in the magnificent collection of the Rev. W. B. Clarke, the Government geologist of New South Wales) than in New Zealand itself! This is a state of things discreditable in the extreme to New Zealand generally, as well as to its respective provinces!

and require such works, and a supply would speedily meet the demand : offer the necessary funds for publication, and England will forthwith send you a Manual, which cannot fail to be useful to all who are connected with land—and who is not?—among your settlers. A Museum may appear to many of you a formidable undertaking, and one that may well be delayed, while so many institutions of more immediate importance must be established. The idea of a Museum may be associated in your minds with conceptions of a large, handsome, imposing structure, a well-paid and full staff of consequential officials, and endless glass cases ; it will thus appear *necessarily most expensive*. But, in a young colony like this, it need be nothing of the sort—at least in *nucleo* ; and all I would at present recommend is the formation of a *nucleus*—a step which may be taken with little trouble, and at little cost. The first desideratum is perhaps a few earnest men to take action in the matter—to organise a committee, to hold public meetings, to agitate by means of the press. Surely some of the energy which has lately been, and is now being, displayed by the office-bearers of the “ Young Men’s Christian Association,” might be spared in this direction. I think I can venture to assure them that many would flock to their standard were it only raised. I have met many settlers, of every class and in every part of the province I have visited, most favourable to the establishment of a Provincial Museum, and who, I feel assured, would aid it by every means in their power—by subscriptions, if necessary—had they only a guarantee that such an institution would be properly managed, by which I mean, principally, had it only a competent Head as Director or Curator. The feeling is very general (and I confess I think the feeling a just one), that a Provincial Museum should be established and maintained essentially by Government. But this is no excuse for inaction. It is not at all likely that Government will take the initiative in a matter of this kind, when it has so many more pressing and serious wants to relieve. Nor, it is possible, might Government, at any more favourable time, be disposed to patronise such an undertaking, unless under the *vis a tergo* of strong public feeling ; and, lastly, Government may repudiate all connection with such an establishment, and refer it to the individual or collective exertions of the colonists themselves. On what plea they might do so I know not. Education is the care of the State, and a Museum of this kind is just as much an *educational establishment* as any of your provincial schools. If it be not so, it ought to be so, and the fault must lie with its originators and managers. I can show, I think, presently, that a Museum may be made an educational establishment of a very high and important kind—one allied in character to our home universities. It seems to me desirable that the colonists should

actively take up the matter themselves—aided by Government if possible, without Government assistance if necessary. Let a few friends of the movement convene a meeting, and pass a resolution that the establishment of a Provincial Museum is desirable, and I doubt not for a moment the practical heads and active hands of the colonists will soon find means of carrying their views into effect. All that is at first wanted is a room for storage; by and by another room fitted up with presses, shelves and drawers would be added. I am told that there are buildings in Dunedin already existing which might easily afford the accommodation requisite; and I am given to understand, further, that such an appropriation or application of their space, or at least a portion thereof, would be quite consistent with the objects for which such buildings were erected. I refer to such a building as the *Athenæum*, which I think could not be devoted to a more legitimate use than that of a Provincial Museum, and which is at present, I am told, occupied as a merchant's warehouse or store-place. What arrangement should be made with the proprietors of such a building, I leave it to you to decide. My acquaintance with its history, and my local knowledge, are such that I can venture no suggestions on this head.

I look upon a competent *Curator* as the essential feature of a Museum, and, in your case, as its most expensive feature. Without a curator to organize its machinery, and to keep that machinery smoothly in regular and constant action, I have no great faith in the stability or permanent usefulness of a Provincial Museum. Without such an officer, I do not see how you can satisfactorily start it. You must have some competent authority to name and to classify specimens—some enthusiast, with the necessary leisure and interest in his work to encourage the collection of objects of natural history, and the study of such sciences as geology, mineralogy, botany, and zoology. Unless you have already in the colony some naturalist, quite competent, in the first place, and quite willing, in the second, to give his whole services, either gratuitously or for suitable remuneration, as curator of the nascent Museum, you must—if you wish your Museum to hold a real position of usefulness—procure a suitable person *from home*. However well qualified your colonial naturalists may be, I have met with nobody who could sacrifice the necessary time for so worthy an end; all are too much engrossed with their own private affairs. I believe a properly selected head of your Museum would be an immense acquisition to your province. He would, in the first place, be at his post to receive specimens sent up from the country, and I feel certain he would soon succeed in inducing settlers in all parts of the colony *to collect*. He would name and arrange these specimens in

the Museum. He would be ready to receive country settlers during their visits to town, and to instruct them practically how and what to collect; how to preserve specimens; to set them in the way of acquiring a thorough knowledge of the Natural Sciences, or of such of them as they might have a bias towards, or find it more particularly their interest to study. He would be at hand to solve difficulties, answer inquiries, and afford encouragement. He would be librarian as well as curator, and introduce students to the judicious use of the works in Natural History placed under his charge. In due time he might become a *Collector* as well as a *Curator*. His first duty would be to organize the Museum,—to provide or see provided, the requisite rooms, fittings, apparatus and staff. Thereafter, he might take occasional excursions through the province, making observations and collections in all departments of Natural History, or in special departments, sending or bringing back his collections to the Museum, which would always form his head-quarters. Such excursions would be of great importance, not only to himself, but to the promotion of scientific interests in the province. He would thus not only become thoroughly acquainted with the natural resources of the province, but he would personally come in contact with all classes of settlers, in whom he would not fail—in many cases at least—to inspire a certain love of, or respect for, Natural History, or an interest in the objects for which the Provincial Museum was founded, and for which he was exploring the country. The result would, in all probability, be, that in every tour or excursion he would raise up fresh friends of the Museum—friends who would aid it by their subscriptions, or their contributions of specimens—probably both. Another great advantage of such excursionizing would be, that he might fulfil the duties, or occupy the position, of a *Surveying Naturalist*, and that, in course of time, by such means, a thorough *Natural History Survey* of the province might be secured. Were the summer months, for example, devoted to such excursions, an assistant being left to manage the routine of Museum business, important results might in a few years be obtained. There is yet, however, other work for him, whereby you might still further “get value for your money.” In addition to being curator of, and collector for, the Museum, as well as Provincial Naturalist, he might be most useful as a *Lecturer* and *teacher*. He might give lectures, or institute classes, for different sections of the community, at different hours of the day, or at different seasons of the year, in connection with the Museum. With this view, a lecture room would fall to be added to the accommodation proper of the Museum. By the aid of the diagrams now so common and so cheap, and with all the resources of the Museum shelves at his command, science might

in his hands be made popular and most attractive. Such courses of instruction would, or should, be attended by, or at least open to, the *ladies* of the province; and, if they do not differ remarkably from their fair sisters at home, the Provincial naturalist would not find *them* his least anxious caterers, his least zealous supporters. Natural History has become a common and favourite feminine study at home—with what success the ferneries, the albums of sea-weeds, the baskets of shells, the marine aquavivaria of our ladies' fancy bazaars sufficiently testify. But he might disseminate his knowledge much more widely. While on his tours, he might lecture in district schoolhouses and churches, a boon which I venture to predict would be duly appreciated in country districts. I am assured on every hand, both in town and country, that lecturers are "at a premium," to use a commercial phrase: they are scarce, sometimes "not to be had," (to which circumstance, by the way, I must not forget, I probably owe my presence here this evening), and any addition to the lecturing staff of the province would be most acceptable. The newspapers are also open to such an officer; with a daily sheet at his command, how rapidly could he disseminate information—how easily announce the arrivals of specimens, intimate his excursions or lectures, and issue his suggestions. The Press, by and by, will be open to him in a more substantial and permanent form. You will doubtless, in due course, have your publishing houses in Dunedin, whereby he may issue his pamphlets, or his volumes, on subjects bearing on Provincial Natural History. It will naturally occur to some of you, as an objection to the appointment of such an officer, that such appointment would entail a very heavy expense. This, however, is not necessarily the case; though, even if it were, I think you would find the outlay well invested. At home there are many young men, enthusiasts in the pursuit of Natural History, who would be delighted with the privilege of occupying so novel and so rich a field, with so brilliant a prospect of gathering their laurels—of making substantial additions to science—with so promising a sphere of usefulness. Such youths are to be found as assistant-curators in our public Museums, our University and National Museums, and the experience they have there acquired would render them most useful public officers here. Some of this class, I should think, would be glad to come to terms. They would probably look more to their opportunities as men of science, than to mere salary or emoluments, though indubitably Naturalists *must live* as well as other people, and on something more substantial than enthusiasm. In addition to a guaranteed salary, which, considering his real usefulness and scientific status, should, in my opinion, be handsome, such an officer might be permitted by his employers, be they the Govern-

ment, the public, or the members of a private association, to add to his emoluments by the fees of lectures and classes, or of such other offices or duties as he could undertake without prejudice to the objects for which he was brought from home. I see no difficulty in securing a suitable person on suitable terms, which must, of course, be the subject of private arrangement. I am not sanguine enough to suppose that a Provincial Museum will ever become of the same real use to the present, as to the rising, generation. To the latter, I think it might be made one of your most important educational establishments, and therefore it is that I should like to see it systematically organized, thoroughly equipped, and firmly rooted as a State institution of recognized value; with a view to all which I will only be too happy if I can in any way forward its interests at home.

Geological Survey of Otago.

The fact that the Government has recently appointed a *Provincial Geologist*, who is expected shortly to enter upon his duties here, is without prejudice in any way to the remarks I have just made under the head of Museum. I am quite aware of the circumstance of the appointment having been made; but so far from such appointment interfering in any way with the schemes I have proposed, it appears to me most opportune, as affording a means whereby you may at once commence your Museum, organize its machinery, and carry it on, till you can procure a special curator. The assistance of so experienced a naturalist as Dr Hector, who has distinguished himself by his labours in the Palliser expedition across the Rocky Mountains of North America some years ago, could not fail to be valuable. I cannot refrain from stating here that I think it reflects high credit on Otago that she is the first province in New Zealand to have made arrangements for a systematic geological survey, a survey which should imply the expenditure, in my opinion, of at least £10,000; that is, including the cost of the proper publication of reports, maps, and drawings, which publication can only be carried out in Britain, and at necessarily great expense. And it seems to me doubly creditable that the suggestion regarding such a survey was made, and the arrangements carried out, considerably prior to the discovery of the gold fields of Tuapeka and Waitahuna; so that I presume it was from a firm faith in the usefulness of geological science in a practical sense that the Provincial Government passed its vote for this survey. I am glad to find that other provinces of New Zealand are following in your wake in this respect, and I doubt not *all* will sooner or later be encouraged by the results of *your* Geological Survey to do so. I know not how far the suggestion may meet with the approval



of your Government or Dr Hector; but I hope neither of them will be offended by the simple expression of individual opinion. It appears to me that Dr Hector's presence might legitimately be taken advantage of in establishing the nucleus of the mineralogical and geological sections of the Provincial Museum, as well as in the dissemination of a knowledge of local geology and mineralogy by the medium of lectures or classes. For such purposes advantage might be taken of the completion of the survey, of the winter season, or of periods of inclement weather, when the survey may be temporarily suspended. A properly named and classified collection, first, of the rocks and minerals of Dunedin, and by and by of those of other parts of the province, might be made. Whenever the funds would admit, I would strongly recommend the purchase of coloured geological diagrams, models of crystals, and other scientific educational apparatus, now easily to be had, as well as a series of selected named specimens—as standards—of rocks and minerals similar to those of Otago, and which throw light on the relations and uses of the latter; specimens, for instance, from the Trap districts of Scotland, the volcanic regions of Iceland or Italy, the Chalk districts of England, or the Tertiary formations of Germany and France. These are procurable, at a moderate expense, at such establishments as Professor Tennant's, in London; or Krantz and Co., of Bonn and Berlin. With the aid of such specimens, diagrams, and apparatus, as illustrations, and by means of field excursions round Dunedin, your Provincial geologist might teach practically, in a most attractive way, details of the sciences of geology and mineralogy, or the rudiments thereof, according to circumstances. Government would surely grant every facility in the way of accommodation and assistance for so desirable an end. I am told Dr Hector is daily expected. I am not at all aware whether any or what preparations have been made here for the prosecution of his survey: but it appears to me that the way may be so far paved for him; his labours so far facilitated and aided; his objects co-operated in. There is not one of you, probably, who may not in some way, however humble, promote Provincial geology by co-operating in the efforts of the Provincial geologist. You are at considerable expense in providing the staff and requisites for a geological survey; your being so shows the value you place upon the possession of the information such a survey is expected to collect or educe. It is surely your interest to get the best, the largest return for your outlay—to make the most of your engagement. You will best consult your own interests then by doing all that in you lies to further the aims and objects of Dr Hector. You may do so in various ways, but on one only of these will I touch. I would suggest that so soon as Dr Hector arrives, Government should insert in all the local newspapers an

intimation of his arrival, and thereafter announce, from time to time, the localities he is about to visit, calling upon settlers of every kind to note all peculiarities of rocks in their neighbourhood, and to collect specimens of rocks and minerals—all which should be produced in due time to Dr Hector. This would probably direct his attention to places and circumstances that might otherwise escape him. I have myself derived great benefit from collectors of "curiosities," which generally include a miscellany of rocks, minerals, fossils, shells, eggs, insects, and so forth. I have frequently seen specimens in such collections which at once arrested my attention. Their owners or collectors knew perhaps little or nothing of their value, but they could tell where they picked them up, and could lead me to the spot. In such a way some of the most interesting geological phenomena I have seen have come under my notice. These so-called "curiosities" were casually collected as mementoes, perhaps, of visits to localities beyond the beaten track of travellers, as playthings for children, or as mantelpiece ornaments. Were, however, the attention of the same collectors directed to the fact that such specimens might lead the Provincial geologist to discoveries, if not practically important, at least scientifically interesting, with this higher motive, and a more intelligent conception of the bearings or value of such specimens on the promotion of scientific research, they would become more careful observers, more diligent collectors, and ultimately many of them efficient co-operators, and learned geologists. Dr Hector will probably necessarily be engaged for some time in Dunedin preparing the groundwork of his survey, and Government would therefore, on or after his arrival, have ample time—especially as it now has at its command the inestimable advantages of a daily newspaper—to communicate with the settlers in the most remote parts of the province, and so set them on the alert as observers and collectors in Natural History. It is evident that the more the Provincial geologist sees and hears of the geology of your province—the greater the number of specimens presented to his notice—the more varied and abundant the information supplied,—the more valuable will be his survey report, and the better pleased you will be with the expenditure of your money. Let me hope, then, that, in the light of your own interests as well as in those of science, you will accord your heartiest assistance to the able naturalist you have been fortunate enough to secure as Provincial geologist.

Botanic Garden.

A second great desideratum in Dunedin is a *Botanic Garden*, whether regarded in a purely scientific light, or in that of a public

recreation ground, a promenade and place of resort.* At present I am not aware there is anything of the kind, while there appears no lack of suitable sites in the possession of the Government. I have found among the settlers throughout the province a strong feeling in favour of the establishment of a Botanic Garden; and so long as you possess men so admirably qualified by experience and tastes to act as committee men in promoting such an undertaking as Mr Matthews of Dunedin, Mr Martin of Saddlehill, Mr Bower of Anderson's Bay, and Mr Buchanan of the North-east Valley, you need have no hesitation in taking the initiative. Many of the remarks I have made under the head "Museum," apply equally to the Botanic Garden. It may appear to you a more extensive and expensive undertaking than even the Museum; visions of magnificent hothouses, greenhouses, and conservatories, of ornamental sheets of water and fountains, of terraces and parterres, will probably appear to those of my audience who have vivid imaginations. But none of these are necessary to begin with; they will all be added *ultimately*, I doubt not. A small piece of ground and a small staff would suffice at its first start. It is most desirable that the Botanic Garden should have a separate and competent Head as Director; but, should the expense of such an arrangement be found an insurmountable obstacle, the Curator of your Museum might act as director for a time. Indeed, should a more convenient building not offer itself, the Botanic Garden would form an excellent and appropriate site for a new Museum building. The latter would be rendered all the more attractive by the luxuriant foliage of, and the beautiful views from, the former. The first aim of such a garden should be to collect together, name, and classify, *specimens of all the plants of Otago*, so that a glance might give the stranger or the mere townsman a good idea of the general vegetation of the province. By and by, the collection might be made to include, in proportion as the garden space was extended, and the garden finances permitted, the plants of the adjacent provinces, of the North Island and Stewart's Island, of Tasmania and Australia, of the Chatham and other neighbouring islands. Thereafter, European plants might be added, if it were

* The experience and example of the Sydney Botanic Garden may be useful. It occupies one of the finest sites about Sydney, and is most tastefully laid out,—less, however, as a garden arranged purely for the purposes of science than as pleasure-grounds for the purposes of public recreation. Its attractions include an orchestra for the regimental band, which plays regularly on certain days of the week,—on which "band days" the garden becomes the most fashionable promenade in Sydney; and an aviary, with ponds for aquatic birds. But the scientific is not forgotten, for the garden possesses an excellent Botanical Library and Museum, as well as a Lecture-room, where the excellent Director, Mr Moore, gives public courses of Lectures on Botany. Amidst such environments, it would be surprising did such lectures fail to prove attractive to a considerable section at least of the general public.

deemed advisable ; but the garden ought to be *essentially representative and local*—a collection of *Otago plants, Otago products*. Secondly, it might become an excellent *Experimental Garden*, a place where, appropriately, properly conducted experiments might be instituted as to what foreign plants are suitable for growth in Otago ; what will flourish in its climate ; what might be acclimatised or naturalised, so as to become useful to the settler for his shrubberies or gardens, his plantations or hedges ; what trees may be made to supply the place of the monarchs of the virgin forests, which are fast disappearing under the axe and fire of the settler—under the insatiable requirements of advancing civilisation. Thirdly, the *Exchange of seeds* would probably form an important feature in the usefulness of your Botanic Garden. There are many of your native shrubs, such as the *Eurebias* and *Fuchsias*, which would make beautiful and acceptable additions to our shrubberies and gardens at home ; and I am sure that our nurserymen and seedsmen in Britain would gladly, in exchange for the seeds of your shrubs as seemed suitable for British cultivation, send to your garden the seeds of any plants usually cultivated in their gardens or forcing-houses. In connection with such exchange of seeds would be the intercommunication of information regarding the plants represented by the seeds—information mutually important, mutually desirable. Many of your Provincial gardens—your settlers' gardens, I mean—are quite Botanic gardens in miniature : they are carrying out on the small scale the aims which it should be the business of the Botanic Garden to carry out on a larger one. In the gardens of some individual settlers you may find excellent collections of the more interesting shrubs of Otago, of Tasmania, and Australia—shrubs which are being naturalized ; and of British or other plants, the seeds whereof have been received in exchange for those of Otago plants. Such gardens are those of Mr Matthews, of Dunedin ; Mr Shaw of Finegand, on the Clutha ; and the Rev. Mr Will of the Taeri. These are not solitary instances ; for the wants, which a Botanic garden would supply, are extensively felt.

University of New Zealand.

Not less important, as a means of promoting science in your province, than either a Museum or Botanic Garden, at the same time not as superseding these, would be the establishment of a University, or at all events at first the nucleus thereof. Such an institution is desirable on many other and more important grounds. A University in Dunedin might become not only the Provincial University, or the University of the Middle Island, but the *University of New Zealand*. I believe that New

Zealand will gradually, more and more than at present, be selected as the residence of our successful Australians, and East Indians. Its climate will be in request as one better suited to the physical training of youth than the climates of more tropical countries. Did good educational establishments, of the higher kinds especially, exist in the Middle Island of New Zealand, this would probably prove a great attraction, from the excellent opportunities offered for the education of youth, to many who might not otherwise have thought of emigrating, or of sending their sons or families hither. For many reasons, it appears to me the Middle Island will be preferred to the north as a place of residence, and a place of education. And further, it seems to me that the first of the New Zealand Provinces, which will establish a University, will, *ceteris paribus*, attract towards itself the classes of persons above referred to. I am glad to find the same view has occurred to the late Dr Arthur Thomson, in his "Story of New Zealand," the most recent and best work on New Zealand, with which I am acquainted. His remarks are so apposite and brief, that I make no apology here for introducing them. "*A University of the highest order is urgently required in New Zealand. Such an establishment would draw within its walls large numbers of the Australian aristocracy, as Anglo-Saxons born and reared on that continent are occasionally destitute of the bodily energy requisite for the full development of the mind. Two generations in Australia change the children of the broad-shouldered emigrants into a lithe race, more nervous than muscular. 'Sydney corn-stalks,' as the youths of that city are denominated, are no match in intellect against men brought up in colder countries. For this reason, Australian settlers will strive to give their children a few years' residence in climates more invigorating than that in which they were born; and it is already discovered that the best period for this is on the approach of maturity, when youth is rising into manhood, and the mind is developing itself for the future battle of life. Whichever province in New Zealand, therefore, can found a University, where youth can be taught as the youth of England are taught, will acquire a name and an influence in the Southern hemisphere, not to be measured by money or the figures of the statist.*" [Vol. ii., p. 230.] So far, at least, as the study of Natural History is concerned, I can scarcely conceive a more fitting site for an "Academe" than Dunedin. In the midst of magnificent scenery, with a rich and varied geological and botanical field before him, the student's natural advantages are great; and these advantages would be enhanced, had he access to Museums and Botanic Gardens, Libraries and Class-rooms, as already described. If a Museum or Botanic Garden appear to you necessarily expensive, I fear a

University will appear much more so, if you think of it as a massive pile, richly ornamented, its niches containing statues of the great and good, with endless lecture-rooms, a commodious library, and an imposing Senate Hall, two or three dozen learned Professors, and a venerable Principal. It is, however, far from necessary—it is not, perhaps, desirable—that you should at once establish a University complete in all its parts.* All I would recommend, so far as Natural History is concerned, is the appointment of two or three professors, who would be easily accommodated, and who might be most useful Government officers, independent altogether of their “chairs.” No separate buildings would be necessary at first. I would house the Professors in the Museum, though this would imply your making this building commodious. Your Provincial Geologist might be *Professor of a certain section of the Natural Sciences*, certainly of *Geology and Mineralogy*, with their economical applications. He might act as Government referee on all questions relating to mining and quarrying in their scientific aspects. The Director of your Botanic Garden might be your *Professor of Botany*, and perhaps also of *Zoology*, with their economical applications. He might also hold office as *Conservator of Forests*, or have a seat at a *Board of Woods and Forests* as professional referee. He would be a useful authority in all questions relating to the acclimatisation of plants or animals, agriculture and arboriculture,

* Let me indicate the University of *Sydney*, less as a model for imitation, than as a beacon of warning against dangers liable to be incurred. I have visited and greatly admired it *as a building*. But I think it has aimed at too much in the form of building, which is both too extensive and expensive for the requirements of the age and of the colony. The consequence of this error is, *inter alia*, that, contrasted with the size of the edifice, there is a miserable paucity of students; and throughout the colony there seems to exist a feeling of insecurity or want of confidence in the solidity or stability of this the Provincial University of New South Wales. While the building is on too large and expensive a scale for present requirements, the Professorial staff seems inadequate,—especially in so far as the Natural Sciences can be thereby represented. There is only *one* Professor of these sciences, the particular science taught being *Chemistry*. In a country which owes so much of its prosperity to its gold, its coal, its sandstone, there is *no Professor of Geology or Mineralogy*,—while New South Wales possesses at least one local Geologist eminently worthy of such a chair, the Rev. W. B. Clarke of Sydney, whose distinguished labours in connection with the discovery and development of the Australian gold-fields, I am happy to see, have recently been rewarded by a handsome Government grant or honorarium. Nor is there any Medical School in connection with the University, though there is a large general Hospital in Sydney, as well as large Lunatic Asylums at Tarban Creek and Parramatta in its vicinity. And yet all of these might exist *at a moderate expense, and without a separate University building at all!* It would have been desirable had the large expenditure of the Sydney University taken first the direction of the *essential* constituents of an efficient *Academe*,—*a staff of Teachers of celebrity*: the walls or buildings would gradually have followed as they were required and could be afforded.

in their scientific aspects. Another Professor could hardly be dispensed with; indeed his usefulness is, perhaps, more evident than in the case of the other two. I refer to a *Professor of Chemistry*, who might also be *Government Analyst and Assayer*. At present, I believe you send your gold for assay to Sydney; and you are dependent entirely on Australia or Britain for chemical reports on the percentage of iron in your iron sands or ochres, sulphur in your sulphur muds, lime in your limestones, gas or oil in your lignites. Would it not save time and trouble, delay and suspense, as well as expense, to have your analyst always at hand—always at your service. He would be ready to act as agricultural chemist, making analyses of soils for settlers. He would be a valuable referee in medico-legal cases, in judicial examinations in cases of poisoning for instance, and in similar cases where the chemical, would necessarily be the chief, evidence. He would act as a detector of adulterations in your foods and drinks—adulterations which are generally rife in such a state of society as exists in Dunedin at present. Even as a reporter on the quality of the water-supply proposed for your towns, he might be of signal service, especially in regard to indicating the proper materials for conveyance and storage. Such Professors might give systematic instruction in their respective sciences to all who desired to take advantage thereof, as well as more general and popular courses of lectures to the public at large. The contents of the Museum would afford abundance of suitable illustrations. All three Professors might, if necessary, occupy the same lecture room at different hours, or at different seasons, though there is no doubt separate and ample accommodation would be desirable. The Professor of Chemistry would, in addition, require a small Laboratory, the cost whereof would reside chiefly in its fittings, which, however, may be had at a very moderate outlay. Let us hope that ere many years elapse, “New Edinburgh” may possess in the Southern Hemisphere a University rivalling that of “Old Edinburgh” in the Northern; and if it does so, I will venture to predict it will become *the* University, not only of New Zealand, but of this (Southern) Hemisphere!

Teaching of Natural Science in Schools.

But, pending the establishment of a University, which must be gradual in its growth, there seems no reason why the existing educational machinery of the province should not be rendered available for bestowing upon the rising generation scientific tastes, if not acquirements. I would commend to the favourable consideration of your Education Board the propriety of introducing the teaching of at least the *rudiments* of Natural Science,—of such

sciences, especially, as Physiology, Zoology, Geology, and Botany, into your district schools, and especially into your High Schools. The means of doing so are now ample. Books and diagrams, named specimens, and working models, may be purchased at extremely moderate rates from educational publishers. What is called "Object-teaching"—the teaching of science by means of actual specimens—is now common in our most elementary schools at home. This method of teaching has the immense advantage that facts are indelibly graven on the mind, without any appreciable effort of thought or memory, the observative more than the reflective faculties being thus educated in a way most attractive and interesting to the pupil.

Natural History Society.

Much might be done mutually to diffuse a taste for natural history studies, by the establishment or formation of a *Natural History Society*. Meetings might be held periodically for the reading of papers relating to local natural history; specimens of rocks and minerals, plants, insects, shells, &c., might be exhibited; excursions might be planned; encouragement and assistance given by the more to the less experienced. Such societies are abundant at home, and are most serviceable, especially to youth. The "Natural History Society of Dunedin," or of Otago, might, in the first instance, be an offshoot from the "Young Men's Christian Association," and would probably, ere long, have an independent existence, and a distinct sphere of usefulness. The establishment of a Natural History Museum, Botanic Garden, and University, and the introduction of science-teaching in schools, will probably, each and all of them, give an impetus to the formation of societies of this kind; and this is an additional reason why no time should be lost in taking steps towards the carrying out these most desirable schemes. There is no lack of settlers with the requisite tastes and enthusiasm to form a solid nucleus, a good working committee; nor is there wanting the readiness to become members of a Natural History Society, were it only established. But the great difficulty here, as in all the schemes I have propounded, is *the start*; and here is another instance, added to the many that can be cited, of the value of the presence of competent Naturalists among you,—men who have their whole time to devote to the prosecution and promotion of Natural Science, whose interest it is pecuniarily to use their best efforts for its progress,—whose bias and tastes, whose habits and experience all tend in this direction, and who would be the soul and life of such a society—of any association, which might have for its object or aim the study or applications of Natural Science.

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Example of Victoria in its Encouragement of Natural History.

In connection with the schemes I have above advocated, I would ask you to look to the example set you by your near neighbour and fair sister Victoria. Consider what Melbourne has accomplished for the promotion of science during the short ten years of her existence. Visit her magnificent Botanic Gardens—her excellent Museums of Natural History, Geology, Mining, Agriculture, &c.—her University and her Royal Society—and see whether these do not afford proof of an enlightened recognition of the value of the Natural Sciences as a power in colonisation. What Melbourne has achieved, Otago may, with similar energy, aspire to accomplish. I am sanguine as to the future of your province. I believe that, independently altogether of the gold-fields, it possesses many elements of future greatness and distinction; that it has boundless resources, which it will take many years fully to develop and appreciate; that, prior to the discoveries of Gabriel Read, Otago was progressing “slowly but surely,” silently and unostentatiously, but substantially, in all that constitutes a healthy colonisation. But I believe, further, that though this satisfactory progress would have continued had there been no gold discovery in your province, one result of such discovery will be to render the said progress greatly more rapid. You will find the colony in one year in a position it would probably not have reached in ten or perhaps twenty. Much depends—though everything does not depend—on the *permanency* of your Gold Mining, the extent and productiveness of your gold-fields. To take the worst view of the question,—should the gold-fields fail, should they prove of very limited extent, there would probably result an immediate great commercial distress—a distress, however, that would probably affect your speculative Victorian brethren more than yourselves. Your country would remain much in the state in which the discovery of the gold-fields found it, with the exception that your settlers would have the advantages of additional population, additional roads and bridges, and other desirable results of the temporary influx of population. There is no reason, however, for supposing the gold-fields of Otago to be of very local character, or of very limited extent; while experience is daily showing they are abundantly productive. To what extent they will compete in the long-run with the Victorian gold-fields remains to be seen. That they are at present competing successfully therewith is a fact that can no longer be doubted. I have repeatedly been assured by gold-seekers—many of them of Californian as well as Australian experience—that the escort returns of no single gold-field in other countries, within the same space of time, have ever equalled those of Tuaepeka, and that Gabriel’s Gully has proved,

in proportion to its size, the richest gold-field ever discovered or worked in any part of the world. The present horizon of the colony, therefore, is bright and cloudless. Your land, your flocks and herds, are rising rapidly in value. Dunedin is being metamorphosed, at a Victorian rate of progress, from a village into a city. The stream of population is at the flood. This population brings with it large numbers of the educated classes of society; and these classes, let me hope, will not be long in experiencing the want, and demanding the supply, of many, if not all, of the institutions, the establishment whereof I have been recommending.

Conclusion.

Circumstances have compelled me altogether to omit the sections I had originally intended on such subjects as—

I. Under the head of the *Botany of Otago*—

a. The necessity that seems to exist for the careful *Conservation* of your *Forests*, and for the establishment of a *Board of Woods and Forests*.

b. The importance of investigating the nature of the *Tutu Poison*, and of discovering a preventive or remedy therefor.

c. The importance of a comparative study of the *British* (naturalised) and native *Grasses*, as fodder for sheep and cattle on your ranges and runs.

d. The desirability of experiment on the *Acclimatisation*, or introduction, of such of the *trees or shrubs of Tasmania, Australia, or Europe*, as might be found suitable as a protection or environment to your dwellings, or as a substitute for your Forest trees.

e. The development of the economical appliances of your *Flax fibre*.

f. The economical applications of a number of your commonest weeds; for instance, the *Lichens* of your rocks, which I have proved by experiment here are capable of yielding good dyes.

II. Under the head of the *Zoology* of Otago*—

a. The *Acclimatisation* of many of our *British fish, birds, and other animals*, viz.:—The introduction of the salmon and trout into your streams and rivers; of the grouse, partridge,

* Our ignorance at home of New Zealand zoology may be happily illustrated, in one of its phases, by the following apostrophe by the author of "The Pleasures of Hope:"—

"On Zealand's hills where tigers steal along,
And the dread Indian chants a dismal song," &c.

It so happens that the most striking feature of the very meagre *fauna* of New Zealand is the *entire absence of animals dangerous to man*,—a circumstance that goes far to render that country one of the safest of all the British colonies.

capercailzie, ptarmigan, and other of our game birds on your ranges; of our common British insectivorous birds generally.

b. The desirability of the establishment of an *Acclimatisation Society*, similar to those of Melbourne and Sydney, of Paris and London.*

III. Under the head of the *Meteorology of Otago*—

a. The importance of establishing throughout the province a chain of *Meteorological Stations*, in connection with a *central Provincial Observatory at Dunedin*, with a view to the collection of precise data, whereon to base conclusions as to the true character of the *climate of Otago*.

Contributions to Ornithology. By Sir W. JARDINE, Bart. No. V,

Acanthylis sabini (Gray). A specimen of this spine-tailed swift was procured by Captain Sabine (now Major-General Sabine) when upon duty on the west coast of Africa; and the first notice of it as a new species appeared in Griffith's edition of Baron Cuvier's "Animal Kingdom," part xvii., 1828, as *Chaetura sabini*, Gray, MSS.—"Bluish black, belly and rump white; Africa, Captain Sabine." The specimen thus referred to is now in the British Museum.

It was afterwards more fully described, and the dimensions given (1831), in Gray's "Zoological Miscellany," under the name of *White-Rumped Chaetura*, *Ch. bicolor*. Why the change of name we do not know.

When M. Duhaillu returned to America from his first expedition in Western Africa, his ornithological collections were described, and a list of all the species was given, by John Cassin in the Proceedings of the Academy of Natural Sciences, Philadelphia, and *Chaetura sabini* (Gray), with the synonyms applied to that bird, was printed with the list.

At a later period M. Duhaillu came to Europe, and his specimens of gorilla and other animals and birds were exhibited in the rooms of the Geographical Society of London. On visiting this collection, we saw that the swift named by Cassin as *Ch. sabini* was not identical with that bird, but had not at that time an opportunity of comparing them. On leaving Europe to resume his explorations in Africa, the remains of his collections were sent to Mr Stevens for disposal, and we then procured a specimen of the bird we had before partially seen.

From the first description in Griffith's "Animal Kingdom," the

* An excellent example has been set by the recent establishment of one in Auckland, New Zealand.

mistake was easily made, no measurements being given; but a comparison of the specimens leaves no doubt of their being distinct.

In the "Athenæum" of May 30th, "Dr Sclater pointed out the characters of a species of spine-tailed swift of the genus *Chaetura*, from West Africa, for which he proposed the name of *C. cassinii*." This may be Duchailu's bird, or any other; but we cannot recognise such notices as descriptions, or as any authority for a name. It has been the practice for many years for gentlemen thus to *announce new names* without anything more, and it has been the source of confusion and great multiplication of synonyms. We now give the characters of the two allied species, describing the old bird from a specimen in the museum of T. C. Eyton, Esq., who procured it from the refuse specimens of the once fine ornithological collection of the Zoological Society.

Acanthylis sabini (Gray), 1828.* Mus. British. T. C. Eyton.

Above, black with blue and violet reflections, rump and up. tail-coverts white, shafts of each feather black, tail-coverts as long as the rectrices without the spine. *Below*, greyish-black; belly, vent and under tail-coverts white, the latter long with narrow black shafts; tarsi and feet slender. Length to end of tail spines, 4; wing, 4.5.

Chaetura sabini (Gray), Cuv. Reg. An., ed. Griff. ii., No. xvii. p. 70. Hartlaub, Syst. Orn. W. Africa, p. 25.

C. bicolor (Gray), Zool. Miscel., p. 7.

Acanthylis bicolor (Strick.), Proc. Zool. Soc., 1844, p. 99.

A. sabini (Gray), Gen. B. Fol. Sp. 8. List B. Brit. Mus., p. 16.

Mull. Cab. Journ. Orn., 1855, p. 3.

Pallene leucopygia (Boie), Isis, 1844 (Auct. Hartl.)

P. sabini, Bonp. Consp. Avium, p. 64.

Hab., W. Africa: Sierra Leone (Sabine); Fernando Po (Fraser).

Acanthylis hartlaubi, Jard., June 1863. Mus. Brit. Jard. Philad. (Auct. Hartl.)

Above, black with slight greenish reflection; up. tail-coverts short, those next rump white, forming a narrow band, outer rectrices pale near the shaft. *Below*, chin greyish-white; throat, breast and sides grey; belly, vent and under tail-coverts white; tarsi and feet, strong. Length to end of tail spines, 4.3; wing, 6.

Chaetura sabini (Cassin), Proc. Ac. Nat. Soc. Philad. (without syns.)

Hartlaub (included in synonyms of *C. sabini*), Syst. Ornith. W. Africa, p. 25.

Hab., W. Africa (Auct. Cassin).

* For this date, we take the notice on 16th Number of Griffith's edition of Cuvier,—“The 17th Number of the Animal Kingdom will be published in October 1828.”

NOTE.—*A. hartlaubi* differs from *A. sabini* in the stout short form of the body, long wings, narrow white band on upper tail-coverts, and in the form and strength of feet.

Charadrius virginiacus.—In Dr Hartlaub's *System der Ornithologie*, West Africa, *Charadrius pluvialis*, or common golden plover, only is noted. In a small collection of birds lately sent from Fernando Po by Consul Burton, there is a specimen of *C. virginiacus*. This is not remarkable, being a bird of wide geographical distribution, but it has not yet been noticed as occurring in Western Africa.

PROCEEDINGS OF SOCIETIES.

Royal Society of Edinburgh.

Monday, 2d March 1863.—Principal Sir D. BREWSTER, V.P., in the Chair.

The following Communications were read:—

1. Letter from Sir D. Brewster relative to the specimens of Topaz with Pressure Cavities presented by him to the Museum of the Society.
2. On the Polarization of Rough Surfaces, and of Substances that reflect White or Coloured Light from their Interior. By Sir David Brewster, K.H., F.R.S.
3. On a Clay Deposit with Fossil Arctic Shells, recently observed in the Basin of the Forth. By the Rev. Thomas Brown, F.R.S.E.

The author having stated the circumstances which led to his discovering this bed with its fossils near the harbour at Elie, referred to a drawing of the section, and explained the position and contents of the different strata.

Specimens of the shells were exhibited, as named by Dr Otto Torrell of Lund, who had supplied important information as to their distribution. They are all, without exception, now living in the Arctic Seas. A majority of them are exclusively Arctic. Several are new to the British glacial deposits—viz., *Thracia myopsis*, *Pecten groenlandicus*, *Crenella decussata*, *C. lævigata*,* *Turritella erosa*,† and a new *Yoldia* found in Spitzbergen in 80° north lati-

* "Most probably, but much injured."

† "Almost certainly this species, yet cannot be positively asserted."

tude.* It was shown how strongly this evidence goes to prove the former existence of a Boreal or Arctic climate in Scotland.

The shells seem also to indicate some considerable rise in the level of the land. They are deep-water species—some of them very markedly so. Four distinct series of facts appear to show that they have not been washed up and transported, but are lying in the clay-bed where they originally lived. As the deposit is now rather above high-water mark, the fair inference would seem to be that the whole sea-bed of the Firth must have been considerably raised.

Reference was made to the discovery of the glacial beds of the Clyde by Mr Smith of Jordanhill. They had been looked for on the Forth, but without success. Dr Fleming struck the first trace of them at Tyrie, but it was faint, there being only two or three specimens of the shells, and these he was led to think not indigenous. In the Elie clay the same two species occur rather abundantly, along with others, all evidently in the clay-bed where they had lived. The group is so characteristic that there need be no question now as to the occurrence of the true old glacial beds with Arctic shells in the basin of the Forth.

Various reasons were stated for holding that this bed is very closely connected with the boulder clay, being not improbably a sea-formation contemporaneous with some portion of that deposit.

It was shown, that the facts brought to light in this section give us some glimpse into the circumstances under which the period of Arctic cold passed away.

4. On the Remarkable Occurrence of Graphite in Siberia.
By Thomas C. Archer, Esq.

Monday, 16th March 1863.—Dr CHRISTISON, V.P.,
in the Chair.

The following Communications were read :—

1. On the Polarization of the Atmosphere. By Sir David Brewster, K.H., F.R.S.
2. Concluding Note on the Star Observations at Elchies.
By Professor C. Piazzzi Smyth.
3. On a new fossil *Ophiuridan*, from Post-pliocene strata of the valley of the Forth. By Professor Allman.

I am indebted to one of our University students, Mr Peter Lawson, for a specimen of a star-fish, which he informed me had been found, along with many others, in a deposit of brick-clay near

* The other species are—*Saxicava rugosa*, large form, *Tellina proxima*, *Astarte compressa*, *Leda truncata*, *L. pygmaea*, *Natica groenlandica*, large form. Fragments also occur which seem to belong to *Cyprina Islandica* and *Mya truncata*.

Dunbar. The interest of this fact was a sufficient inducement to cause me at once to visit the locality where the star-fish was obtained, and where, by the kindness of Mr France, the proprietor of the brick-works, I succeeded in obtaining good specimens of the fossil.

Notwithstanding some very marked characters, which might possibly be regarded as possessing higher than specific value, I prefer referring the star-fish of the Dunbar brick-clay to Müller and Troschel's genus *Ophiolepis*, rather than encumbering the existing nomenclature with a new and doubtful generic name. The species, which is very distinct from every other described member of the genus, may be defined by the following diagnosis:—

Ophiolepis gracilis (mihi), nov. spec.

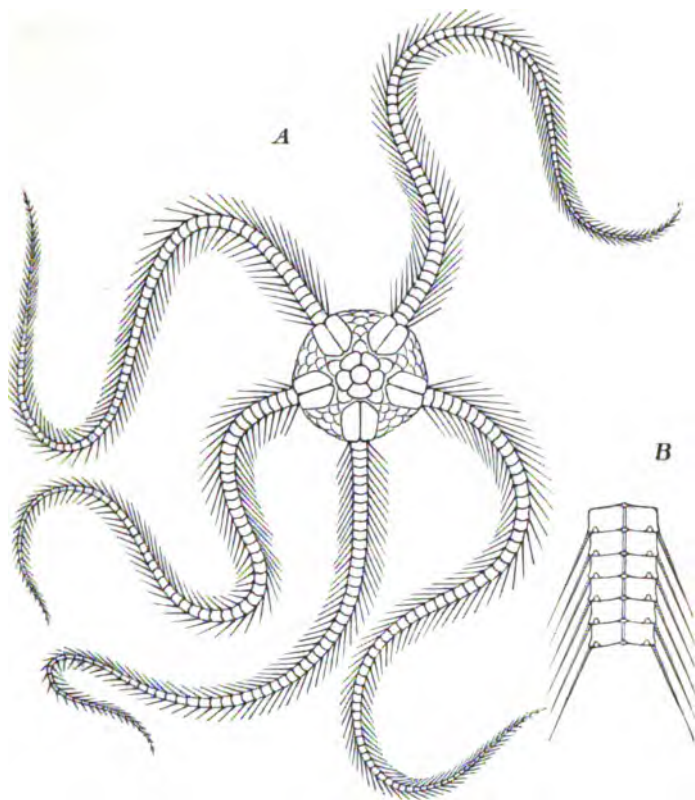
Upper surface of the disc covered with imbricated plates, a single circular plate occupying the centre, and with the radial shields large, and having their opposed edges in contact for their entire length. Dorsal shields of the arms about twice as broad as long near the disc, and thence with their breadth gradually decreasing in proportion to their length, until towards the distal extremity of the arm they become longer than broad; they cover the whole dorsal surface of the arm, and have their adoral and aboral margins transverse and parallel. Ventral shields of the arms very minute, and allowing the lateral shields of one side to meet those of the opposite side in the inferior median line of the arm. Aboral edge of each lateral shield with a notch for the exit of a cirrus. Spines about once and a-half as long as the breadth of the arms. Arms about five times as long as the diameter of the disc, and gradually tapering to a fine point.

The size of the largest specimens obtained is about four inches from tip to tip of the arms.

Fossil in brick-clay of the Post-pliocene age, near Dunbar, Scotland.

One of the most remarkable features in the present species is the rudimental condition of the ventral shields of the arms; these shields being much smaller than in *Ophiolepis ciliata* (Mül. and Trosch.), where they are exceptionally small among the *Ophiurida*, and where the lateral shields bear only short papilliform spines instead of the long, highly-developed spines of *O. gracilis*. It was only after having sought in vain for the ventral shields in some dozens of specimens that I succeeded in detecting them in a single instance. In this case they appeared in a view of the internal surface of the ventral wall (fig. B) as very minute rhombic plates lying along the mesial line, where they were interposed between the angles of the lateral shields. It is quite possible that the ventral plates are more fully displayed upon the outer surface of the wall, but in no case did I find this surface sufficiently exposed to enable me to obtain a view of them; while the inner surface, on the contrary, was frequently well exposed by the disappearance in the fossil of the dorsal shields, and of the series of vertebra-like ossicles, which, in the living *Ophiurida*, occupies the axis of the

arms. In most of the specimens sufficiently well preserved to afford a view of the ventral walls of the arms, the lateral shields were seen to be slightly separated from one another along the ventral



Ophiolepis gracilis—(A) viewed from the dorsal side, slightly enlarged; (B) ventral wall of one of the arms, still more enlarged, viewed from within; showing the lateral shields slightly separated from one another along the mesial line, where the minute ventral shields are introduced between their angles. The apertures for the exit of the ambulacral cirri are seen near the outer edges of the lateral shields.

suture, leaving here a distinct but narrow fissure, which was not interrupted even by the intervention of the minute ventral plates, which had in almost every case disappeared. In some instances, however, the lateral shields escaped displacement, and the two series were then in contact with one another along the line of suture. The notch for the exit of the cirri or tentacular ambulacra, situated on the aboral edge of every lateral plate, is very distinct, and is completed into an entire aperture by the adoral edge of the plate

next in succession. The spines along the sides of the arms are long and slender; in no case, however, could I satisfy myself that more than a single spine was borne by each lateral shield; but the condition of the specimens does not justify our thus limiting the number of those spines. Neither was I able to discover in the specimens any evidence of scales over the apertures for the cirri.

The deposit in which *Ophiolepis gracilis* occurs is a fine dense tenacious blue clay of Post-pliocene age. It is situated upon the shore of the Firth of Forth, about two miles to the west of Dunbar, and is largely worked for the manufacture of bricks. It lies low; and were it not for an artificial embankment, would be flooded at high tide.

In this deposit, at about five feet from the surface, is a horizontal bed, where the star-fishes are found. They occur in great numbers upon the surface of the bed, which is occasionally separated from the bed above it by a thin parting of fine sand. They are remarkable for their un mutilated condition, lying there with their slender arms, even to the extreme points, in the position which they must have naturally held during life, thus showing an entire absence of that spontaneous dismemberment which is so characteristic of the *Ophiuridæ* when dying under any prolonged irritation, and indicating some sudden cause of deprivation of life, such as we may suppose to result from an irruption of fresh water into the part of the sea inhabited by them.

None of the specimens I obtained, however, were sufficiently well preserved to enable me to make out all their characters as completely as I could have wished, the nature of the clay in which they were imbedded being apparently not suited to the preservation of the more delicate structures. The oral surface of the disc, especially, was in no case retained so perfectly as to allow of the mouth or the disposition of the plates of this part of the animal being observed. It was only in some instances that traces of the spines were visible, and then almost the only indications left were their impressions in the surrounding clay.

It is a curious and interesting fact, that not only did all the specimens found belong to a single species, but that not a vestige of a shell, or of any other organism, could be detected in any part of the clay which I had an opportunity of examining.

Monday, 6th April 1863.—PROFESSOR KELLAND, V.P.,
in the Chair.

The following Communications were read :—

1. Note as to Portions of Lead from the Roof of the Lower Storey of Nelson's Monument, injured by Lightning on the evening of 4th February 1863. By Professor C. Piazza Smyth.

The portions of sheet-lead above mentioned had attracted my atten-

tion on the days following the 4th of February, when engaged in repairing some damage which had then occurred to the electric wires connecting the Nelson Monument and the Observatory; and finding that plumbers (employed by the Town-Council) were removing the old lead and substituting new in its place, and being also encouraged by Professor P. G. Tait, who with me visited the spot, to believe that the markings which had been discovered were electrically of unusual interest, I lost no time in applying to Mr J. D. Marwick, town-clerk, for those portions of the leaden covering which contained the marks in question, with the view of presenting them to the Royal Society.

Mr Marwick was as obliging as prompt in responding to such a request, and sent me the required specimens next day, accompanied by the enclosed memorandum written by Mr H., assistant to Mr Cousin, city-architect.

10th February 1863.

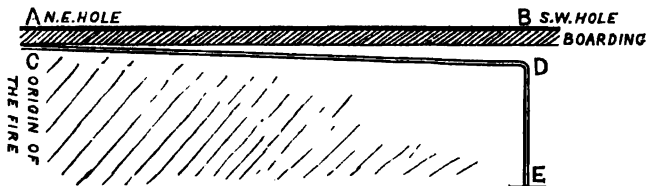
"The accompanying pieces of lead were taken from the north-west side of the lead platform on the roof of the lower part of Nelson's Monument.

"The distance between the holes was about nine feet six inches.

"Both holes were immediately over a block-tin gas-pipe, which is here carried under the lead and boarding of the platform, and which was found melted the whole length between the two holes, and a foot beyond the north-east hole.

"These holes in the lead were evidently *directly* caused by the burning gas from the pipes underneath.

Fig. 1.



"The sketch shows section through the platform between the two holes A and B. CD shows the gas-pipe under and between them.

"From this sketch we might naturally expect a hole melted in the lead at A, the pipe being here at its highest level immediately under the boarding, only one inch from the lead.

"The pipe between C and D would very quickly be melted by the burning gas, as it lies horizontally and can be more easily acted on by a flame issuing from it.

"At D the pipe dips vertically about eighteen inches.

"It is evident that the *vertical* length of pipe DE would not be readily melted by the strong flame burning at D.

"The boarding and lead at B, immediately over D, would there-

fore be exposed to the flame of the gas for a much longer time than any part between A and B. And here we might expect what we find, a large piece of the boarding burnt, and the larger of the two holes in the accompanying pieces of lead.

"How the gas was at first ignited remains to be shown.

"H."

The above memorandum gives an excellent matter-of-fact description of what chiefly remained to be seen at the time when it was written, but it does not mention what had much struck me several days previously, when I first caught sight of the small and neat oblong hole in the roof at A; and, on subsequently pulling up the leads, which at that time had not been disturbed at that place since the storm, I found the under surface of the metal strangely burred and scored around the aperture, and also perceived small globules of melted lead, driven away apparently by some radiating force from the hole, until caught and jammed between the remaining uninjured lead and its wooden surface of support.* Neither does the memorandum account for the first igniting of the gas, but expressly says, that what may have caused that "remains to be shown."

This is in truth the most important part of the whole affair, and which I will now endeavour to describe.

The evening of the 4th of February 1863 ushered in one of the most violent storms of thunder and lightning that has been experienced in Edinburgh, and perhaps in most parts of Scotland, for many years, and its violence was all the more remarkable, inasmuch as the month of February is near the minimum of the year for electrical manifestations in the shape of thunder-storms. This point, not yet generally acknowledged, is indicated pretty certainly by the following numbers, extracted from the Registrar-General's printed Reports for Scotland; they are, in fact, the deductions prepared for that officer at the Royal Observatory, Edinburgh, from the schedules of fifty-five observers of the Meteorological Society of Scotland, and give, for the means of three years, as follows:—

	Number of Stations at which Lightning was seen.	Mean Number of Times at each Station.
January, . . .	34	4
February, . . .	11	2
March, . . .	42	3

* These globules had very imperfect adhesion, and had mostly dropped off when the plate was presented to the Royal Society; but some few of the more distant ones still remained, and all the others had left marks, usually of a yellowish colour, showing their former positions, sizes, and shapes.

	Number of Stations at which Lightning was seen.	Mean Number of Times at each Station.
April, ...	24	4
May, ...	86	5
June, ...	85	7
July, ...	91	7
August, ...	54	3
September, . .	31	3
October, . . .	50	8
November, . .	30	4
December, . .	37	4

The storm, then, was anomalous in its season of occurrence, and in its violence; also, as it would appear from the newspaper accounts, by the regularity and broad spread of its passage over the country from west to east, occurring nearly an hour earlier at Greenock than at Aberdeen or Edinburgh. In Glasgow and its neighbourhood several buildings were struck, a tall chimney and a church entirely ruined; a lodging-house of operatives injured in every floor; and a large number of the telegraph instruments of the Private Telegraph Company thrown out of order, and one clerk rendered senseless.

This storm began in Edinburgh about 7^h P.M., and lasted nearly an hour; it came with very strong west wind, and accompaniments of rain and hail; and it was described to me by Mr Wallace, who was on the Calton Hill at the time, as being most remarkable for the slanting, almost horizontal, direction of the lightning, as well as its greenish-blue colour. The thunder was at the same time deafeningly loud, and on one occasion apparently coincident with the flash; shaking the house he was in (the old Observatory Tower), and giving the idea that either that building, or the Royal Observatory, must have been struck. Going out immediately to see what might have happened, he met the servant at the door, who spoke of the flash of lightning having entered the lowest room of that tower, "gone half way across the floor," and left an overpowering smell of "brimstone" behind; and also called his attention to Nelson's Monument, about 200 yards east-south-east of them, being apparently on fire, because sparks were issuing from the roof of one of the low rooms at its foot, on the western side.

Now, at the winter period of the year, it seems that the tenant there (Mrs ———) finding the above monumental building very cold, prefers to live in a wooden house close by, and on the evening of the 4th of February she was in that house ill and in bed; but when the particular flash occurred which had been so much noticed by Mr Wallace and the Observatory servant, it seemed close to her

also, filled her house with the brimstone odour, and so firmly impressed her with the belief that the Monument had been struck, that she sent out her servant "to see if the time-ball was still at the top of the building;" believing that if mischief had occurred anywhere from lightning, it would be near the summit of the structure. The answer, however, brought back was, that the time-ball was quite safe, but that sparks were coming out of the roof of the low west room. The policeman of the hill was likewise on the ground almost immediately after the flash, and testified both to the then sudden appearance of sparks issuing from the roof of the uninhabited room, and its accompaniment, by the traditional sulphurous smell in the air of strong lightning, or "ozone."

The door of the Monument was then speedily opened, access to the roof obtained, and the new-lit fire, caused by the burning of gas and wooden rafters, speedily extinguished. In this manner the gas, no doubt, after it was set on fire, did much mischief to both leaden roof and wooden rafters, especially at the place on the sketch marked B, which contains big and rather confused holes; but if any one still asks what first set the gas on fire, I think there is equally little doubt that we may answer "it was that particular slanting flash of local and ozone-producing lightning which excited the residents on the hill so much at the time."

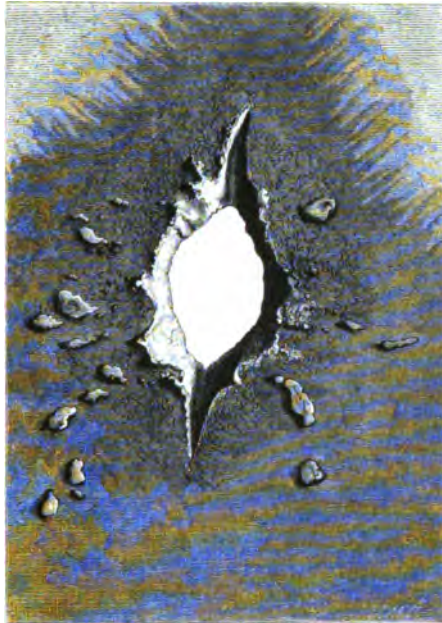
We may probably also assume that the lightning struck at the point A. I had already been directed to that point from the similarity of the hole there, to a lightning hole in sand, but could not imagine why the fluid should have pierced a hole through a good conductor, viz., a sheet of lead. On mentioning this difficulty to Professor Tait, he remarked, "that if the hole was due to the immediate action of the spark, I might be quite sure that there was a conductor below, which the lightning was trying to get at and pass off by," and *after* that opinion had been so expressed, the lead was lifted at the place (it had previously only been raised at B), and the gas-pipe was found precisely there at its closest point of approach anywhere to the roof, as may be seen clearly represented in the assistant architect's subsequent and independent drawing.*

However anomalous, therefore, the case may appear at first, some theoretical principles are remarkably borne out by it; and that the foot of the building should have been struck, and not the top, seems to follow from the low, level and almost horizontal direction in which the lightning was sensibly observed to come, and with the wind and rain,—causing thereby the windward foot of the tall building to become for that occasion the shortest passage for the fluid to reach the ground by.

* Professor Tait has also remarked, and it seems well worthy to be noted as a memorandum for any future occasion, that it would have been advisable to have preserved the boarding at "A," as well as the lead; for the manner of action and of piercing through wood by lightning is very different to the burning action of flame; and thick wooden planking *was* everywhere interposed between the gas-pipe and the leaden roof.

In such a case, though, it may be suggested, that the long horizontal wire extending through a length of 4000 feet between the Nelson Monument and the Castle for the service of the time-gun, should have been most abundantly charged by induction. That is true; and there is little doubt but that the said wire was

Fig. 2.



The hole at "A," as it appears on the under surface of the lead;
carefully drawn full size by Mr J. M. Corner, wood-engraver.

copiously filled, and might have produced dangerous effects, had it not been furnished at either end with large copper plates in close proximity to many pronged conductors ending in wet earth, which led away innocuously the greater part of the charge. Enough however still remained to do some singular damage to the electrically controlled clocks at either end of the line. Thus, the members of one of the bundles of permanent magnets, near the pendulum-bob of the Castle clock, had their poles changed and their new attraction made rather stronger than their old; the members of a similar bundle in the Observatory window-clock had their poles partially changed; and in the interior of the Normal Mean Time clock one of the gold contact points was partially fused, and spattered on its steel spring, which was blued at that part as though by heat.

The gold contact point thus treated, it will be understood, was

in direct metallic connection at the instant with the long open-air wire; and the magnets that were altered were in indirect connection, or rather, in inductive position, for they were surrounded at the time by the wire coil of the pendulum-bob, whose composing wire is a connected continuation of the long open-air wire.

To this it only remains to add, that these lightning-made magnets at the Castle clock, when duly replaced in the bundle suitably with their new poles, have given the most steady and satisfactory results in working the control of the clock ever since.

2. Note on the Anatomical Type in the Funis Umbilicalis and Placenta. By Professor Simpson.

3. On Earth-Currents during Magnetic Calms, and their Connection with Magnetic Changes. By Balfour Stewart, Esq., M.A., F.R.S. Communicated by Professor Tait.

In two previous communications made by the author to the Royal Society of London, it had been endeavoured to show that earth-currents and auroræ, which occur simultaneously with magnetic storms, are secondary currents due to the small but abrupt changes in the magnetism of the earth which such storms denote. Earth-currents also occur during periods of magnetic calm, but they can then be rendered visible only by means of a delicate galvanometer.

Such has been constructed by Mr C. V. Walker, who has by its means registered those earth-currents which occurred during the three last months of 1851, a period of magnetic calm.

The object of the present communication is to discuss those observations of Mr Walker in connection with the simultaneous changes which took place in the values of the declination and the horizontal force component of the earth's magnetism, these changes being furnished by means of continuously acting magnetographs at Kew Observatory. By this method Mr Walker's observations were divided into three classes,—

- 1st class. Observations during moments of magnetic calm.
- 2d " Observations during minor magnetic disturbances.
- 3d " Observations during greater magnetic disturbances.

In the first of these classes a law of hours was manifestly observed, the values of the earth-currents for the hours of the night being extremely small. But in the second, and especially in the third class, the law of hours was observed to fail; and in the latter of these classes the peculiar action of disturbances was very manifest, the tendency of such disturbances being to create very strong earth-currents at the moment of their occurrence; and it is noticeable that these earth-currents were as often positive as negative.

It was remarked by the author, that the very great strength of those earth-currents, which take place at the moments of disturbance, is in favour of the theory of induction, since the peculiarity

of a disturbance is not so much *a very great as a very rapid and abrupt* departure of the magnet from the normal position; and since, on the theory of induction, the corresponding earth-current will be due to abruptness of magnetic change, we thus obtain an explanation why the currents which accompany disturbances are so very powerful, and also why these are as often positive as negative.

With regard to the first class of earth-current observations, or those which occurred during magnetic calm, the author believed the daily range indicated by these observations to be the induction effect of the daily magnetic change, on which hypothesis the small value of the currents for the night hours might be accounted for by the corresponding fact that during these hours the magnetic change is exceedingly small.

4. Note on a Pictish Inscription in the Churchyard of St Vigens. By Professor Simpson.

Dr Simpson considered the inscription to be "Drosten, Son of Voret, of the family of Fergus."

5. On some Kinematical and Dynamical Theorems. By Professor W. Thomson.

6. Note on a Quaternion Transformation. By Prof. Tait.

Monday, 20th April 1863.—PRINCIPAL FORBES, V.P.,
in the Chair.

The following Communications were read:—

1. On the Conservation of Energy. By Professor Tait.
2. On Fagnani's Theorem. By H. F. Talbot, LL.D.
3. On the Theory of Parallel Lines. By H. F. Talbot, LL.D.

An Address to His Royal Highness the Prince of Wales was adopted, and ordered to be forwarded to the Duke of Argyle for presentation.

Royal Physical Society.

Thursday, 26th February 1863.—JAMES M'BAIN, M.D., R.N., President,
in the Chair.

The following Communications were read:—

- I. *On the Fossils of the Boulder Clay (Till) of Caithness.* By CHARLES W. PEACH, Esq., Wick.

Mr Peach gave details of the various kinds of shells that occur in

many places of the boulder clay, and referred to their orders and classification, the number of species being as follows:—32 Shells, 15 Univalves, 17 Bivalves; 1 Balanus; 1 Annelide; 4 Polyzoa; 2 Sponges; 1 Coral; 1 Alga—42 total species. Of the shells, 29 are British; 2 Scandinavian; 1 Arctic. All the others are British. He stated that the clay might be traced all over Caithness, especially in the estuaries and the sides of rivers and burns. Some sections showed a depth of 60 or 80 feet; while in other places, from low-water mark, it rose a height of 200 feet and upwards above the level of the sea. It differed greatly in constitution in the same locality. In some places shells were abundant, with very few stones, the clay being pretty soft; in others, stones were abundant, shells rare, and the clay so compact that it could not be blasted with gunpowder. Occasionally it contained large blocks of stones, such as granite, porphyry, gneiss, quartz, &c., some of which contained fossils, which were all more or less worn and polished.

II. *Observations on British Zephytes.* By T. STRETHILL WRIGHT, M.D.

III. *Notes on Ophrydium versatile.* By Mr W. R. M'NAB.

Wednesday, 25th March 1863.—JAMES M'BAIN, M.D., R.N., President, in the Chair.

The following Communications were read:—

I. *Notes on some Surgical Homologies.* By A. M'K. EDWARDS, Esq.

II. *On the Osteological Homology of the Vertebrate and Articulate Classes.* Part I.—The Cranium. By Professor WILLIAM MACDONALD, St Andrews.

III. *Note of a Fireball, recently observed near Auchterarder.* By JOHN ALEX. SMITH, M.D.

In the beginning of the month of February, a notice appeared of the descent of a so-called aerolite upon the farm of Drumtersal, near Auchterarder. Lieutenant-Colonel Hunter, who saw the fall, favoured Dr Smith with details regarding the phenomenon. The fire-ball fell on Tuesday the 27th of January, at five minutes to five p.m. The day had been beautifully bright with sunshine; the sun had set for eighteen minutes, it was however still full daylight, when Colonel Hunter's eye was attracted by the descent of a most brilliant luminous body. Its fall was nearly perpendicular, and it had the appearance of being the size of a twenty-four pound shot. It seemed to fall in a lea field. Colonel Hunter's gamekeeper also saw it, from another direction, apparently fall in the same field, and after twenty-four hours had passed, with heavy rain, he went to see if he could discover anything. His eye was soon attracted by the peculiar appearance of a stone which lay in the place where the meteor seemed to fall. The stone was twelve ounces in weight, had a sulphurous smell, and the grass round about it was blackened. Part of the stone was examined by a chemist who recently published the result of his analysis, and stated, among other components, that it contained 17·22 per cent. of sulphuret of nickel.

From the fact of no report of any kind being heard when the ball was seen to fall, Dr Smith was very doubtful of any stone-like body having fallen,—a loud report generally accompanying the fall of an aerolite. He fancied the ball must therefore have consisted simply of gaseous

or electrical agencies, the resistance of the atmosphere to its rapid passage through it being apparently so slight as to cause no noise. He was aware also of the danger of any peculiar or unusual-looking stone lying near the place where an apparent fall had taken place, being mistaken for the actual body that was apparently seen to have fallen there. The analysis, however, was decidedly against this view of the case; and Colonel Hunter politely forwarded to him the mineral mass for examination, and some of its broken portions, to allow of the correctness of the analysis being tested. On examining the mass, Dr Smith found no traces of the dark-coloured outer coating or skin which exists on ordinary meteoric stones, depending, it is believed, on the fusion of their surface, by the heat evolved in their passage through the atmosphere; but, instead, the surface was rough and irregular, showing on one side various cubical coaly-looking masses; and on the other the coaly-like matter projecting through a series of thin layers, apparently of white sulphuret of iron, or white iron pyrites. The specimen appeared to be a portion of one of those small seams of white iron pyrites, which are not uncommon in our coal seams. On showing it to various friends—Professor Allman, Dr M'Bain, Mr Alex. Bryson, and others—they all agreed with him, there could be no doubt of its being simply a mass of white iron pyrites. The existence of the sulphurous smell in its gradual decomposition, after exposure to continued rain, was, of course, to be expected from its composition as a sulphuret of iron; and the same would also account for the blackened appearance of the vegetation round the mass of pyrites. The broken portions of the stone Dr Smith put into the hands of Dr Murray Thomson, telling him nothing of its history, and requesting to know if it contained any metal besides iron; and was favoured with a reply, stating that he had made a lengthened examination of the substance sent him, and could detect no traces of anything besides iron, sulphur, and a little siliceous matter.

IV. *Remarks on a "Raised Beach" at Ardross, in the County of Fife.*

By the Rev. WALTER WOOD, Elia. Communicated by JAMES M'BAIN, M.D., R.N.

Wednesday, 22d April 1863.—JAMES M'BAIN, M.D., R.N., President, in the Chair.

The following Communications were read :—

I. *On the Evidence of the Rise of the Shores of the Firth of Forth.* By ALEXANDER BRYSON, Esq.

Mr Bryson described various places on the shores of the Firth of Forth, to show that there was not the slightest evidence to be found of any rise of level, at least in recent times. He referred to the rock at Cramond sculptured with the Roman eagle, believed by antiquaries to be the work of the Romans; and showed that it was now just above high-water mark, so that, if a rise of 25 feet, as has been assumed, or indeed any rise at all, had taken place since that time, this rock must have been entirely under water at the Roman period. He also alluded to the position of the Roman wall at Carriden, and showed that no remains of it had been visible for at least 200 years back; so that no evidence of any kind could be got from it. He also pointed out the existence of an extensive fluvial or fresh-water deposit at the mouth of the Almond at Cramond, without any included marine remains, the existence of which would have been impossible had any great rise of the land taken place here.

II. *Remarks on a Peruvian Skull.* By JAMES M'BAIN, M.D., R.N.

Dr M'Bain stated, there was no foundation for the assumption that the crania of the ancient Peruvians always showed additional or wormian bones in the lambdoidal suture of the skull.

III. *On the Occurrence of the Rosy Feather-Star, Comatula rosacea, on the Eastern Shore of Scotland, especially that of Caithness.* By CHARLES W. PEACH, Esq., Wick.IV. *The Kjökken-Möddinger of Denmark, and their similitudes on the Elginshire Coast.* By the Rev. GEORGE GORDON, LL.D., Minister of Birnie.V. *Notes of Bones of the Ox and Dog, and a curious Bronze Implement found in a bed of undisturbed gravel at Kinleith, near Currie, Mid-Lothian.* By JOHN ALEX. SMITH, M.D.

The bones and bronze implement (exhibited) were found in the valley of the Water of Leith, at a depth of about 12 feet from the surface, in a bed of gravel, which was covered by a bed of undisturbed sand and silt, about 5 feet in thickness. Dr Smith had seen no notice of any weapon of a similar character having been found in Britain or Ireland; it was, however, closely analogous to "a crescent-shaped" implement of bronze discovered at Steinberg, one of the ancient piled lake-dwellings of Switzerland, which is figured in M. Troyon's work on these lake habitations.

Botanical Society of Edinburgh.

Thursday, 12th March 1863.—Professor BALFOUR, V.P., in the Chair.

The following Communications were read:—

I. *Notice of Pandanus odoratissimus (Fragrant Screw-pine), now flowering in the Palm-house at the Royal Botanic Garden.* By Professor BALFOUR. (Plate III.)

Pandanus odoratissimus has recently produced pistilliferous flowers in the Palm-house at the Royal Botanic Garden. The plant is a native of the East Indies, China, and the Islands of the Pacific Ocean. The plant in the garden is about fifty years old, and has attained a height of 40 feet, with a stem about two feet in circumference. It has sixteen large alternate branches, and is provided with sixty aerial roots, some of them five feet long. (Plate III. fig. 1.) The leaves vary in length from three and a half to five feet. The plant has produced two globular spadices of pistillate flowers (fig. 2). As no staminate flowers have appeared on any of the plants in the Palm-house, the fruit will not be perfected. The fragrance is chiefly confined to the staminate flowers. The plant is used for hedges and for various economical purposes, such as the preparation of matting, bags, baskets, &c. A specimen of the pistilliferous spadix was exhibited. The species, so far as known, has not previously produced flowers in the botanic gardens of this country.

Explanation of Plate III.

Fig. 1. Sketch of plant in the Palm-house of the Edinburgh Botanic Garden.

Fig. 2. Branch with leaves and globular cluster of fruit.



R. K. Greville, del.

W. E. M. Parlane, lith. Edin.

Pandanus odoratissimus.



II. *On a Method of taking Impressions of Plants by means of Printers' Ink.* By WALTER ELLIOT, Esq., of Wolfelee.

Mr Elliot explained to the meeting his process of taking impressions of fresh plants by means of a press and printers' ink. The specimens are covered equally on both sides with the ink by means of a roller, and then placed in the press between sheets of paper, and pressure applied. The whole process is exceedingly simple, and may prove useful to travellers. The press in operation was exhibited.

III. *Remarks on the Economical Use of the Carnauba Palm* (*Copernicia cerifera*). By Professor ARCHER.

IV. *Notes regarding some new and rare species of British Mosses.* By Mr JOHN SADLER, F.R.P.S.

V. *Register of the Flowering of certain Spring Plants in the Open Air at the Royal Botanic Garden, Edinburgh.* By Mr M'NAB. (Second list.)

	1863.	1862.	1861.
Knappia agrostidea, ...	Feb. 13		
Symplocarpus foetidus, ...	... 15	Feb. 26	Feb. 9
Daphne Mezereum, ...	... 16		
Tussilago nivea, ...	... 18		
Muscari botryoides, ...	... 20	... 22	... 26
Dondia Epipactis, ...	... 23	... 28	... 1
Orobus vernus, ...	... 25		Mar. 4
Arabis albida, ...	... 28	Mar. 5	... 6
Scilla bifolia, cœrulea, ...	... 28	Feb. 20	... 7
Ribes sanguineum, ...	March 2		
Scilla bifolia, alba, ...	... 4	Mar. 10	... 9
Erythronium Dens-canis, ...	... 4	... 15	... 14
Draba aizoides, ...	... 4		
Orobus cyaneus, ...	... 6		
Narcissus pumilus, ...	... 8	Feb. 27	... 6
Primula nivalis, ...	... 8		... 12
Puschkinia scilloides, ...	... 9	Mar. 16	... 18
Scilla sibirica, ...	... 10	... 17	... 8
— bifolia, rubra, ...	... 10	Mar. 12	
Corydalis nobilis, ...	... 11		... 12
Pulmonaria mollis, ...	... 11		... 12
Hyoscyamus Scopolia	... 12		... 9
— orientalis, ...	... 12		

A letter was read from Professor Martius of Erlangen, to Professor Balfour, in which he says:—"I wish to call your attention to the fact, that the seeds of *Abrus precatorius* contain an alkaloidal poisonous matter. It is easily obtained by boiling the crushed seeds several times

with alcohol of 0·830 to 0·812, filtering, and then distilling the alcohol until two ounces remain per pound of seeds. If it stands a long time, the poisonous matter crystallises out. Weak alcohol exhausts the colouring matter."

Thursday, 9th April 1863.—Professor MACLAGAN, President, in the Chair.

H.R.H. the Prince of Wales was elected an Honorary Fellow of the Society.

A congratulatory address to H.R.H. the Prince of Wales on the occasion of his marriage was adopted.

A memorial to the First Commissioner of H.M.'s Office of Works, asking for more accommodation at the Botanic Garden for the Herbarium and Library of the University, was also adopted.

Professor Balfour read a short notice of the death of Mr John Wright Brown, who had acted for six years as assistant in the Herbarium department of the University Museum.

The following Communications were read :—

- I. *Note on Lemania variegata of Agardh.* By GEORGE LAWSON, LL.D., Ph.D., Professor of Chemistry and Natural History in the Queen's University of Canada.

(This paper appears in the present number of this Journal.)

- II. *Some Account of Paullinia sorbilis and its Products.* By T. C. ARCHER, Esq., Director of the Industrial Museum of Scotland.

- III. *Notice of Observations by F. COHN, Breslau, on the Contractile Filaments of the Stamens in Thistles.* Communicated by Dr ALEXANDER DICKSON.

(This paper will appear in next number of this Journal)

- IV. *On an Easy and Effective Style of Nature-printing.* By Mrs STIRLING of Kippenross. Communicated by T. C. ARCHER, Esq.

Take some finely powdered lamp-black, mix it smoothly with fine almond oil and a small proportion of pale drying oil, till of the consistence of very thick cream. The paper used should be of a soft spongy kind. Smooth crayon paper or fine Bristol board is best; a hard, rough-grained paper will not answer well. Take a small branch where the leaves are nearly perfect. As accuracy is chiefly required, it is better to lay the branch face downwards on your paper, and with a pencil lightly trace out the stem and side branches, marking where the principal groups of leaves lie,—this will ensure a more faithful copy than sketching the branch by sight. Place the branch upright before you in a pot of wet sand, and then you can either strip off these leaves and print them, or by having other branches beside you, you can select leaves resembling those of your model, remembering that the side of the branch next your right hand is on the left of the paper. Take the finger off a clean white kid glove, put a small bit of jeweller's cotton into it, so that when you put the glove finger on the first finger of your right hand, the cotton forms a

soft cushion at the end. Fold some soft paper in several folds, upon this lay the topmost leaf, or group of leaves, face uppermost; take some of the black upon your first finger and smear it over the leaf (or leaves) quite evenly and quickly; lay them down on your crayon paper in their right position; put a thin piece of paper over them, holding it quite steadily, and rub each leaf firmly all over, following the veins of the leaves, running your finger from the stem to the point, and from the mid-rib to the edge. Each bit should be done at once; going over the same part twice often causes a double impression. Raise the leaf quickly when done. In blacking the leaves include the footstalk; it will never make a perfect impression, but serves to mark the outline, and forms a guide for you afterwards. No blots, or thicker portions of the black must be allowed; they will make a blotch in the impression. Work downwards from the top of the branch. When all your leaves are finished, take lithographic chalk and draw in the whole of the stem, branches, and foot-stalks, marking any irregularities, knots, or buds, as these should be finished with equal care and accuracy. Lithographic chalk has the advantage of fixing at once, and blends better with the printing than any crayon or glossy pencil. As to the capabilities of this process, I found that a group of acacia or elm leaves could be done together; the horse-chestnut, ash, or walnut leaves, require to be separated. No very glossy or very rough leaf will take the black sufficiently to make a good impression. All the leaves must be perfectly fresh. I have done a series of forest trees, including several of the fir tribe. These last print very imperfectly, but still well enough to show the character of the tree, and the imperfect parts can be filled in with chalk. I have also done a number of the ferns, the difficulty with them being solely in their size; but I divide them into suitable portions, and no break is discernible in the impression. I thus managed to print both the *Osmunda regalis* and the common bracken. If the surface you have to blacken is too large, the first bit you do becomes dry before the rest is finished. Many flowers are capable of being printed.

V. *Notice of the Tallow Tree of China (Stillingia sebifera), lately introduced into the Punjaub.* By WILLIAM JAMESON, Esq., Saharunpore. Communicated by Professor BALFOUR.

VI. *Memorandum on Local Museums in the Punjaub.* By HUGH CLEGHORN, M.D.

VII. *Notice of an Ash Tree struck by Lightning at Dunipace.* By the Rev. THOMAS ROBERTSON.

Mr Robertson gave some account of an ash tree which was struck by lightning on 14th June 1849, of which he was an eye-witness. The tree, which was of large size, grew close by the Avon Burn, and near the manse of Dunipace.

VIII. *Register of Plants in Flower in the Open Air at the Royal Botanic Garden.* By Mr JAMES M'NAB.

1. *Third Spring List* :—

Asarum europæum, March 15.	Narcissus moschatatus, March 25.
Saxifraga crassifolia, March 18.	—Pseudo-Narcissus, March
Fritillaria imperialis, March 22.	28.
Dalibarda geoides, March 28.	Anemone nemorosa, April 2.
Corydalis solida, March 24.	Adonis vernalis, April 7.

2. *List of Plants in Flower in the Open Air on 9th April 1863, not hitherto recorded as flowering at so early a date in the Botanic Garden.*

Rhodora canadensis.
 Rhododendron alta-clarense.
 ————— ciliatum.
 Rhodothamnus Chamæcistus.
 Phyllodoce cærulea.
 ————— empetriformis.
 Andromeda tetragona.
 Bryanthus erectus.
 Potentilla alba.
 Euphorbia gloriosa.
 Saxifraga tridentata.
 Epimedium alpinum.
 ————— violaceum.
 ————— rubrum.
 Lepidium procumbens.
 Draba confusa.
 Cochlearia officinalis.
 Alyssum saxatile.
 Dicytra spectabilis.
 Fumaria lutea.
 Ranunculus montanus.
 Anemone apennina.
 Crespidium Richei.
 Symphytum tauricum.
 Holosteum umbellatum.
 Sesleria cærulea.
 Luzula sylvatica.

Luzula pilosa.
 Carex montana.
 ————— japonica.
 Helonias bullata.
 Sanguinaria canadensis.
 Equisetum arvense.
 Iris reticulata.
 Tulipa Gesneriana.
 Hyacinthus orientalis.
 Leucocoryne alliacea.
 Ornithogalum excapeum.
 Narcissus Tazetta.
 ————— odorus.
 Scilla italica.
 Primula ciliata, purpurata.
 ————— Polyanthus.
 ————— marginata.
 ————— viscosa.
 ————— integrifolia.
 Pulmonaria virginica.
 Jeffersonia diphylla.
 Erica hybernica coccinea.
 ————— alba.
 ————— minor.
 Trillium grandiflorum.
 Sieversia triflora.

3. *List of Fruit and other Trees in Flower, and with Buds far advanced towards flowering, on 9th April 1863.*

Cherry, gean, pear, plum, apricot, currants, gooseberries, in full flower. Apple and thorn, some of the buds almost open. Elm, Norway maple, Canadian mespilus, *Pyrus nivalis*, in full flower. Thorn in full leaf, covered with the fruit of 1862. Thorn in full leaf, with the leaves of 1862 still green.

Mr John Reid, manager of the Edinburgh and Leith Gas Company, presented a large specimen of a new gas-coal from Gorebridge, near Arniston, accompanied with the following analysis:—Products from 168 lbs. of coal—Oil tar gas and volatile matters, 82 lb.; coke and ash, 86 lb.; gas, in cubic feet, 560=11,333 cubic feet per ton; specific gravity of gas, temperature 53°, 580·7, air=1000; heavy hydro-carbons, condensed by bromine, 15·5; carbonic acid, condensed by potash, 0·25; duration of 1 foot of gas in maintaining a flame from single jet 4 inches in length, 83·3 minutes; illuminating power by photometer, 27·94 stand. candles.

SCIENTIFIC INTELLIGENCE.

ZOOLOGY.

Syrrhaptes paradoxus.—The following paragraph appeared in the "Times" Newspaper of 6th ult. :—

"SIR,—Another specimen of this remarkable bird has met its death on British soil, and narrowly escaped passing into oblivion unhonoured and unknown. Shot by some unknown person, the precise locality also as yet unknown, it was received on the 1st inst. from Perth, in a basket of leverets, rabbits and pigeons, forwarded to Mr A. Ruthven, wholesale game-dealer of this town, for sale in the ordinary way of business, without note or comment of any kind. By Mr Ruthven it was kindly given to Mr Thomas Littler, who presented it to this museum. It was a very fine adult male, and had apparently been dead a day or two.—I have the honour to be, Sir, your obedient servant,

"THOS. J. MOORE, Curator.

"DERBY MUSEUM, WILLIAM BROWN STREET,
Liverpool, June 6."

The appearance of these birds in this country of late years is very curious and remarkable, and the questions arise, What is the cause? Have they been previously overlooked? Specimens of the bird were formerly extremely difficult to procure, not perhaps from the scarcity of the species in the districts which it frequents, but from inaccessibility of the districts, and the general difficulty of procuring anything from them which was out of the ordinary line of commerce. The opening up of China made them more common, and specimens brought by Mr Swinhoe from the neighbourhood of Pekin were sold at a moderate price. This will not, however, account for the appearance of these birds in Great Britain.

I have received a note from Professor Dickie of Aberdeen which may throw some light on Mr Moore's letter in the "Times," and the specimen forwarded to Liverpool was either part of the same flock of birds which went on to Aberdeenshire, or of other flocks driven over to us under the same circumstances.

ABERDEEN, June 1, 1863. DEAR SIR WILLIAM,—“Two specimens of *Syrrhaptes pallasi*, Temm., were shot on the coast near this at Munchals, seven miles to south of Aberdeen. There was a flock of at least fifteen. Mr Carfrae, the curator of our Museum, directed my attention to the specimens, they having been given him to stuff.”

Professor Dickie has also enclosed the following notes from his intelligent curator, Mr Carfrae. The food they selected in this country and manner of flight, &c., are all interesting points :—

“They were shot near Stonehaven, May 28, 1863—Weight of two specimens, 1 lb. 9½ oz., 1 lb. 9¼ oz. avoidr. Size—extreme length, 18½ inches; breadth, 25¼ inches. Bill, bluish, darker at tip, slightly compressed and depressed. Nostrils, small, basil, covered with feathers. Iris very dark. Ears, external apertures *very large*; the colouring agrees with all the descriptions I have seen. Stomach very muscular, inner layer of a horny consistency, intestines short or medium length. Crop large, and filled with turnip and grass seeds; the birds were in fine condition, fat, and apparently healthy; there were about twelve or fifteen in the flock when first seen; they were very shy, and flew very quickly, but low; they have not been seen in the neighbourhood since; the bones, &c. of the specimens are in the Museum here.”

W. J., Ed.



BOTANY.

Botanical Collector in British Columbia.—Mr Robert Brown, the Botanical Collector sent out by the British Columbian Association, reached Victoria, Vancouver's Island, on 6th May 1863, and has now commenced his examination of the Flora of the Island.

Cinchona Bark.—The Red Bark grown in India, after two years' growth, has been found to yield quinine in the same quantity as the same species of *Cinchona* in South America. In Mr MacIvor's report of 31st March 1863, it appears that there are now 146,548 *Cinchona* plants in the Nilghiri Hills, 35,750 of which have been planted out. A qualified gardener is about to proceed to Darjiling to superintend *Cinchona* plantations there.

MISCELLANEOUS.

Dr Hector's Exploration of New Zealand.—The following notice has been contributed to the "Otago Daily Times" by one of Dr Hector's party. It is dated "Left Bank of the Matukituki River, March 8th, 1863." After alluding to the preparations made for the expedition to the West Coast, he says:—

"Proceeding up the valley of the Matukituki, we found it to preserve the same character for several miles, viz., a broad shingle channel, out of all proportion to the magnitude of the stream, and only very partially occupied by it. Our progress was slow, so that by the evening of the first day we had only accomplished seven miles. A dense fog continued to obscure the higher mountains from our sight. Early next morning I obtained my first view of the icy pinnacles of Mount Aspiring, its apparent altitude being greatly increased by the rolling mist which still enshrouded its base. Mount Aspiring is one of a group of lofty mountains, all of which it evidently overtops by a pyramidal mass of rock almost too steep to allow of eternal snow resting on its surface, and which forms the characteristic feature by which it might be recognised almost at any distance. About ten miles above our main camp, the Matukituki is formed by the confluence of two equal-sized branches. The one from the north, although the shortest, may be properly considered the main stream. The other, which sweeps from the west, joins it through a rocky defile, which at first sight we anticipated would offer an obstacle to our progress. We were, however, surprised to find it occupied by an open flat, fringed by noble forests of beech, which clothe the mountain sides also to the height of about 2000 feet. A short distance above the junction of the two branches, we forded the stream. The scenery for two miles above this ford is extremely beautiful. The mountains on either hand are to be seen furrowed by impetuous torrents issuing from the snow at their summits, and gaining the level of the river by a succession of wild leaps. One of the most strikingly picturesque of these cascades descends by six precipitous bounds from an altitude of about 1200 feet, forming graceful curves of white spray that are momentarily swayed into fantastic shapes by every gust of wind. The dark green shades of the massive beech forests along the base of the mountains also, are relieved at intervals by the pale verdure of patches of Mallow trees, which at this season were covered with their snowy white blossom.

"On the 13th February, we recrossed the river, and making a considerable ascent on its right bank, gained the elevated terrace which borders the gorge immediately below M'Kerrow's Flat. The tangled nature of the scrubby vegetation in this portion of the valley, and the frequent succession of deep gullies, rendered our progress so slow and tedious, that Dr Hector struck off to the river, and succeeded in finding a more practicable track by which to avoid the labour of hewing, as well as

a means of again descending into the immediate valley of the stream. In the gorge, the channel is beset with huge boulders, rendering the river for some distance a series of foaming rapids, as well as preventing any track being practicable along the edge of its waters. Just above the gorge, through a narrow fissure in the mountains, there is a fine view of the glaciers which clothe the southern flanks of Mount Aspiring.

"The Matukituki in this part of its course flows due south, and we were now within a few miles of the boundary line of the Province of Canterbury, which crosses it at right angles, and opposite to a depression in the mountains to the westward which from a distance had seemed favourable to our further progress to the west, and from which there issued a large tributary to the river. As it was Dr Hector's wish to keep as close to the boundary line as possible, and within the province of Otago, he devoted the 15th in searching for a pass in this direction. By a laborious climb, he ascended the mountains to a height of about 5000 feet, but could observe no available route, as the extent of the nick, or depression, was deceptive, and confined to the exterior range bordering the valley; while to the westward a succession of lofty mountains rose from an extensive *mer de glace*. To the north, however, he perceived that, by following the Matukituki to its source, a comparatively low saddle would lead into a region of country in which the mountains appeared to have a lesser elevation. He resolved, therefore, to make the first attempt in this direction.

"On the 16th we continued our journey along the edge of the river, climbing over the huge boulders which are dispersed along its margin. The mountains on either side of the stream present cliffs quite perpendicular to its waters, so that we were compelled to climb a spur and pass through the woods to avoid the obstacles which they thus offered to our progress. Passing through the thick woods, which are intersected by deep cuttings choked with fern, scrub, and masses of decayed vegetation, we again descended to the boulders in the bed of the river, and following it a short distance, merged on a small plain about a mile and a half long, and occupying almost the whole breadth of the valley. We encamped here, at the foot of a strip of mountain, which some time previous had been cleared of forest by the violence of a recent avalanche. A mass of trees torn up by the roots, and surrounded by an enormous quantity of rock, lay piled around us, an indication of the tremendous power which produced such ruin. The ascent towards the source of the Matukituki from this point is very rapid, and at the distance of about three miles from it the first glacier is met. It is very insignificant, indeed scarcely deserving the name. From the ice cave, at its lowest extremity, a small stream flows to the main channel, which, both above and below their junction, is contracted and rocky, causing the river to assume the character of a series of foaming cascades. Four miles still further up the valley, we attained the limit of the woods; and although early in the afternoon, we encamped at this place, as we should get no fuel until we had crossed the saddle and descended to the limit of the woods on the other side. The altitude at which this limit on the eastern side of the saddle is observable is about 3500 feet above the sea. From this point to the base of the height of land, the river valley is extremely rugged, being occupied by old glaciers presenting the general chaos common to all moraines,—viz., a confused mass of angular and smoothly worn boulders distributed without any regard of size, or order of deposit.

"We were now at the most north-westerly portion of the province of Otago, and close to the boundary line which separates it from that of Canterbury, having followed the largest tributary of the Molyneux to its source. By a steep climb, we gained the summit of the saddle, following the course of the larger of the minor arms of the Matukituki, travelling

a portion of the distance on the uppermost glacier which feeds the river. From this point a most magnificent view was spread out before us. The high mountains to our right, with their clear blue pinnacles of ice pointing to the sky, and shrouded in enormous glaciers, presented a truly Alpine scene, while the whole valley at our feet was completely filled by a glacier occupying an area of about five square miles. This latter, as well as the mountain from which it descends, and the river to which it gives birth, Dr Hector named in honour of Mr Haast, the provincial geologist of Canterbury, that gentleman being the first scientific traveller to describe the glaciers of New Zealand. After making an examination as to the best way to descend, a task both perilous and laborious, owing to the glacialised surface of the rock composing the mountain, and the high angle of inclination of its side, we commenced our descent. For some distance we succeeded easily, but occasionally we were completely stopped by a peculiar blue rock interbedded with the slate, and which may be appropriately likened, from its undulated surface, to corrugated iron. Although we had started from the saddle at about 1 p.m., it was growing dark by the time we had gained the upper part of the small glacier to our left, and it was at least 800 feet above the true base of the mountain, or 900 above the surface of Haast's Glacier. The continual fall of avalanches and loose stones in the valley occupied by the smaller glacier rendered travelling very dangerous, and as night was fast approaching, Dr Hector thought it advisable to reascend a portion of the mountain, and avail ourselves of a hole in the rock for a night's lodgings, as well as a shelter from the falling ice.

"The night was extremely comfortless, with a high wind that howled among the mountain fastnesses, and chilled us in our rocky cave; and at intervals a crashing noise, like the outburst of distant thunder, told us of avalanches quitting their icy thrones to pay their tribute to the ocean king. Early next morning we descended to the small glacier alluded to above, and prepared to traverse it. The valley which it occupies does not exceed a mile and a half long, and about a quarter of a mile broad; but owing to the steepness of the glacier, and the deep crevasses or rents that occur in the ice, a large amount of Alpine experience and skill is requisite before attempting to cross it. It assumes also the shape of a wedge, and in consequence of its sides nowhere touching the rocks that border it on either hand, but surrounded by wide chasms instead, its impracticability is therefore rendered still more apparent. Previous to starting from Dunedin, Dr Hector, by his foresight, had provided a small-sized cord of about three-sixteenths of an inch in diameter, and eighty feet in length, similar to what is used as tracking-lines in canoe voyages on the rivers of Canada, and from its great utility to parties fording rapid rivers in this country, experience has taught us that it cannot be too strongly recommended. For instance, when crossing the strong and rapid current of a river, our method was to tie the cord to our waists, at a distance of twelve or fourteen feet the one from the other, then, while the leader was baffling with the force of the stream in the deepest and strongest part of its course, he was supported by the others until he had reached a portion of the river the force of which he was able to contend with, and at the same time afford assistance to the next in order to cross, and so on until all had gained the opposite bank. In descending the small glacier in like manner, we were thus attached the one to the other, to prevent any serious accident happening in case of slipping the foot or falling into one of those frightful crevasses, the sight of which is often alone sufficient to unnerve the stoutest heart. We encountered two very dangerous fissures in the descent. Both were too wide to leap, and we cleared them in the following manner. Dr Hector sought out the most

practicable part, where the ice on one side approached more nearly the ice on the other, and with an axe cut steps to where it was possible to leap; then he continued cutting steps in a slanting direction until he had gained the top of the ice on the opposite side of the crevasse. This being done, we attached the packs to the middle of the cord, and he pulled them across. Then with our waists carefully attached, and following the steps he had cut, we passed over singly in the manner he himself had done. We occupied two hours and a half in getting to the commencement of Haast's Glacier.

"Dr Hector having carefully examined the surface with his glass, and fixed upon the best route for avoiding the 'crevasses,' took the lead, three of us being lashed together as before by the rope. At first we had to ascend the glacier for some distance, to head a wide and profound crevasse compelling us to pass among blocks of ice and stone, the debris of avalanches which fall every ten or fifteen minutes from the higher part of the glacier. Luckily for us, by hurrying on we passed this danger during an interval, but had scarcely got beyond its reach when one of the most tremendous avalanches that we had witnessed came rolling down, sending blocks of solid ice of many tons in weight to within a few yards of where we stood, while the place we had so recently passed was overwhelmed with smaller fragments and masses of rock. The height from which this ice cascade descends is about 1500 feet, and the mass of ice thus formed into a glacier is probably not less than 500 feet in thickness at its upper end, and at its lower about 100 feet. Its length is five miles, and in that distance the slope of its surface amounts to 1100 feet.

"We occupied the same time in crossing Haast's Glacier as that in descending the smaller one already alluded to, viz., 2½ hours, and at its completion we encamped a few yards below where the river of that name commences its hurried course. Four and nine miles from its source, the river passes through gorges with an enormous velocity, and so rapid is the descent of its valley, that viewing tall trees at a comparatively short distance off, their tops are seen on a level with the eye. Dr Hector ascertained its fall in this part of its course to be 7 in 30. Continuing our course for about six miles further, we were arrested in our progress by a precipitous cut cliff, which for about 100 feet above the surface of the river is quite devoid of vegetation, and presents a perpendicular smooth surface. Considering that we had been travelling the whole distance down the stream in a northerly direction, and thereby penetrating into the province of Canterbury, Dr Hector felt that he was thus on ground beyond the limit of his especial duties. Having, however, taken so much trouble in contending with the difficulties of this district, he would obtain as much information as possible relative to the mountainous region to the westward, before proceeding any further by the valley of the Haast. The following morning we proceeded along the summit of the ridge to the highest point of the mountain, when, to our great delight, the ocean shore lay stretched before us at the distance of about fifteen miles. From this point the whole course of the river by which we had descended was accurately traced, and it was found to sweep through a perfect canon at a distance of two miles above the point at which it debouches into a river, the valley of which ranges from three to five miles in width, and whose volume of water is at least equal to the Molyneux where it leaves the Wanaka Lake. From careful bearings, which Dr Hector had taken throughout the route we had followed, we learnt that what we saw was Jackson's Bay; he therefore named the river which flows into it the Jackson.

"The sight of the ocean was too great a temptation for us to be satisfied that we had proceeded far enough, and, although our provisions were

almost entirely consumed, we determined to push on. Keeping along the ridge, which forms the northern boundary of a thickly timbered valley that leads into that of the Jackson, we travelled for about one and a half miles before commencing the descent. From the ridge, the slope down to the former is grassy and undulating, with no timber, but dotted thickly with small lakes surrounded by peat. The whole surface of this grassy slope is cut up by numerous rents or fissures, indicating, probably, that this region had been formerly visited by earthquakes.

"It was about noon before we descended into the woods; the thick masses of decayed vegetation, overgrown by mosses and undergrowth, served to fill up the spaces between the rugged angular masses of rock, which on this portion of the Pigeon Mountain seemed to be detached from the mountain side. Great care was taken in passing over this first part of the journey, as sometimes treacherous holes lay concealed beneath a spongy layer of moss and rotten wood. The tomatocowra, that painful impediment to the traveller, does not exist in the woods of the west, and the bayonette grass, equally cruel, is of exceedingly rare occurrence. This is undoubtedly owing to the woods having never been devastated by fires. The latter fact is remarkable, and affords a strong presumption that for a great number of years perhaps never have these localities been traversed by man. A succession of deep descents, occupied by splendid timber (the *Totara* being the forest king) led us to the edge of the most thickly-clad flat which occurs at the forks of the Jackson and Haast Rivers. Just as darkness had set in we struck the margin of the latter, and, for the first time since starting, retired to rest supperless.

"The magnificence of the primeval forests of the West Coast must be seen to be thoroughly appreciated. Not only do the *Totara* and beech attain gigantic proportions, but the *Fuchsia* and *Tutu* become very large trees, averaging two feet in thickness.

"Our camp on the bank of the Haast was uncomfortable in the extreme; the rain poured down on us in torrents, saturating us in a short time, while the myriads of mosquitoes made a sortie from the bush, and kept us in a perfect state of fever the whole night. With the dawn of the following morning, the 23d, in a tropical-like flood of rain, and with no breakfast, we were fortunate enough to strike probably the only ford for some distance at about a mile above the forks, and proceeded by the flats about four miles down the valley of the Jackson. The rain had evidently set in for a lengthened period, and although this was the first long continuation of wet weather we had experienced since leaving Dunedin, it was a circumstance greatly to be regretted, seeing that we were now only about eight miles from the sea, and the rapidly increasing flood of the river was the greatest obstacle to our reaching its mouth. Taking into consideration also the density of the woods, and the distance we should have to travel before obtaining further supplies, Dr Hector thought it advisable to abandon the project of descending the valley further. The accomplishment of the journey to the very brink of the ocean, under present circumstances, would have been at a great risk; while, on the other hand, with sufficient provisions and moderate weather, the work would have been the easiest which had fallen to our share since leaving the central camp on the Matukituki."

List of Publications received is postponed till October Number.

THE
EDINBURGH NEW
PHILOSOPHICAL JOURNAL.

*St Michael's Mount and the Phœnicians.** By R. EDMONDS,
Esq.

To the astonishment of Cornishmen, especially of those best acquainted with the subject, it was at the Truro meeting of the Cambrian Archæological Association in August last, gravely questioned whether the Phœnicians ever visited Cornwall. It is true that some authors have lately contended that the tin in Canaan in the days of Moses (Num. xxxi. 22) must have come from India. But they have done so without a tittle of evidence; there being no more reason for supposing India to have supplied Canaan or Egypt with tin 3000 years ago, because tin is now so largely exported from the Isle of Banca, than for imagining Cornwall to have then exported copper as well as tin because it does so now. Other authors have contended that the tin then in Canaan came from Tartessus, near Cadiz, the Tarshish of Scripture (Jon. i. 3; Isa. xxiii. 6; Ezek. xxvii. 12). But if Tartessus, or any other part of Spain, had in ancient times produced much tin, it would in all probability, like Cornwall, have continued to do so to the present day. No tin mines, however, appear to have ever existed in Spain, as Dr George Smith was lately informed by the College of

* Read (with the exception of the postscript) at the Annual Meeting of the Royal Institution of Cornwall on the 29th of May 1863. Communicated by the author.

Mines in Madrid in answer to his inquiries. "I cannot learn," says the writer from Madrid, "that Spain ever produced any quantity of this metal. The Government do not work any mines of tin. The quantity being produced at present is very small, chiefly by streamers, near the granite hills in Galicia and Zamora. I cannot learn that there is any tin-mining in the country." This quotation is from a note prefixed to an excellent little work on the *Cassiterides*, published this year by Dr Smith, wherein he shows, by the most satisfactory historical and other evidence, that the Phœnicians never procured their tin from India, but always from Cornwall, with the exception perhaps of a small portion from Tartessus.

In addition to what has been stated by this learned author, I would mention a few more things worthy of being recorded.

That Mountsbay was known to Greek historians before the time of Herodotus, and as far back at least as the 6th century B.C., is shown by the following quotation from Diodorus, who was evidently not aware that the locality described by Hecatæus and the other historians was the very Belerion which he himself has described, where the tin was dug out of the earth, and near which was the island Iktin. To those, however, who are familiar with Mountsbay, this identity is very apparent. "Amongst them that have written old stories much like fables, Hecatæus and some others say that there is an island in the ocean, over against Gaul (as big as Sicily), under the arctic pole, where the Hyperboreans inhabit, so called because they lie beyond the breezes of the north wind. That the soil there is very rich and very fruitful, and the climate temperate, inasmuch as *there are two crops in the year*." The island here referred to *can* be no other than Britain; but the only part of Britain to which the description applies is Mountsbay, where still "*there are two crops in the year*." Whether the other writers alluded to by Diodorus lived before or after Hecatæus, we are not informed; but as Hecatæus was born 549 B.C., we may conclude that the fact recorded was known in Grecian history as far back at least as the sixth century B.C.: and from whom is it so likely to have been first related as by the Phœnician mariners, who were

then, and had been for many centuries before, the greatest and most extensive navigators in the world !*

Let us proceed with the quotation. "They say that Latona was born there, and therefore that they worship Apollo above all other gods. . . . They say, moreover, that Apollo once in nineteen years comes into the island, in which space of time the stars perform their courses and return to the same point, and therefore the Greeks call the revolution of nineteen years "the Great Year." This cycle of nineteen years, at the end of which the new and full moons happen within an hour and a half of the same times of the year, as they did at the beginning, and by which Christians have always regulated their moveable festivals, was thus apparently known to the Druids in Britain ages before Meton discovered it in 430 B.C. To it, therefore (and not to the twelve months in the year and the seven days in the week, as Borlase imagined), the four "Druidical Temples," within six miles of Penzance, may have pointed. These consisted originally each of nineteen detached stones or unhewn pillars placed upright, from three to five feet above ground, in rude circles, the smallest circle being about sixty-five, the largest about eighty feet in diameter. Each circle is vulgarly, and has been immemorially, called the "nine maidens," an abbreviation doubtless for "nineteen maidens." This I mention because some modern authors have, contrary to Borlase, contended that the original number of pillars forming each circle was more than nineteen, as it has a space between two of its pillars much wider than that between any other two. But this widest space may have been originally intended for the passing in and out of religious processions. The name *Belerion*, anciently given to the Landsend district, and the name *Druids*, by which the priests there were called, because they performed their religious rites within groves of oaks (Hos. iv. 13), as did the worshippers of *Bel* or Baal in Palestine, are quite in keeping with the supposed Phœnician origin of these four very ancient temples.

* Josh. xix. 29 ; Is. xxiii. 8, 7, 8 ; Ezek. xxvii. 8, 4, 12, 88. It is stated in Haydn's Dictionary of Dates (8th edition) under the word TIN—"The Phœnicians traded with England for this article more than 1100 years B.C."

As Mountsbay is the only place that answers to the description referred to by Diodorus, so the Mount which gives name to that bay is the only place that coincides with Diodorus' description of Iktin. I say Iktin, because Diodorus calls it by no other name. His translators (French and English as well as Latin), assuming this barbarous name to be declinable, and finding it in the accusative case, have concluded Iktis to be the nominative, although, if it *were* declinable, Iktin might have been the nominative with as much grammatical propriety as Iktis,—particularly as the Greek word *tis* does not give *tin* at all for the accusative, but *tina*. Had they any doubt which it should be, a moment's reflection would have shown that the nominative here must, in order to agree with Diodorus' account of the mount, be *Iktin*, "Tin-port," inasmuch as Iktis has no reference whatever to tin. Were it not for this misnomer in the translations, no author probably would ever have contended that the Isle of Wight, the ancient Vectis, was the Iktin of Diodorus. Many have done so, at various times; but after the richly deserved criticisms by Dr Smith, to which they have been subjected, none, I think, will venture to do so again; not even the very bold writer in the *Saturday Review* of 8th November 1862 (p. 563), who, finding no place but St Michael's Mount to agree with Diodorus' description of Iktin, tries to stultify that author by alleging that he says, "not only the Isle of Wight, but all the islands between Gaul and Britain, can be reached from the mainland at low water," although, as we shall presently see, Diodorus says no such thing. The reviewer, in giving us what he thought the meaning of the author, seems to have forgotten that Diodorus Siculus lived in an island of the Mediterranean, and wrote for those who knew nothing about the ebbing and flowing of the sea, so that after having described the Isle of Iktin as accessible by carts "during the recess of the tide," he was obliged to explain himself, and to illustrate the effects of the tides, by adding,—“and it is something peculiar that happens to the islands in these parts lying between Europe and Britain; for at the full tide, the intervening passage being overflowed, they appear islands, but when the sea retires a large space is left dry,

and they are seen as peninsulas." This explanation of what his countrymen would have regarded as a most improbable phenomenon, is literally true; for he does not say, as the reviewer imagines, that *all* the islands between Europe and Britain came within this description—nor indeed that any of them did, which were in deep water like the Isle of Wight; but he speaks only of those which, like the Mount, were in shallow water and on the coast.

The only reason given by the reviewer for considering the Isle of Wight to be the ancient Iktin, is its being nearer than the Mount to Gaul, and because Diodorus says that at Iktin "the traders purchase the tin of the natives, and transport it into Gaul; and, *finally*, travelling through Gaul on foot, in about thirty days they bring their burdens on horses to the mouth of the river Rhone." This passage, particularly the word *finally*, which I have written in italics, leads me to conclude that the tin was carried most of the way from Iktin to the mouth of the Rhone by sea, and therefore probably to the inmost part of the Bay of Biscay, and from thence by land to Marseilles, at the mouth of the Rhone, to avoid sailing round the Spanish peninsula, as was the more ancient route while Tyre and Tarshish were flourishing. And it is remarkable that the sandy parish of *Lelant* in the Bay of St Ives, only four miles from the Mount, is called by the same name as *Les Landes*, the sandy coast of the Bay of Biscay (*t* and *d* being interchangeable letters), as if the one name had been derived from the other through the tin-traders, whomust have been well acquainted with both places. Moreover, the carriage by land from the *north* of France to the Mediterranean, as the reviewer reads Diodorus, would, in those rude ages, have been not only more expensive, but more unsafe than by the route I have mentioned.

As it is now established beyond all reasonable question, that the Phœnicians came to Mountsbay for tin, both before and for many centuries after the time of Moses, and that the Mount under the name of Iktin, is the most ancient British port known to history; it will be interesting, in conclusion, to consider the derivation of this name, and of the name of the island in which we live.

Ik is the Cornish for "port;" *tin* is the metal so called

(as is supposed) by the Phœnicians, and the name continues unchanged in the Saxon, English, Dutch and Danish languages; but the Swedish name is now *tenn*, the German *zinn*, the French *etain*, the Latin *stannum*, the Irish *stan*, the Cornish *stean*, the Armoric *stean*, and also *staen*; the first letter of each of the last five names being a mere prefix, as in the modern word *sneeze* for *neeze* (Job xli. 18). With this exception, and except the termination of the Latin name, these eight different spellings are merely different ways in which different nations pronounce *tin*. *Iktin*, therefore, signifies "tin port;" and the Mount was probably thus called by the foreign tin-traders who frequented it. The Cornish or ancient Britons would have called it *Tinik*, although *porth*, another Cornish word synonymous with *ik*, rarely follows, but almost always precedes, the word to which it is joined. When other tin-ports began also to be frequented, the foreign traders may, by way of distinguishing it from the Falmouth or other tin-ports, have called it *Bre-tin-ik*, "the Mount-tin-port," *Bre* being the Cornish for "Mount;" and euphony as well as the British mode of collocating these three words, would have prevented them from calling it *Bre-ik-tin*. Assuming this to be the true derivation, it is a beautiful illustration of the fact of ancient names being originally faithful descriptions of the places or things to which they were given. For *Bre-tin-ik*, "the Mount-tin-port," was not only a mount, and one of the strongest natural fortresses to protect the tin carried thither from the mainland, but also the safest port in the bay for ships to remain in until laden for a foreign voyage. And this name, although so appropriate, was at the same time as uncommunicative of its locality as could have been desired by the Phœnicians, who, as is well known, sought to conceal the place whence they procured their tin.

As a name originally confined to a very small part of a province or country often becomes the name of the whole, so the Mount, which gives its *modern* name to the finest bay in England, may have given its *ancient* name to all Britain, not only because it was the most striking object, and the most important place in our island known to the Phœnicians, but also because this ancient name (*Bre-tin-ik*) told no more

of its locality than did the name *Cassiterides*, "Tin Islands," used by Herodotus B.C. 445. And *Bre-tin-ik* is almost *literatim* not only the name (*Brettanike*) by which Diodorus calls Britain, but also the original Phœnician or Hebrew word (*Baratanac*, "the land of tin") from which, according to the learned Bochart, our island was called.

To Cornishmen, the last survivors in England of the ancient Britons, this subject must be particularly interesting, especially when we regard Tyre, in its most flourishing condition, as typical of England at the present time,— "very glorious in the midst of the seas;" "whose merchants are princes;" and who hath been for ages, and still is "the covering cherub" (Ezek. xxviii. 14, 16), beneath whose wings fugitives from their own lands, for offences not deemed by us "worthy of death or of bonds," have always found a secure asylum.

P.S.—Since writing the above I have seen the pamphlet just published of Colonel Sir Henry James, R.E., director of the Ordnance Survey, entitled "Note on the Block of Tin dredged up in Falmouth harbour." This block, presented forty years ago to the Royal Institution of Cornwall, weighs about 130 lbs., and appears to be of the very form (*αστραγαλὸν ῥυθμοῦς*) into which Diodorus says the tin was cast before it was carried in carts to Iktin. "It is 2 feet 11 inches long, 11 inches wide, and 3 inches thick at the centre, perfectly flat on one side, but curved on the other, and having four prolongations at the corners, each one foot long;" thus resembling in its outline a London butcher-boy's tray, and well adapted for being carried by hand by two men; for being firmly placed on the curved bottom of a boat for exportation, and for being afterwards strapped, two of them together, to a pack-saddle with their flat sides against the sides of the horse. A plan of it is given below. The boat containing this block is supposed by Sir Henry to have been lost on its voyage from St Michael's Mount to France, at the entrance of Falmouth harbour, where it was dredged up. This publication will be printed in the next Report of the Royal Institution of Cornwall, together with a letter to Sir Henry, dated 16th June 1862,

from the late Sir George C. Lewis, stating he was satisfied that St Michael's Mount "was the port from which the tin was shipped for the coast of Gaul." The pamphlet thus concludes, "I do not think it necessary to discuss the claims of the Isle of Wight to be considered the Iktis, as the description of the place does not now, and could not at any time have applied to it in any one particular." This agrees with my own remarks in respect of the Isle of Wight.

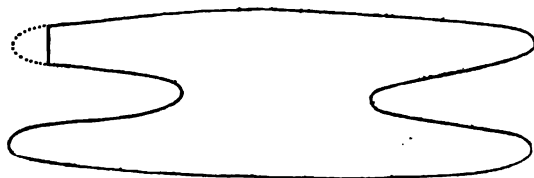
Sir Henry has confined himself to the places where this block of tin was shipped and lost. I have elsewhere* pointed out the very spot where it was probably cast into its most singular shape before it was carried to its place of shipment. This spot is at the mouth of the stream, half a mile north of the Mount. By a temporary diversion of the stream in 1849, a deep section was made in the sand-hillock forming its western boundary. On that occasion my nephew (Frederic Bernard Edmonds) and myself discovered, two or three yards beneath the surface, remains of ancient walls rudely built of unhewn stones, and close by them great quantities of ashes, charcoal, slag, broken pottery of very rude manufacture, brick, and *fragments of a bronze vessel* resting with their outsides on charcoal. A considerable portion of the copper had combined with the charcoal during the lapse of ages, and a beautiful green substance had resulted—the carbonate of copper. Two of the fragments, each about 6 inches long, 4 wide, and only the sixteenth of an inch thick, had been evidently parts of the circular top or edge of a furnace (3 feet in diameter) bent back into a horizontal rim, three quarters of an inch broad. No charcoal was on the insides of the fragments, but their outsides were completely blackened and covered with it. Diodorus speaks of two fusions; the tin ore being no doubt, as at present, first *smelted* in contact with the fuel; and the metal, thus purified, being afterwards *melted* in furnaces by fire applied only externally. For the latter process, this very ancient bronze† furnace was probably

* The Land's End District, p. 9.

† In Liddell and Scott's Greek-English Lexicon (abridgment), 1859, it is stated that "Tin was usually melted and cast upon bronze." But it is now melted in *iron*, and the diameter of the *iron* furnace used at Messrs Bolitho'

brought and used by the Mediterranean traders as long as it continued fit for service. When the traders ceased to visit Mountsbay these forsaken ruins were gradually and completely buried beneath the sand driven in by the winds from the shore, and the turf had from time immemorial until 1849 being growing over them.

One of the bronze fragments I presented, soon after their discovery, to the Museum of Economic Geology in London, where there is also a cast of the block of tin described by Sir Henry. The other fragments, with some of the charcoal and slag, I have now presented to the Royal Institution of Cornwall, so that the block itself and a part of the furnace in which it may have been melted, may be seen, side by side, at its Museum in Truro, confirming, in a most singular manner, a passage in the history of Britain written before the Christian era.



Descriptions of New Genera and Species of Diatoms from the South Pacific. By R. K. GREVILLE, LL.D., F.R.S.E., &c.* Part II. (Plate IV.)

GRAMMATOPHORA.

Grammatophora pusilla, n. sp. Grev.—Small; septa undulate, with the inner ends incurved; undulations becoming obsolete towards the margin; striæ obscure. Length of frustule about '0018". (Plate IV. fig. 15.)

Hab.—Curteis Straits, Queensland, in a dredging communicated by Dr Roberts of Sydney.

Melting House, near Penzance, is very nearly the same as was that of the bronze furnace now described.

* Read before the Botanical Society of Edinburgh, 9th July 1863.

A very delicate little species, coming nearest to *G. anguina* of Kützing, but differing remarkably in the character of the undulations of the septa, as will be perceived at once by a reference to the figures. In *G. anguina* the undulations are equal, or nearly so throughout; while in our present species the lower ones are scarcely perceptible, and each septum near the margin resembles a sort of footstalk to the upper (or inner) part. I find the number of the undulations to vary, the difference not apparently depending upon the size of the frustule.

AMPHORA.

Amphora magnifica, n. sp. Grev.—Frustules large, rectangular, slightly constricted, truncate at the ends; outer portion only visible in the middle, where it is narrow and inflected; nodule minute; dorsum with numerous longitudinal lines (about 10 in '001"), and brilliant scattered puncta. Length '0040" to '0055". (Plate IV. fig. 1.)

Hab.—Curteis Straits, Queensland.

The finest species of the genus. The whole surface is covered with numerous longitudinal lines, which have a plicate appearance, and are sprinkled over with luminous gland-like puncta. The outer portion is inflected in the middle for little more than a third of the length of the frustule.

Amphora pulchra, n. sp. Grev.—Frustules large, constricted, somewhat panduriform, with the ends truncate; outer portion very narrow, disappearing before reaching the rounded corners; dorsum with very numerous longitudinal lines, and four brilliant submarginal puncta on each side. Length '0040" to '0050". (Plate IV. fig. 2.)

Hab.—Curteis Straits, Queensland.

Evidently related to the preceding, but differing in the much greater constriction, in the more numerous longitudinal lines, and in the luminous puncta being limited to four on each side (two in each lobe, one of them being near the angle, and the other at no great distance from it).

Amphora undulata, n. sp. Grev.—Frustules rectangular, with truncate shortly produced ends, and four sub-equal in-

flations; striæ coarse, 14 in $\cdot 001''$. Length $\cdot 0030''$. (Plate IV. fig. 3.)

Hab.—Curteis Straits, Queensland.

This is a very conspicuous member of a little group, the frustules of which are distinguished by their constricted or sinuated margins. Three species—*A. binodis*, *angularis*, and *lyrata*—were discovered and described by my late indefatigable friend, Professor Gregory, and one, *A. Sarniensis*, was recently added by myself, from a dredging made in Guernsey by Dr Wallich. The present is the most robust species of the section, and is rectangular in shape, with the corners rounded. The whole surface is undulated.

Amphora flexuosa, n. sp. Grev.—Frustules constricted, nearly linear, with truncate produced ends, and six gentle undulations having a marginal row of very minute puncta; Nodule situated at the angle of the constriction. Length $\cdot 0034''$. (Plate IV. fig. 4.)

Hab.—Curteis Straits, Queensland.

A comparison with the following, which has also six undulations, will show at a glance that our present diatom is a genuine species.

Amphora sinuata, n. sp. Grev.—Frustules narrow, oblong-elliptical, with truncate shortly produced ends, and six undulations, the two middle ones being the largest; nodules situated at a distance from the middle sinuation; striæ obscure. Length $\cdot 0028''$. (Plate IV. fig. 5.)

Hab.—Curteis Straits, Queensland.

In the preceding species there is a true constriction, the nodule occupying the angle; in the present one there is no proper constriction, but a concave sinuation, the nodule being situated apart from it on an inner longitudinal line.

AMPHIPRORA.

Amphiprora Thwaitesiana, n. sp. Grev.—Front view somewhat rectangular, broadly truncate at the ends, the wings much rounded at the corners and then widely concave; supplementary wings striate, convex, constricted at the nodule, intersecting the primary wings, so as to divide the whole length into about three equal parts. Length $\cdot 0040''$. (Plate IV. fig. 9.)

Hab.—Curteis Straits, Queensland.

A most distinct species, and attractive from the graceful manner in which the wings intersect one another. The primary wing makes a concave sweep from the rounded corners, and touches the central portion at the nodule. I have not been able to detect the striæ in this part. The supplementary wing makes a sweep in the opposite direction, intersecting the primary one at two points, and is so constricted in the middle that the nodule lies in the angle, and appears to serve both wings. The striæ in the supplementary wing are conspicuous.

Amphiprora Kützingiana, n. sp., Grev.—Front view broadly and rectangularly oblong, truncate at the ends, with widely rounded corners, and somewhat straight sides, suddenly and deeply constricted; supplementary wing commencing obliquely at the ends, then forming a swelling curve, which keeps within the margin, and passes down to the nodule, where it appears to be fixed; central portion with point longitudinal lines; striæ obscure. Length .0050". (Plate IV. fig. 6.)

Hab.—Curteis Straits, Queensland.

Panduriform and lyre-like in its general aspect, partly from its shape, and partly from the peculiar curve of the supplementary wings, and the longitudinal lines of the central portion. In this, as well as in the preceding species, the supplementary wing does not form a single curve as usual, in passing either within or beyond the angle of the constriction, but is itself constricted, and in the present instance very nearly follows the curve of the primary wings as they approach the nodule. I have not been able to resolve the striæ in any part of the frustule.

Amphiprora Meneghiniana, n. sp., Grev.—Front view broadly subrectangular, the ends truncate, wings widely rounded at the corners, with a wide and rather deep constriction; supplementary wings narrow, convex, widely and very slightly concave opposite the nodule; all the wings striate, striæ about 15 in .001". Length .0045". (Plate IV. fig. 7.)

Hab.—Curteis Straits, Queensland.

A very fine strongly margined species, with both primary

and supplementary wings conspicuously striate, the striæ of the former having the appearance of being radiate at the rounded corners. The supplementary wings commence with a curve which, if continued, would carry them beyond the angle of the constriction; but it is soon arrested, and becomes nearly straight, or very slightly concave, and passes just within the line of the nodule. The central portion is marked with a few wide, faint, longitudinal lines.

Amphiprora Brebissoniana, n. sp., Grev.—Front view broadly panduriform, truncate at the ends; wings widely rounded at the corners, and then rapidly inclined in a deep constriction; supplementary wings very narrow, widely, and slightly concave opposite the nodule, striæ conspicuous only in the supplementary wings, about 22 in '001". Length '0035". (Plate IV. fig. 8.)

Not very dissimilar in general appearance to the preceding, but marked nevertheless by the shorter frustule, and more abrupt and straight outline of the wing between the rounded corner and the constriction. A closer examination brings out the apparent absence of striæ in the primary wing, and considerably more numerous striæ in the much narrower supplementary wing. The central portion has a few faint longitudinal lines.

Amphiprora rectangularis, n. sp., Grev.—Front view rectangular, the ends very slightly rounded; wings with the sides nearly straight, slightly constricted; supplementary wings convex, overlapping the primary ones only at the notch-like constriction; striæ very fine. Length '0035". (Plate IV. fig. 10.)

Hab.—Woodlark Island, in a dredging communicated by Dr Roberts of Sydney.

This approaches near to *A. pusilla* of Gregory (*Trans. Roy. Soc. Edin.*, vol. xxi., Pl. IV., fig. 56); but after examination, I am satisfied that it is distinct. It is considerably larger and more robust. The supplementary wings are stronger, and form a flattened curve, becoming parallel with the straight sides of the primary ones, and consequently only overlapping the notch which constitutes the constriction.

Amphiprora? Rabenhorstiana, n. sp., Grev.—Front view .

elongated-oblong; the wings converging towards the shortly truncated ends, gradually constricted in the middle, and edged with a border of exceedingly short striæ, 16 in $\cdot 001''$, no supplementary wing apparent. Length $\cdot 0055''$. (Plate IV. fig. 12.)

Hab.—Curteis Straits, Queensland.

I feel so much uncertainty regarding this diatom, that its present position must be held as merely provisional. The general contour is as if it were composed of two broadly elliptical lobes. The coarse striæ are so very short, that they form a mere edging, but showing at the same time a radiating direction.

Amphiprora Jolisiana, n. sp., Grev.—Front view oblong, with perfectly rounded ends and gently constricted sides; wing resembling a narrow border, with a middle row of puncta; central portion oblong, with an apparently prominent edge, and marked with a few faint longitudinal lines. Length $\cdot 0032''$. (Plate IV. fig 11.)

Hab.—Curteis Straits, Queensland.

A beautiful and distinct little species, without any evident supplementary wing. The puncta are conspicuous, seven or eight in $\cdot 001''$; but I cannot see any striæ.

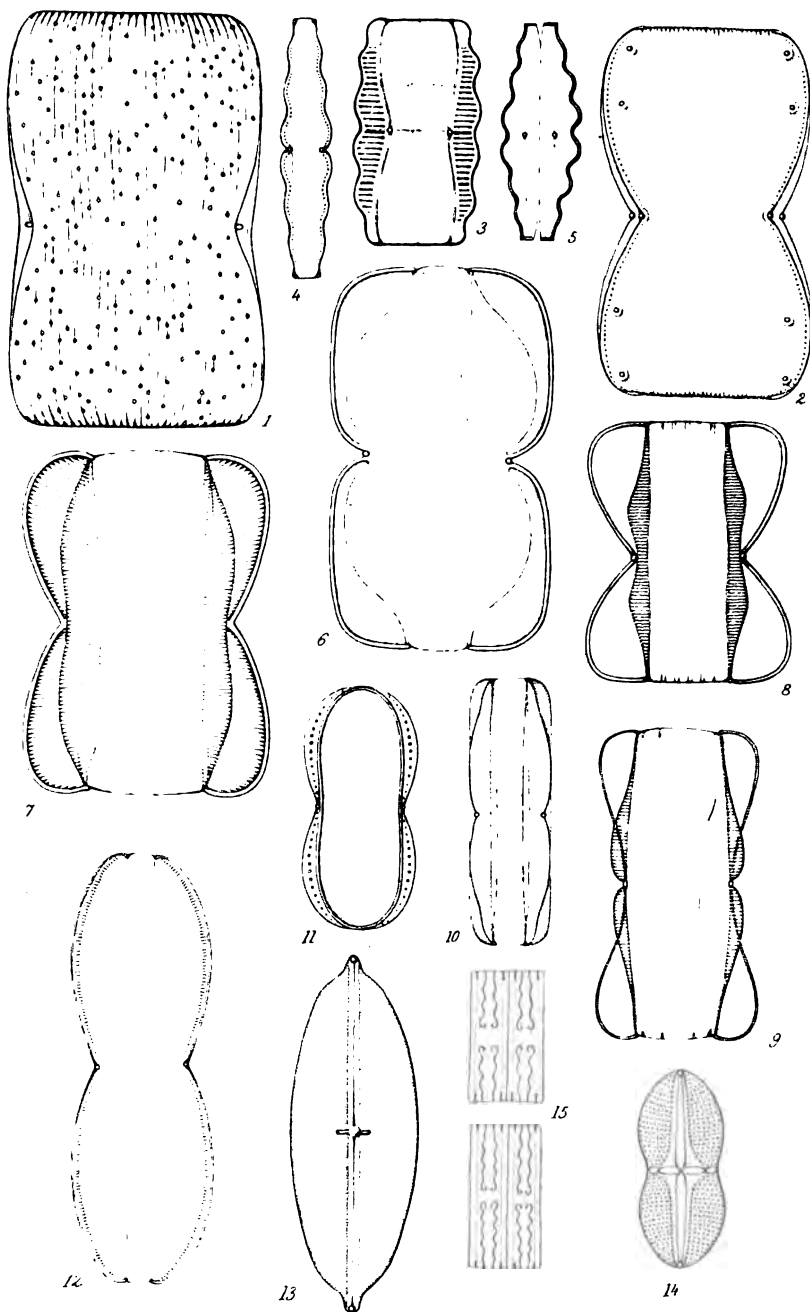
NAVICULA.

Navicula diversa, n. sp., Grev.—Oblong, slightly constricted at the middle, the lobes ovate-elliptical, obtuse at the ends; striæ moniliform, interrupted by a transverse bar composed of two elongated cellules on each side opposite the nodule, where an angular space is also left by the striæ receding from the median line towards the constriction. Length about $\cdot 0025''$. (Plate IV. fig. 14.)

Hab.—Curteis Straits, Queensland.

The most characteristic feature in this small species is the transverse bar, which shows an interruption of the striæ extending to the margin. This bar is composed on each side of the nodule of two unequal cellules, the one next the nodule being much more elongated than the other. The true median line is very fine, while the extra ones are strongly marked; striæ conspicuously moniliform, 12 in $\cdot 001''$.





STAURONEIS.

Stauroneis Australis, n. sp., Grev.—Elliptical-oblong, bluntly apiculate; striæ not reaching the median line, 22 in '001"; stauros very short and narrow, but well-defined. Length '0047". (Plate IV. fig. 13.)

Hab.—Harvey Bay, Queensland, in a dredging sent by Dr Roberts.

A rare species, and sufficiently distinguished by the exceedingly short, narrow, and sharply defined stauros.

DESCRIPTION OF PLATE IV.

- Fig. 15. *Grammatophora pusilla*, . . . front view.
 1. *Amphora magnifica*.
 2. " *pulchra*.
 8. " *undulata*.
 4. " *flexuosa*.
 5. " *sinuata*.
 9. *Amphiprora Thwaitesiana*, . . . front view.
 6. " *Kützingeriana*, . . . front view.
 7. " *Meneghiniana*, . . . front view.
 8. " *Brebiissoniana*, . . . front view.
 10. " *rectangularis*, . . . front view.
 12. " *Rabenhorstiana*, . . . front view.
 11. " *Jolissiana*, . . . front view.
 14. *Navicula diversa*, . . . side view.
 18. *Stauroneis Australis*, . . . side view.

All the figures are x 400 diameters.

The Bee-hive British Dwellings at Bosphrennis and Chapel Euny, near Penzance. By R. EDMONDS, Esq., Plymouth.*

The recently discovered "Bee-hive Hut" at Bosphrennis in Zennor which attracted so much attention at the Truro meeting of the Cambrian Archæological Association in August last is described in last month's *Archæologia Cambrensis* (pp. 120–129) by our Cambrian friend, E. L. B. He thinks, however, that "no similar remains in the same

* Read at the Annual Meeting of the Royal Institution of Cornwall, on the 29th of May 1868.

perfect state are known in Cornwall." But in the Reports of this Institution for 1857 and 1858, and also in my published account of the *Landsend District* (p. 52), I have noticed a similar and probably a more perfect one five miles S.S.W. of it, which I accidentally met with at Chapel Euny, in Sancreed, and which I called a "Bee-hive cave," because it was originally, with the exception of its entrance, buried beneath thick turf. I suspect this was the case also with the bee-hive dwelling at Bosphrennis, and that the opening described as "the small window" was made merely for ventilation. The two dwellings resemble one another in most respects so closely that I see no reason why they should not have been likewise both *caves* with their entrances concealed by furze or other evergreens, like the well-known longitudinal cave at Boleit in St Buryan. In fact, the little that remains of the roof at Bosphrennis is still covered with turf.

It appears to me that in the Landsend District isolated British dwellings, having no central areas open to the sky, were, with the exception of their concealed entrances, generally buried beneath heaps of earth or stones, and then covered with turf or growing furze, briers, and thorns, thus appearing externally, if they rose above the level of the adjoining land, as mere natural elevations, the tops or roofs of which were used by their occupants as watchtowers. This I consider to have been the original description of the Bosphrennis dwelling. For when thus buried, it would have been difficult for an enemy either to discover or to destroy it, whereas if it were a mere hut, with walls all exposed as at present, it could have been easily discovered and quickly pulled down. Moreover, such an unprotected dwelling would have been quite out of place in the Landsend district, where the remains of numerous strongly fortified hill castles, cliff castles, and walled towns and villages, indicate a locality that was once continually exposed to hostile incursions. If it be asked, where are the traces of this dwelling having been ever thus buried? I ask in return, where are the traces of the huge mound of earth within which we know that the large Cromlech of West Lanyon in Madron was completely buried until its discovery

in 1790, when the mound was removed for agricultural purposes?

The longitudinal chamber of the cave at Chapel Euny is on a lower level than the bee-hive chamber into which it leads, and is therefore furnished with an external entrance, as in the Irish example referred to at p. 128, whilst the bee-hive chamber is without any external entrance. At Bosphrennis, on the contrary, the bee-hive chamber is the outer-room, and possesses the only external entrance, because that room is apparently (p. 123) on a lower level than the longitudinal one into which it leads. When a cave or hut on sloping ground has only one entrance, that entrance is, no doubt, generally at or near its lowest accessible part, whilst the highest, driest, and inmost part, is used chiefly as a sleeping apartment.

The length of *each* dwelling is in a straight line running nearly north-west and south-east, of which the bee-hive chamber occupies about a dozen feet at the north-west end; the remainder of the length is occupied by the longitudinal chamber, which is only 9 feet at Bosphrennis, but 18 feet at Chapel Euny. I do not call the latter chamber rectangular, because its side-walls are probably nearer each other at the roof than at the base, by reason of the stones overlapping one another. As these walls and roof do not appear to have been ever disturbed, the Penzance Natural History and Antiquarian Society might, for the sake of their being examined and described, well lay out a few pounds in removing the heap within which they are now concealed.

E. L. B.'s paper contains another passage on which antiquarians will differ in opinion. It is this (p. 122):—"Those members who visited the stone-works at Carn Goch, in Caermarthenshire, in 1855, will remember that in the thickness of the walls were discovered low galleries formed by horizontal and vertical slabs, which were thought to have been used for the passage of sheep or goats in leaving or returning within the works. If they were intended for concealed sallyports for the use of the defendants, they could have been passed only on the hands and knees." Borlase considered somewhat similar remains near Penzance (now entirely removed) to have been part of a British vil-

lage, and a like low gallery (5 feet high) within the thickness of the walls, to have been a private entrance into it.* But I have elsewhere† shown that such low narrow galleries or roofed spaces between two parallel walls, especially when those walls are concentric, and enclose an area open to the sky, leaving only one external entrance, were probably sleeping places, divided by transverse walls into separate apartments, all opening into the area—these sleeping-places being buried beneath an annular mound of earth or stones covered with turf, furze, and thorns, except where the apartments opened into the area. A cluster of such dwellings constituted a British village.

P.S.—I saw the Bosphrennis dwelling after the above paper was read, but so transiently, owing to an unexpected fall of rain, that I mention it merely to add, that in going thither I observed, close to the south side of a small meadow called the Vineyard, into which the Bosphrennis residence‡ opens, large portions of the walls of what appeared to me to have been a cluster of British dwellings: and the uncemented stones forming the modern high walls enclosing the meadow were in all probability taken from these dwellings.

Notice of Observations by F. COHN, Breslau, on the Contractile Filaments of the Stamens in Thistles.§ Communicated by Dr ALEXANDER DICKSON.||

The following is the substance of a letter addressed to Professor C. Von Siebold, in which the author details his observations on the contractility in the filaments of the stamens in thistles:—The five anthers cohere, forming a

* Antiquities, p. 278.

† Landsend District, pp. 45, 47.

‡ The villagers of Bosphrennis would laugh at this being called a *residence*, although in its day it was doubtless no mean habitation. They call it a *crow*, which is a Cornish word (pronounced like *crow* in *crowd*) signifying a "pig-gery."

§ Ueber die contractilen Staubfäden der Disteln. Ein Sendschreiben von Ferdinand Cohn in Breslau an C. v. Siebold. Siebold and Kölliker's Zeitschrift für Wissenschaft. Zoologie, XII. p. 866.

|| Read before the Botanical Society of Edinburgh, 9th April 1863.

tube. At the time of flowering, this tube is shut in at the top, and encloses the style. About this period the anther-tube rises to about 4 millimeters above the extreme points of the corolla; and if the same is touched, pollen in lumps issues from its summit, the anther-tube at the same time undergoing a remarkable twisting. After an interval—say five minutes—the experiment may be repeated: pollen issues anew from the anther-tube, and twisting movements are executed as before. The style gradually becomes elevated above the summit of the anther-tube, and by the time it projects about 4–5 millimeters beyond, the irritability has completely disappeared. At this time the style becomes fitted for fertilisation, since the two branches of the stigma now first diverge from one another. In general, a period of at most twenty-four hours, often less, elapses between the commencement and cessation of the irritability. When the styles are visible, it is too late for instituting experiments.

These phenomena are produced solely by the contraction of the filaments of the stamens, which, on each touch, instantly contract, and after a little resume their former length. The expulsion of pollen depends upon the anther-tube being drawn downwards upon the style by the contracting filaments, and then pushed up again. The contractility of the filaments is most interestingly shown, if the stamens are cut away from the corolla. In such a preparation there is the anther-tube with the five filaments depending therefrom; and these latter, on every touch, exhibit the liveliest irritability,—throw themselves backwards, straighten themselves again, bend to the opposite side, and wind round one another. One might fancy that it was a *Hydra*, rather than a vegetable organism. These motions had been previously described by Cohn himself in 1861, and subsequently by Kabsch (1861), and Unger (1862).

This contraction may be induced by electrical stimulus. Weak currents produce immediate contraction of the filaments, which, however, extend themselves after an interval. Strong currents, on the other hand, kill them at once; and in consequence of this, the contracted filaments never extend themselves again, but become further short-

ened, so that after about an hour they are shortened by about one-half.

Other means of death—as soaking in alcohol, glycerine, or even water—produce a like result; showing that shrinking by evaporation cannot be the cause of the shortening. By death the filaments are contracted to their minimum length. The ultimate projection of the style to about 5 millimeters beyond the anther-tube depends mainly upon the shortening of the filaments in dying. By this shortening, the anther-tube is at last about $\frac{1}{2}$ –1 millimeter below the corolla-points; while a few hours before it extended 3–4 millimeters beyond them.

The filament consists of a central bundle of annular and closely-wound spiral vessels, surrounded by longitudinal rows of long cylindrical cells with straight partition walls. Externally it is covered by an epidermis of similarly shaped cells, but which are more thickened and convex on their free surface, so that the filament appears uniformly channelled. From the surface project peculiar conical hairs, each consisting of two flat cells applied to one another; these, as well as the epidermis cells, are covered by a pretty thick *cuticula*. When the contractile filament is elongated, the inner cells, seen in longitudinal section or otherwise, appear *longitudinally striated*, “as if they were provided with longitudinal fibres.”

The contracted state of the cells is best seen when the anther-tube has come to be below the level of the corolla-points. By this time the cells of the filaments are all dead, as is proved by the shrunken primordial utricles. The cells are now all closely cross-striped—to appearance, as if the filament consisted of a number of spiral vessels. In those places where especially shorter cells occur, there is the closest transverse striation—almost like that of striped muscular fibre. This striation is caused by a very regular and close transverse corrugation of the cells, in the contraction of the filament; hence the side-walls of the cells appear finely and closely wrinkled, so that 10–20 cross folds occur in every $\frac{1}{10}$ of a millimeter. This corrugation affects all the cells,—including those of the epidermis,—except some in the centre near the air canals, whose cells often remain

unwrinkled. The process of contraction may be followed directly under the microscope, since water, or glycerine, kills the cells sooner or later. The edges of the cells are seen to become wavy. After a little, the cross-stripping is everywhere evident. Almost instantaneously the extreme contraction of the filament occurs, and at the same time the closest corrugation of its cells.

Although unable to follow the action of a *temporary* stimulus under the microscope (since the filaments soaked in water to render them transparent do not again extend themselves), the author expresses his conviction that the *momentary* shortening of the filament by stimulus, equally with the *permanent* contraction in death, depends upon a transverse corrugation of the cells. The filaments in shortening become thicker; the following are measurements of breadth *before* and *after* stimulation:—

Before,	$1\frac{8}{10} \frac{4}{10} \frac{6}{10}'''$	After,	$1\frac{1}{10} \frac{1}{10} \frac{6}{10}'''$
"	$1\frac{9}{10} \frac{9}{10} \frac{6}{10}'''$	"	$1\frac{1}{10} \frac{1}{10} \frac{6}{10}'''$
"	$1\frac{1}{10} \frac{1}{10} \frac{6}{10}'''$	"	$1\frac{1}{10} \frac{1}{10} \frac{6}{10}'''$

From the circumstance that contraction ensues upon death equally as upon a temporary stimulus, the author is induced to believe that the cells of the elongated filament are in a state of active expansion, while the contracted condition depends upon a relaxation, in consequence of which the *elasticity* to which the expansive force was opposed now comes into play. This elasticity appears to reside chiefly in the very thick cuticula, which does not, even in the most contracted filament, exhibit any corrugation, thus showing itself to be highly elastic.

The author believes that at least in the lowest animals (which possess, not muscles, but a contractile parenchyma) the same conditions hold as in contractile vegetable cells. In these animals occurs the same momentary contraction by stimulus, and the permanent and utmost contraction in death, apparently in consequence of the elasticity of their cuticula; whilst their elongation and extension is a vital and active process. For example, the stalks of the Vorticellas become rolled up equally upon stimulation as upon

death. Further, in *Amæba*, *Actinophrys*, *Diffugia*, *Arcella*, and the *Rhipopoda* generally, the processes are manifestly actively elongated, while these organisms, by stimulus as by death, are contracted into globules. Similar phenomena are exhibited by *Hydra viridis*, &c. In all these organisms there is a permanent contraction in death, which yields only to diffuence of the body by decomposition. There is no final relaxation, as in true muscular tissue.

The author gives some figures representing the microscopic appearances of the cells of the filament in the expanded and contracted conditions, but which it is unnecessary to reproduce here.*

On Muscular Power. By H. F. BAXTER, Esq.

§ 1. *On the Mechanical Power of Muscles exerted during Muscular Contraction.* § 2. *On the Application of the Principle of Conservation of Force or Conservation of Energy to the explanation of Physiological Phenomena.*

That muscular contraction is accompanied with the development of *electrical action*, I have already shown on a former occasion,† thus confirming the results previously obtained by Matteucci and Du Bois Reymond. That *heat* is evolved during muscular contraction, has been shown by Breschet and Becquerel,‡ and by Matteucci.§ That changes of a *chemical* nature take place in muscles, in consequence of muscular action, has been established by the experiments of Liebig, Helmholtz, and others; and Matteucci|| has ascertained that, during muscular contraction, carbonic

* There appears to be some mistake in the representation of the section in fig. d (*l. c.* p. 369): it purports to be a transverse section of the filament, and the more or less hexagonal outline of the cells would bear out the assertion; yet I do not understand how *transverse* corrugation could appear in a transverse section at all in the way represented in the figure, almost like the edge of a close frill.—A. D.

† Philosophical Magazine, 1855. Essay on Organic Polarity, 1860.

‡ Traité de l'Electricité, tom. iv. p. 9.

§ Annales de Chimie et de Physique. Juin, 1856.

|| Ibid.

acid is evolved. Here, then, we have the fact established, that during muscular action the three forces, *heat*, *electricity*, and *chemical* action are manifested; but the most important function connected with muscular action, and which remains to be considered, is undoubtedly its mechanical power; and just as the mechanical philosopher endeavours to solve the problems respecting the mechanical power of his machinery, so may the physiologist attempt the solution of similar questions respecting the mechanical power of the animal body; and the question may be thus put,—*What is the mechanical equivalent of muscular power exerted during muscular contraction?*

§ 1. *On the Mechanical Power of Muscles exerted during Muscular Contraction.*

Matteucci,* in a series of experiments undertaken for the purpose of solving this question, says, “Avec un poids de 100 grammes attaché à un gastrocnémien, qui pèse à peu près 0·320^{gr} et qu’on fait contracter, le poids est soulevé à une hauteur de 0·84^{mm}; avec un poids de 70 grammes, cette hauteur est de 1·130^{mm}; avec 40 grammes, de 1·270^{mm}; et avec 10 grammes, la hauteur est de 1·412^{mm}.” He adds, “M. Helmholtz, qui employait un poids de 180 grammes, a obtenu une élévation qui a varié dans une série d’expériences successives, de 0·88^{mm} à 0·65^{mm}.”

“J’adopterai donc,” continues Matteucci, “pour déterminer le travail mécanique d’une contraction d’un gastrocnémien qui soulève un poids de 10 grammes, et son propre poids qui est 0·320^{gr}, la hauteur de 1·412^{mm}. ce qui donne pour le travail mécanique d’une contraction 0·00001457 kilogrammètres.” The fact that Helmholtz obtained similar results, although he employed a much heavier weight, did not escape Matteucci’s notice, and he says, “Malheureusement pour le but de mon recherche, M. Helmholtz a fait l’expérience avec un poids de 180 grammes attaché au gastrocnémien, et qui était soulevé par la contraction; or il est très-facile de prouver, et c’est, je crois, M. Schwann qui l’a fait le premier que le travail mécanique d’une contrac-

* Annales de Chimie et de Physique. Juin, 1856.

tion augmente dans certaines limites avec le poids attaché au muscle, et qui est ainsi soulevé ce qui nécessairement fatigue et détruit beaucoup plus rapidement la force du muscle."

Professor Haughton of Dublin has recently investigated the question in the human subject, and has arrived at the conclusion, "that 1 lb. weight of a muscle is capable of lifting 1.56 ton through one foot before it became exhausted."*

Astounding as these results may appear, it is not for physiologists to ignore them. The circumstance that a great difference exists between one human individual and another in regard to their muscular powers, makes it difficult to suppose that the same results would be obtained in every case. Professor Haughton himself has shown that age has great influence respecting the time in which a muscle becomes exhausted. The question however occurs, are we enabled to ascertain the muscular power of an animal by its total weight, or are we obliged to ascertain by experiment the power of each individual separately.

In the following experiments, instead of employing the gastrocnemius muscle of the frog separated from the animal, as was done by Matteucci and Helmholtz, I adopted the plan pursued by Schwann in his investigations on muscular contraction, viz., the limb remained connected with the animal, so that the vital conditions of the muscle should be maintained during the experiment; wishing also to have the animal as perfect as possible, in my first experiments the sciatic nerve was merely exposed, but not divided, and the muscle made to contract by stimulating it; but from the frequent and general struggles of the animal, together with the contraction excited in other muscles by reflex action, this mode was soon abandoned, and the sciatic nerve divided. With the sciatic nerve divided, struggles on the part of the animal will occasionally occur; and although the gastrocnemius muscle itself is not contracted, still the animal can elevate the limb, and raise the weight, but this effect can be readily discriminated from that due to the

* Outlines of a New Theory of Muscular Action. Dublin, 1863.

contraction of the gastrocnemius alone. The importance of confining the contraction solely to this muscle, as observed by Matteucci, the perineal nerve was divided, so as to avoid the contraction of the extensor muscles; this could be readily done, as the sciatic nerve is seen to divide into two large branches at the lower part of the thigh. Care, however, must be taken that the posterior branch be not divided; to ascertain this point, the nerve was stimulated after the division, to see which muscles were contracted. Instead of keeping the frog in the horizontal position, according to the method pursued by Schwann, I found the following to be the most convenient and satisfactory mode of conducting the experiments:—

The apparatus made use of was similar in principle to that employed and described by Matteucci in the *Phil. Trans.* for 1846, and to which I must refer. It consists of a glass stem fixed upon a wooden stand; to this stem could be adjusted and fastened, by means of screws, several moveable pieces; to the lower one the pulley was attached, and the scale upon the dial was divided into 80° . The circumference of the pulley was $1\frac{1}{2}$ inch, consequently 1° of the scale corresponded to rather more than $\frac{1}{3}$ of an inch. The next piece above the pulley consisted of the portion fork-shaped, and through the holes at its extremities the silk passed, to prevent it from slipping off the pulley: to the upper end of the silk was tied a small hook; to its lowest end the scale was attached which carried the weight. The two upper detached pieces were for the purpose of securing the frog; on the lower one, which projected horizontally forwards, the frog was placed astride, one limb being attached to the pulley, the other limb was tied at the ankle, and fastened, by means of a string, to one of the arms of the lower piece which supported the dial; by these means the two limbs were balanced when the weights were placed in the scale. To the upper detached piece, which projected horizontally outwards at right angles to the lower one, the fore-extremities of the animal were fastened by strings. By this arrangement the frog was firmly fixed without interfering with its breathing, and there was no impediment to the free circulation of the blood in the lower limbs.

In my preliminary experiments I first took the weight of the frog, but soon found that during its preparation it frequently ejected a quantity of fluid, amounting to 10 or 15 grains, or more ; in subsequent experiments it was prepared in the following manner:—The tendo-Achillis was exposed by removing a portion of skin ; a piece of strong silk was then passed beneath it by means of a needle, and tied on to the tendon, so as to form a loop to which the hook connected with the pulley could be attached. A portion of skin of the thigh, over the course of the sciatic nerve, was removed, and the ligatures tied on to the fore-extremities, and to the hind limb. The animal was then secured in a screw of paper and weighed ; the weight of the strings and paper were subsequently deducted from the total weight. The sciatic nerve was then exposed and divided, and the frog fastened on to the apparatus.

To excite the nerve, and in order to have a constant power, not possessing a rheostat, one of Smee's middling sized cells, with amalgamated zinc plates, was used. The acid employed consisted of dilute nitro-sulphuric acid (1 acid 40 water). A thick copper wire was connected with the platinode, the free extremity of which passed through a piece of cork and was bent at right angles to the extent of half an inch, so as to press down upon its surface ; between the cork and the wire was a piece of coarse bibulous paper, and which was folded over the extremity of the wire ; the paper was kept moistened, and upon it the nerve rested. The wire connected with the zincode was not so thick, and its central portion wound round in the form of a helix, by the elasticity thus given to it its extremity could be readily applied to the tendo-Achillis without moving the cell, and facilitated the making and breaking of contact. The current was always used as the *direct* form, *i.e.*, it traversed the nerve and muscle to the tendo-Achillis.

The *precautions* to be observed are numerous.

1. *Avoid the loss of Blood.* Having carefully laid bare the sciatic nerve in the upper part of the thigh with a blunt pointed probe, it was withdrawn and divided ; but instead of exposing the nerve throughout its whole course by separating the muscles, it was again carefully dissected out at

the lower part of the thigh and drawn down, by these means the risk of wounding or lacerating the vessels crossing the nerve in the middle of the thigh was avoided.

In tying the tendo-Achillis to form the loop, unless care be taken, the vessel running along its inner border will be wounded when passing the needle.

2. *Not to tie the Muscular Fibres.* If, in tying the tendo-Achillis, the loop be carried too high up, the muscular fibres will be included; in this case they soon become lacerated and torn through, when the weights are added to the scale.

3. *To keep the Sciatic Nerve away from the surface of the Thigh.* It frequently happened that during the struggles of the animal the sciatic nerve would come into contact with the surface of the thigh, although its extremity was resting on the bibulous paper. Under these circumstances contractions may not be excited in the muscles when the current is passed, and this I believe to be due to the current instead of being transmitted along the nerve, *through* the muscle, was thus transmitted by the surface of the muscle or the skin of the thigh. The nerve should be kept straight away from the thigh.

4. *The Electrodes should be clean.* This is a most important circumstance to be attended to, especially with the electrode employed to touch the surface of the tendon. When the electrode is brought into contact with the surface of the tendon it becomes covered with the fluid, and a crust is soon formed upon it which acts as a non-conductor; to remedy this, it is necessary to frequently scrape it, so as to present a fresh and clean surface; mere wiping is not sufficient. The surface of the wire in contact with the bibulous paper should be carefully looked to and frequently scraped, and the paper upon which the nerve rests be kept moist, and the nerve itself occasionally slightly moistened.

5. When exciting the muscles to contract, the tendo-Achillis should be touched *lightly* and *quickly*. If the electrode be allowed to remain too long in contact with the tendon, we have two contractions, one on *making*, the other on *breaking*, contact. If the tendon be struck too forcibly the muscle will be thrown out of its perpendicular position, and thus elevate the weight and move the index.



There are other precautions necessary to be observed, which will be better understood after detailing some of the experiments.

It would have been far more satisfactory to have detailed the observations made upon each individual frog; but as this would occupy too much space, including sixty experiments, I have thought it better to tabulate the results and give the particulars of one experiment, and describe the mode of proceeding in each.

The weight (Troy weight) was first ascertained, and the frog then arranged for the experiments. The index having been pushed back as far as it could be, the degree at which it stood was noted; a weight was then put into the scale, and the index then carried back and the degree noted—this gave the elongation produced by the weight. The muscle was then made to contract, the degree noted, and the index pushed back; the muscle was again made to contract, the degree noted, and the index pushed back; this was done three times consecutively: the weight removed, and the degree to which the index returned noted. Half a minute was allowed to elapse; the electrode was cleaned and scraped, and another weight was then added to the scale, when similar observations were made. I could make the three observations, and record them, in twelve seconds; but with an assistant to record them it took about five seconds. To render the description clearer, I will give the details of one experiment.

July 14. Weight of frog 3 drs.
54 grs., caught yesterday afternoon.

	Index 52.	
4 oz.	39	40
	39	40
	38½	39½

	Index 45.	
6 oz.	35	35½
	34½	35
	34	34½

	Index 43.	
4 oz.	36	37
	36	37
	36	37

Struggles of the animal carried the scale without the weight to 85°.

	Index 44.	
5 oz.	34	34½
	34	34½
	33½	34½

	Index 41½.	
4 oz.	35	36
	35	36
	35	36

	Index 41.	
3 oz.	35	36½
	35	36½
	35	36½

Index 45.		
5½ oz.	33	33½
	32	32½
	32	32½
Index 40.		
4½ oz.	32	32½
	32	32½
	32	32½
Index 38.		
3½ oz.	32	32½
	32	32½
	32	32½
Struggles carried scale without weight to 92°.		
Index 41.		
3 oz.	33	34
	33	34
	33	34
Index 39.		
6 oz.	30	the index just moves
Index 37.		
5 oz.	30	30½
	30	30½
	29½	29½
Index 35.		
4 oz.	29	29½
	29	29½
	29	29½
Struggles carried scale to 92°.		
Index 38.		
3 oz.	31	32
	31	32
	31½	32½
Index 35.		
5 oz.	29	29½
	29	29½
	28	28½
Struggles carried scale to 80°.		
Index 38.		
4 oz.	29	30
	29	30
	29	30

Struggles carried scale to 90°.

Index 38.		
3 oz.	30	31½
	30	31½
	30½	31½

Index 35.		
2 oz.	31	33
	31	33
	32	33

Struggles carried scale to 80°.

Index 38.		
4½ oz.	29	29½
	29	29½
	28½	29½

Index 35.		
3½ oz.	29	30
	29	30
	29	30

Index 36.

The experiment was discontinued for a quarter of an hour, during which time struggles had taken place carrying the index as far as it could. The index was now brought back to 40°.

4 oz.	29	30
	28½	29½
	28½	29½

Index 36.		
3 oz.	29	30½
	29	30½
	29½	30½

Index 37.		
5 oz.	28½	29
	28	28½
	28	28½

The animal was now removed, and its weight found to be 3 drs. 47 grs.

The gastrocnemius of the leg submitted to experiment was somewhat darker and heavier than the other; the slight difference just turned the scale of the balance. The weight of each 3 grs.—Sex, M.

After each experiment the plates were removed from the cell of the battery and rinsed in clean water.

Judging from the results obtained in this experiment, I consider that the normal power of the gastrocnemius muscle of this frog was capable of raising 4 oz. (to which must be added 2 drs. for the weight of the scale) 1° , or $\frac{1}{8}$ part of an inch.

As the mode in which the weights are added have great influence over the results, it is necessary to be acquainted with it. If the weight be rather heavy in the first instance, the elongation of the muscle takes place gradually and continuously, and unless the index be carried sufficiently back before the muscle is made to contract, the full amount of contraction will not be obtained; and if the weight be too heavy for the muscle, it acts by exhausting the muscular power or by overstretching the muscle. If the weight be not sufficiently heavy to elongate the muscle, the amount of contraction is not greater than with a heavier weight, proving the correctness of the conclusion arrived at by Schwann, that the contractile power of a muscle is greater the more it is elongated. There is a limit to the natural elongation of a muscle, and this limit it is somewhat difficult to ascertain. It is not indicated by gradually adding the weights to the scale until contraction is just excited, but more by the permanency and constancy of the results when the experiment is repeated. The criterion which I think may be acted upon with safety is this—commence with a medium weight, and note the results obtained by increasing the weight; if the muscle continues to elongate, and if the index has a tendency to recede, we are getting beyond the limit of its natural elongation. If by diminishing the weight the index has a tendency to advance or to retain its position, we have now obtained some indication of its natural power. In the experiment detailed I began with a medium weight,—4 oz.,—and the index was brought to 39; 6 oz. were afterwards added, this brought it to 35, and during the contraction it fell to 34; 4 oz. were then added, it arose from 35 to 36, and retained that number. If we cast our eye over the results, we find that the heavy weight evidently exhausted the power of the muscle, and

that during the time the muscle contracted there was a tendency on its part to become further elongated. When a lighter weight was substituted the tendency was to contract, and the degree of contraction of the muscle when excited was not increased in proportion. I may just observe, that the motion of the index is not always as if by a sudden action, but by a gradual advance, being carried along, as it were, by some progressive movement.

There is another circumstance to be noticed, the weights raised do not indicate a fractional part less than the half ounce. I will not go so far as to say that a less weight than this might not be measured, but I have found that by adding the half ounce the effects were very doubtful. The best plan to elucidate the result was not by adding or subtracting the half ounce from the previous weight, but to go through a series of weights increasing or diminishing by the ounce; for instance, begin with 3, then 4, and then 5; in the next series begin with $3\frac{1}{2}$, then $4\frac{1}{2}$, and then $5\frac{1}{2}$. By this means the effect of the half ounce could be more readily seen than by adding it to the ounce. The addition of a drachm was not perceptible.

The utmost care was taken to obtain the exact weight of the gastrocnemius muscle immediately after each experiment.

The frogs were fresh caught, and if not used on the same day, were kept in a dark, cool cellar during the night, and used on the following day.

In Table I. will be found the results of 60 experiments, arranged according to the date when they were performed. The sum total shows that 4 drs. 22 grs. weight of muscle were capable of raising a weight equivalent to 332 oz.; in other words, 1 gr. of muscle could raise, in round numbers, 608 grs. through the space of 1° , which was equivalent to $\frac{1}{4}$ of an inch; the extremes were 1048 (Expt. 51) and 520 (Expt. 24). The weight of the frogs before the experiments was 34 oz. 4 drs. 18 grs.; after the experiments 33 oz. 3 drs. 26 grs., indicating a loss of 1 oz. 52 grs. Out of the 60 frogs 26 were females; and in order to ascertain the sex no external character was trusted to, but the body was examined after the experiment.

TABLE I.

No. of Experiment.	Date when performed.	Weight of Frog before Experiment.		Weight raised.	Weight of Frog after Experiment.		Weight of Muscle.	Sex.	
		dra.	gra.	os. dra.	dra.	gra.	gra.		
1	June 26	4	26	4	2	4	18	4	M. {The muscle which raised the weight heavier by 1/4th of a grain, and more vascular.
2	...	8	7	9	2	8	0	8	F. Heavier by 1/4 a grain, and more vascular.
3	...	3	45	4	2	3	28	4 1/2	F. Muscle more vascular, but of the same weight.
4	...	3	20	8	6	3	14	2 1/2	M. Slight difference in vascularity; weight the same.
5	... 29	5	35	7	2	5	28	5 1/2	M. Slight difference in vascularity; weight the same.
6	...	2	55	3	2	2	47	2 1/2	M. {Muscle presents spots of vascularity; weight the same.
7	...	5	5	5	2	4	55	4 1/2	F. Heavier by 1/4th of a grain, and more vascular.
8	...	5	55	6	6	5	47	5	M. Slight difference in vascularity; weight the same.
9	July 1	2	0	2	6	1	55	1 1/2	M. No difference in colour or weight.
10	...	2	8	3	2	2	3	1 1/2	F. No difference in colour or weight.
11	...	2	7	2	6	2	0	1 1/2	M. No difference in colour or weight.
12	...	2	38	3	2	2	33	2	M. Slight difference in colour; same weight.
13	...	4	50	4	2	4	42	3	M. No difference in colour or weight.
14	... 2	5	55	7	6	5	46	6	F. {No difference in weight or colour. This appears to be an old frog; the thigh bone of the limb not experimented upon was fractured during the struggles of the animal; muscles red.
15	...	1	50	2	2	1	44	1 1/2	M. No difference in weight or colour.
16	...	2	48	3	2	2	41	2	M. {Muscle apparently ecchymosed; no difference in weight.
17	...	2	56	3	6	2	48	2 1/2	M. {Muscles of both limbs apparently ecchymosed; no difference in weight.
18	...	5	12	7	2	5	1	5 1/2	F. Heavier by 1/4th of a grain, and more vascular.
19	...	8	0	9	2	7	46	7 1/2	F. No difference in colour or weight.
20	...	8	57	9	6	8	45	9	F. No difference in weight; slightly darker.
21	...	9	0	10	2	8	48	8 1/2	F. Heavier by 1/4 a grain; no difference in colour.
22	...	4	45	5	6	4	36	4	M. Slight difference in colour; heavier by 1/4 a grain.
23	... 4	6	30	6	2	5	21	5 1/2	F. No difference in colour; slightly heavier.
24	...	8	35	10	2	8	21	9 1/2	F. Heavier by 1/4 a grain, and more vascular.
25	...	6	53	8	6	6	41	7	M. {No difference in colour; just turned the scale of the balance.
26	...	9	10	9	2	7	55	8 1/2	F. {Slight difference in colour and weight. Fluid ejected during the experiment.
27	...	2	53	4	2	2	48	2 1/2	M. No difference.
28	... 7	5	51	6	6	5	44	5	F. No difference.
29	...	5	49	6	2	5	40	5 1/2	M. {Slightly darker and heavier. Tetanic contractions when weight was removed, but subsided upon moistening the nerve.
30	...	4	10	4	2	4	1	3 1/2	M. No difference.
31	...	2	13	3	2	2	8	2	M. Somewhat darker, but of same weight.
32	...	2	10	3	2	2	6	2	F. No difference.
33	... 8	2	20	3	6	2	12	2	M. No difference.
34	...	3	0	3	6	2	53	3 1/2	F. No difference.
35	...	2	15	2	6	2	8	2	F. No difference.
36	...	3	8	4	2	2	58	3 1/2	M. Slightly darker and heavier.
37	...	2	56	4	2	2	47	2 1/2	F. No difference.
38	... 10	2	40	3	6	2	34	2	F. Slightly heavier.
39	...	3	0	4	2	2	55	3	M. No difference.
40	...	2	48	4	2	2	43	3	M. No difference.
41	...	8	25	9	6	8	17	8	F. No difference.
42	... 11	5	36	8	6	5	25	7	M. No difference.
43	...	4	0	5	2	3	53	3 1/2	F. Darker, but weight the same.
44	...	4	28	6	2	4	17	4 1/2	M. Darker, and slightly heavier.
45	...	2	22	3	2	2	15	2	M. No difference.
46	... 14	7	5	8	2	6	58	8	F. No difference.
47	...	3	50	4	2	3	45	3 1/2	M. No difference.
48	...	3	54	4	2	3	47	3	M. Slightly darker.
49	...	3	52	4	2	3	43	3 1/2	M. Darker; no difference in weight.
50	...	2	28	3	6	2	23	2	M. Slightly darker.
51	... 15	2	5	3	6	2	0	1 1/2	M. No difference.
52	...	6	20	5	2	6	15	5	F. No difference.
53	...	4	38	5	2	4	33	5	M. Darker coloured, and slightly heavier.
54	...	7	20	10	2	7	12	9	F. {Slightly heavier. Tetanic contractions when the weight was removed, which did not subside upon moistening the nerve.
55	...	2	32	3	2	2	26	2	F. Darker coloured; same weight.
56	... 16	5	58	6	6	5	50	6	M. Slight difference in colour; same weight.
57	...	4	48	5	2	4	42	4 1/2	M. Darker, and heavier by 1/4th of a grain.
58	...	6	52	8	6	6	40	7 1/2	M. Darker, but of same weight.
59	...	4	13	3	2	4	8	3 1/2	F. No difference.
60	...	7	58	9	2	7	45	8	F. Slight difference in colour; same weight.
Total,		os. dra. gra.		os.	os. dra. gra.		dra. gra.		
		34 4 18		332	33 3 26		4 22		

TABLE II.

MALES.						FEMALES.							
Weight of Muscle.	Weight raised.	Weight of Frog before Experiment.		Weight of Frog after Experiment.		No. of Experiment.	Weight of Muscle.	Weight raised.	Weight of Frog before Experiment.		Weight of Frog after Experiment.		No. of Experiment.
gra.	oz. dra.	dra.	gra.	dra.	gra.		gra.	oz. dra.	dra.	gra.	dra.	gra.	
1½	2 2	1	50	1	44	15	1½	3 2	2	8	2	3	10
1½	2 6	2	0	1	55	9	2	2 6	2	15	2	8	35
1½	2 6	2	7	2	0	11	2	3 2	2	10	2	6	82
1½	3 6	2	5	2	0	51	2	3 2	2	32	2	26	55
2	3 2	2	13	2	8	31	2	3 6	2	40	2	34	38
2	3 2	2	22	2	15	46	2½	4 2	2	56	2	47	37
2	3 2	2	48	2	41	16	3½	3 2	4	13	4	8	59
2	3 6	2	20	2	12	33	3½	3 6	3	0	2	55	34
2	3 6	2	28	2	23	50	3½	5 2	4	0	3	53	43
2½	4 2	2	53	2	48	27	4½	4 2	3	45	3	38	3
2½	3 6	3	20	3	14	4	4½	5 2	5	5	4	55	7
3	4 2	2	48	2	43	40	5	5 2	6	20	6	15	52
3	4 2	3	0	2	55	39	5	6 6	5	51	5	44	28
3	4 2	3	54	3	47	48	5½	6 2	6	30	5	21	33
3	4 2	4	50	4	42	13	5½	7 2	6	12	5	1	18
3½	4 2	3	50	3	45	47	6	7 6	5	55	5	46	14
3½	4 2	4	10	4	1	30	7½	9 2	8	0	7	46	19
4	4 2	4	25	4	18	1	8	8 2	7	5	6	58	46
4	5 6	4	45	4	36	22	8	9 2	7	7	8	0	2
4½	5 2	4	48	4	42	57	8	9 2	7	58	7	45	60
5	5 2	4	38	4	33	53	8	9 6	8	25	8	17	41
5	6 6	5	55	5	47	8	8½	9 2	9	10	7	55	26
5½	7 2	5	35	5	28	5	8½	10 2	9	0	8	48	21
7	8 6	5	36	5	25	42	9	9 6	8	57	8	45	20
7	8 6	6	53	6	41	25	9	10 2	7	20	7	12	54
7½	8 6	6	52	6	40	58	9½	10 2	8	35	8	21	24
dr. gra	oz. dra.	oz. dra.	gra.	oz. dra.	gra.		dr. gra	oz. dra.	oz. dra.	gra.	oz. dra.	gra.	
1 30	123	12 2	25	11 7	28		2 22½	171	18 2	9	17 5	27	

In Table II. will be seen the influence of *sex* upon the results, and in order to reduce the number of males to that of the females, and to avoid anything like a selection, every fourth male, as recorded in Table I., was omitted. This Table has also been arranged according to the weights of the gastrocnemius muscles; and it will be seen that the total weight of the female frogs before the experiment was 18 oz. 2 dr. 9 grs., whilst that of the male frogs was only 12 oz. 2 dr. 25 grs. The total weight raised by the female frogs was 171 oz., by the male frogs 123 oz.; but the weight of the gastrocnemius muscles in the females was 2 dr. 22½ grs., that of the males 1 dr. 30 grs., indicating that the muscular power of the males, as compared to that of the females, showed that 1 gr. of muscle in the males could raise 656 grs., whilst in the females 1 gr. raised only 579.

From these experiments we find that the weight of the
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animal is no true indication of its muscular power. They also establish this important fact, that during muscular exertion there is a tendency on the part of the muscle to *increase* in weight and consequently in power. I shall allude to this circumstance again, and will now merely draw the attention of those who consider the animal body as bearing a strong resemblance to a steam-engine, that we have here, in the animal machine, the paradoxical fact of a machine increasing its power whilst doing work. The increased vascularity may be due, in part, to the division of the vaso-motor nerves.

There can be no doubt that *age* would have an important influence over the results; at present I know of no character by which the age of the animal may be ascertained, and consequently unable to obtain any evidence to decide this point satisfactorily.

To show the importance of the healthy condition of the animal, I will draw up in a tabular form the results of a few experiments in which the frogs had been kept without food for three weeks, and in a comparatively warm situation.

TABLE III.

No. of Expt.	Date.	Weight of Frog be- fore Expt.		Weight raised.		Weight of Frog after Expt.		Weight of Muscle.	Sex.
		dra.	gra.	oz.	dra.	dra.	gra.		
1	July 16	3	18	1	0	3	10	2½	M.
2	...	2	10	0	6	2	1	2	F.
3	...	4	15	1	2	4	8	3½	M.
4	...	7	5	1	6	6	54	8	F.
5	...	6	9	1	2	5	59	7½	M.

Table III. will not require any analysis, as the facts will speak for themselves. The frogs were flabby, and the muscles pale and soft.

In the following experiments the limb was separated from the body to ascertain the influence of the circulation of the blood. The spine was divided in the lumbar region, and the whole of the viscera, together with the upper part of the animal, removed, leaving the pelvis and the two lower

extremities, which were fastened to the apparatus as in previous experiments. The results were as follow:— During the first five minutes the muscle could raise the medium weight, provided the contractions were not excited too often. If a heavy weight were put into the scale, the muscle soon became exhausted, and required time to recover itself. If the muscle could elevate the medium weight of 3 oz. 1° at the commencement of the experiment, and if it were made to contract once in a minute, at the end of half an hour this was reduced to 1 ounce. The weight was not kept in the scale the whole time, but removed after every contraction. The contractility of the muscle continued some hours, but the power of the muscle became gradually weaker. In another experiment, the continual suspension of a heavy weight for a quarter of an hour reduced the power of the muscle that it only raised 2 drs. When the weight was removed, tetanic contractions were observed, but upon moistening the nerve they subsided. Tetanic contractions were observed in two of the experiments recorded in Table I., in one (Exp. 29) the effect subsided upon moistening the nerve, in the other (Exp. 54) this had not the desired effect, and they must therefore have been dependent upon other circumstances than the state of the nerve. The importance of maintaining the circulation of the blood in these experiments is very evident. As there was a tendency in the former experiments to an increase in the nutrition of the muscle, I was led to examine whether any increase in its electric condition occurred under these circumstances.

A frog fresh caught, and weighing 4 drs. 25 grs., was taken, and 2 oz. put into the scale; the muscle was made to contract every 10 seconds for a quarter of an hour. The two gastrocnemii were removed as quickly as possible, and examined; the muscle experimented upon indicated 3°, that of the other limb 2°. They were then arranged into a circuit, to ascertain the amount of the differential current; the effect upon the galvanometer amounted to $\frac{1}{2}$ ° in favour of the former muscle. The elements were then arranged to form a combined current, when the effect upon the needle amounted to 4°. The muscle was more vascular than the other. The experiment was repeated upon another

frog, which weighed 7 drs. 10 grs. With a weight of 4 oz., after the lapse of five minutes, the muscles were examined. The separate muscles indicated the one acted upon 4° , the other 2° . When united to show the differential current, the effect was $1\frac{1}{2}^{\circ}$ in favour of the muscle experimented with; when the elements were arranged to show the combined current, the effect upon the needle was 5° .

In other experiments, four in number, the results always indicated an increase on the part of the muscle experimented with.

Not having the necessary apparatus, I could not investigate the question respecting the *heat* that is developed during muscular contraction.

The following conclusions may be deduced from the foregoing inquiry:—

1st, Out of 60 frogs taken indiscriminately, the average results of the experiments show that 1 grain of muscle is capable of raising a weight of 608 grs. 1° , or through a space of $\frac{1}{8}$ of an inch.

2d, That sex has an important influence over the results. In males it was found that 1 gr. = 656 grs., whilst in females, 1 gr. = 579 grs.

3d, That the weight of the animal previous to the experiment gave no indication as to the real muscular power of the animal that could be relied on.

4th, That during the experiment there was a loss of weight in the animal but an increase of weight in the muscle experimented upon.

5th, That an increased nutrition of the muscle was indicated by its weight, its vascularity, increase in its electrical condition, and by the maintenance of its muscular power.

6th, That circumstances influencing the health of the animal, such as absence of food, temperature, and confinement, have an important influence over the results.

7th, That the maintenance of the circulation of the blood is of the utmost importance in these experiments.

§ 2. *On the Application of the Principle of "Conservation of Force or Energy" to the Explanation of Physiological Phenomena.*

In my concluding remarks, I will make a few observa-

tions regarding the application of the principle of "Conservation Force or Energy" to organic phenomena.

When we find it stated that the principal products of the light of the sun are plants, because plants can only grow with the help of the sun-light; that the heat of the sun is the cause of vegetation upon the earth; or that the mechanical power of a man may be ascertained by calculating the amount of carbonic acid evolved during respiration, in the same manner as we might ascertain the power of a steam-engine by calculating the amount of heat evolved during the combustion of the fuel supplied to generate the steam; as physiologists we cannot help feeling that some doubt must exist in regard to the data upon which such conclusions are based. For my own part, I remember, when pursuing some investigations a few years ago, respecting the development of plants, that, upon placing some plants (beans) in a dark cupboard to vegetate, they grew more rapidly in the dark than when exposed to the light of the sun; they were blanched, and therefore may not, perhaps, be considered as in their natural state, but nevertheless they grew without the influence of the sun-light. It is, I believe, considered to be a well-known fact amongst vegetable physiologists, that sun-light, under certain circumstances, retards rather than promotes vegetation. Although I make these observations, I do not for one moment deny the influence of sun-light or of heat over vegetation, or the importance of estimating and ascertaining the relation that the evolution of carbonic acid in the animal body, during respiration, bears to the muscular power of the individual. It is in the application of these facts where the doubt exists, and not in the correctness of the principle upon which they are based.

Professor Helmholtz,* in a lecture delivered at the Royal Institution, has clearly and beautifully shown in a masterly manner the importance of applying this principle to organic nature. The observations that I shall now make will be

* On the Application of the Law of Conservation of Force to Organic Nature. April 12, 1861. I must also refer to a lecture delivered by Professor Faraday at the same Institution, "On the Conservation of Force."—*Phil. Mag.*, April, 1857.

for the purpose of extending these views, and show the importance of its application in the explanation of physiological phenomena. Before doing so, let us endeavour to get a clear view of the object we have in view. In animal bodies, in man, for example, we have the following forces evolved: nerve force, muscular force, heat, and chemical force—now these forces are developed during growth, and maintained by nutrition. Nerve force and muscular force may be considered as the special properties of their respective tissues, and dependent upon the peculiar compounds (chemical) which form them. These compounds are derived from the blood, which, on its part, is derived from the food, hence we must consider the food as the original source from whence the animal body derives its ultimate power. Heat and light influence the formation of the various compounds which go towards forming and maintaining the animal structures. Heat and light may also be given out and developed during the formation of organic compounds; so that we have heat and light necessary for the *formation* of the organic compounds, and we also have the same forces generated during the *disintegration* of the compounds. It appears to me that a great deal of the misconception and confusion which occurs arises in consequence of not keeping clearly in view the *formative* actions, and the *disintegrating* or *destructive* actions which take place in the animal body. Let us now consider for a moment the animal body as a machine doing work. We have nerve work (comprising mental work and nervous action in general) in which nerve force is exhausted; we have muscular work (comprising muscular contraction or mechanical work), in which muscular force is exhausted. The various secretions and excretions which take place in the animal body must be looked upon in the light of *disintegrating* actions; they cannot be looked upon in the same light as we view the generation of nerve force or muscular force; the compounds which constitute the secretions are thrown off, and do not form a permanent constituent of the animal body like the muscular or nervous tissues, although they may be intended for ulterior purposes; the principal office of the secretions and excretions may be to keep and maintain the blood in a

proper and fit state for healthy nutrition. Excessive secretion, as during lactation, many exhaust the vital powers in the same manner as excessive nerve action or muscular action, but we must bear in mind, when applying the principle of Conservation of Force in the explanation of natural phenomena, the distinction between the *formative* action by which the force is *generated* and the *disintegrating* or destructive actions by which the force is *expended*. The analogy which is drawn between the steam-engine and the animal body is too far fetched, if we compare the food and fuel on the one hand and the lungs and the furnace on the other: the respiratory function is not an act by which the vital force is *generated*, although it is one by which it is *maintained*. We cannot say that the vital force of the animal is generated, or originates in the combustion of the carbon which takes place in the lungs, as the power of the steam-engine originates in the combustion of the fuel which generates the heat; that which is primary in one is secondary in the other. During muscular action—the development of muscular power—we have heat evolved, chemical action taking place and electricity evolved, but we are not justified in saying that the mechanical power of the muscle is converted into chemical force, or into heat, or into electricity. It would be more in unison with the facts if we say that the muscular force developed and generated during the nutrition of the muscle is converted into mechanical power, heat, electricity, and chemical force, during muscular action. But we should naturally ask, What do we mean by muscular force? In what does it differ from mechanical power? And this shows us the impropriety of assuming what are mere properties to be forces. The term *conversion* gives but an inadequate idea of what is intended to be conveyed, and consequently inappropriate. The relation which heat, electricity, and chemical action bear to the power exerted by the muscle, in other words, their relative equivalents, is the point to be ascertained.

Similar observations may be made in regard to nerve force. Nerve force is generated and maintained during nutrition, and expended during nerve action. Nerve action

is no doubt accompanied with chemical changes which take place in its tissue, as in the muscles; but whether heat and electricity are developed, as in muscles during muscular action, is extremely problematical.

I have alluded in a former part of the paper to the fact that, during muscular action the nutrition of the muscle is increased, and consequently the muscular power is maintained, if not increased; here we have apparently a *creation* of force during work instead of exhaustion. If we bear in mind that it is during nutrition that the muscular force is generated and maintained, and that *time* must be taken into account in considering these phenomena, there is, notwithstanding the apparent creation of power, ultimately an exhaustion; and consequently these facts go far to establish the correctness of the principle of the Conservation of Force; at the same time show the extreme beauty of adaptation and contrivance in the animal machine when comparing it with the admirable contrivances formed and designed by human skill.

Collection of the Popular Weather Prognostics of Scotland.

By ARTHUR MITCHELL, A.M., M.D., Member of Council of the Scottish Meteorological Society, &c.*

1. *Minutes of Council of the Scottish Meteorological Society, July 1863.*

The Council had laid before them by the Marquis of Tweeddale, a "Collection of the Popular Weather Prognostics of Scotland," drawn up by Dr Arthur Mitchell, M.D., accompanied by a letter from the author, dated 15th December, 1862, and addressed to his Lordship.

The Marquis repeated to the Council, what he had stated to the general meeting of the Society, held on 24th ultimo, that he considered it would be of use to publish these prognostics, and also very important to have them explained and tested on scientific principles. As the general meeting

* In connection with the paper the following minute and letter were transmitted.

had expressed approval of his views, he now begged to intimate his readiness to offer a prize of twenty guineas for the best scientific explanation of the prognostics collected by Dr Mitchell; and he would leave it to the Council to make all the arrangements necessary for carrying out his object.

The Council agreed to record their sense of the great liberality of the noble Marquis in this matter, and also their appreciation of the importance of the subject. They are of opinion that the best way of bringing the prize under the notice of persons likely to compete for it, would be to publish Dr Mitchell's collection of prognostics in the "Edinburgh Philosophical Journal," accompanied by a copy of this minute. They farther agree to intimate that the prize will be given in the form of a gold medal, a piece of plate, or otherwise as the successful competitor may desire.

The papers intended to be given in for this competition must be lodged with Mr Buchan, the Society's Secretary, at No. 10 St Andrew Square, Edinburgh, on the 1st May 1864.

Any persons wishing to have separate copies of Dr Mitchell's paper, may obtain them by applying to Mr Buchan.

DAVID MILNE HOME, *Convener.*

2. *Letter to the Most Noble the Marquis of Tweeddale.*

LAVEROCK BANK VILLAS, TRINITY, EDINBURGH,
15th December 1862.

To the Most Noble the Marquis of Tweeddale, President of the Scottish Meteorological Society.

MY LORD,—Along with this letter I forward to you a collection of the weather prognostics of Scotland, which I have made at your Lordship's suggestion. I have confined myself to those which are strictly popular in their character, and which do not involve the use of scientific instruments.

The object to be served by such a collection appears to be twofold:—

I. These prognostics are thus presented in a form which
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facilitates the testing of their trustworthiness by careful and methodical observation.

II. By having them collected together, we are better able to compare and generalize.

In the effort to attain this object, we may find that phenomena, which are apparently unconnected, but which foretell the same change, are in reality allied through a common cause ; we may discover how it happens that certain classes of phenomena come to predict certain approaching conditions of the weather ; and further, it may happen that while sifting well-established popular beliefs, we may fall on the proof of important weather laws.

I shall illustrate my meaning, and at the same time show how such an inquiry may become interesting and valuable, both in a scientific and practical sense, by reference to the following prognostics :—

A heavy in-shore swell during calm weather is believed to foretell wind. This prognostic has been found to be very generally correct. The swell, in fact, is merely the product of a distant storm then existing, which is travelling towards us, and which will probably, though not certainly, reach us. The log-books of ships have satisfactorily established the truth of this explanation. We are led, therefore, to conclude that storms do not always travel so fast as does their influence on the water ; nor is this the only important conclusion concerning the progress of storms which flows from the examination of this single phenomenon.

Again, when the fisherman of the Dornoch Coast, on a clear and calm night, hears the sound of rushing waters over the *Gizzen-Briggs*, he expects a storm from the east. Now, it is more than probable that this sound is merely another manifestation of the precursory swell of which I have just been speaking, and which causes it by producing broken waters on the sandbank at the mouth of the firth. We have here an illustration of how phenomena seemingly unconnected may possibly be due to the same cause. The “*ebb and flow and ebb again*,” near high or low water mark, in narrow firths, a phenomenon which fishermen call *leakies*, may also have the same explanation, viz., the action on the water of a remote storm, perceived before the arrival of the

storm itself, but whose coming is thus predicted. Even the landward flight of the sea-gull may have the same origin. Before the storm has come the swell may have disturbed his feeding-ground, and thus made his flight inland for food a presage of stormy weather.

After some of the prognostics I have inserted remarks, suggestive or explanatory, and bearing either on a special phenomenon or on a class. When clouds, for instance, on the summits of particular mountains, are thought to foretell coming rain, I have indicated, as accurately as I could, the position of the mountain in relation to the locality in which the prognostic holds. The intention is obvious, being suggestive of one aspect in which phenomena of this class may be studied.

While making this collection, I embraced every opportunity which offered of conversing with those who were reputedly good weather-prophets, and I found two points of practical importance on which all appear to agree.

1. No sign is considered *infallible*. "It is not the less a sign, however," said a cautious and intelligent observer, who added, "We all trust the barometer, yet even it sometimes fails, and the clouds, *well understood*, are only a little less certain."

2. A prediction is seldom founded on a *single sign*. Never, indeed, have I seen the system of "putting that and that together" in more extensive operation. I have been told, again and again, that only by so doing can the coming weather with any certainty be predicted.

I now beg to offer a few remarks on the different classes of prognostics, indicating those on which I think the least reliance can be placed, pointing out the desirability of minute and full descriptions, and giving several reasons why some prognostics, which at first sight may seem valueless, deserve to be fairly considered before rejection.

It appears to me that the accusation of fanciful can with most fairness be brought against those prognostics which are associated with the aurora, halos, mock-suns, thunder, &c.

I have been led to form this opinion from finding that the same phenomenon is by different persons in the same locality made to presage totally different conditions of the weather. Appearances which are marvellous, startling, or uncommon, are apt to be accepted by the ignorant as prophetic of coming change of some sort. It is thus that the aurora has been thought to predict wind by some, and war by others, while the Eskimo looks on it simply as a game, which the spirits of departed relatives are playing.

There is also a probability that not a few of those prognostics which are connected with the conduct and movements of animals, will have to be rejected as groundless. But for several reasons I think that this must be done with caution. For instance, when the screaming of the peacock is said to indicate coming rain, I find that this does not refer to a cry once or twice repeated, which may have a score of causes, but to a persistent screaming, with evidence of discomfort, and without manifest cause. There is a great difference between the simple statement that the peacock's screaming foretells rain, and the other statement, with its important qualifications. In its bare form we might properly reject the prognostic as worthless, and as properly retain it when amplified.

It becomes, therefore, exceedingly desirable that we should possess a *full and minute description* of all popular weather prognostics. Of but a few in the collection can I pretend to have given this, which I believe can only be obtained through the co-operation of many observers, whose attention has been drawn to the things which should be observed by some such collection as that which I now forward. I frequently experienced the greatest difficulty in carrying away a clear understanding of the sky and cloud signs. "You must see the thing, sir, to know it," was often the end of a fruitless effort to make sure of what was meant. The roughest sketch in such a case would be of the greatest value. Of this Mr Clouston has given an illustration, which will be found at page 222.

There is another reason—and one of general application—for caution in rejecting any old popular prognostic as utterly worthless. Though *singly* it may be of little value,

yet *associated with others*, it may give strength, and the *whole* may be safely trusted in determining a debated course of action. Thus, the continued and distressed screaming of the peacock arrests the farmer's attention, and the first thing to which this leads is to his looking around for other signs, and he acts according to what he believes to be the teaching of the whole. I have said that he *acts* on this teaching, and this leads me incidentally to remark, that it is the fact that important operations are every day *actually determined* by such indications, which makes it so desirable to have the false separated from the true, so that the shepherd, the farmer, and the sailor, may trust only to prognostics which are ratified by experience, and, when possible, grounded on reason.

The great antiquity of many of these weather proverbs is also a substantial argument against hastily dismissing them. Peculiarities in the flight and conduct of birds have been held to foretell approaching change of weather for thousands of years. We might suspect this to be a mere extension of the drawing of auguries from them. The victory at Artemesium was assured to Themistocles by the crowing of a cock. Indeed, ornithomancy was an actual science among the early Greeks. But this doubt as to the origin of such weather prognostics is removed by the manner in which Virgil handles them. In his allusions there is nothing of the supernatural or mysterious. He speaks of them as simple deductions from the experience or observation of those whose occupations led to the looking for signs of coming storms. It is true, they may now be called traditional, but as every generation has had the opportunity of testing their accuracy and value, unless they had been found to contain some measure of truth they would soon have ceased to be handed down. It must not be forgotten that they regulate affairs of every-day life, and lead to loss or gain—a test which soon deprives traditional error of its vitality; and this is only a little less slowly its fate when it is linked to or mixed up with religious belief.

So it is with that class of prognostics to which St Swithen's day belongs. In Scotland, at the present time, there is practically no faith placed in them. Except St Swithen's

day, indeed, few are known, and with regard to it careful observation has proved to us that

“Nor Paul nor Swithen rules the clouds and winds.”

I have not included in my collection the numerous prognostics of this kind drawn from festivals in the calendar, first, because they are not now believed in or even known to the people; and, secondly, because they are manifestly not worthy of belief. The very uncertainty as to whether they should be computed by the Gregorian or Julian Calendar is fatal to them. For similar reasons I have not included those prognostics connected with certain days or months, as, for instance, when the direction of the wind on New Year's Eve is made to betoken the weather of the whole incoming year. It is a mistake to suppose that such prognostics are now believed in by the people of any part of Scotland. It is doubtful indeed if the belief in them was at any time general. They probably originated in the fancy of some *prophetic rhymster*, and to a quaint rhyming they are indebted for the life which they have had.

The *wide diffusion* of many popular prognostics is the only other claim for consideration which I shall notice. Not a few of those most familiar to us in Scotland are popular beliefs not only throughout Europe, but also among many of the nations of the East and South. Many of these appear to rest on a wide-spread *faith in the steadiness of annual averages*, and in a supposed *system of compensations* which this belief almost involves.—With great respect, I am, my Lord, your most obedient servant,

ARTHUR MITCHELL.

NOTE.—(1.) Only strictly popular weather prognostics are included in this collection. None of them involve the use of scientific instruments.—(2.) Of the great majority I am able to state that they are not obsolete, being still used as practical guides in daily life.—(3.) I have not thought it right to exclude any because they seemed to me valueless, if others had faith in them.—(4.) Many of them are mentioned in the tours of Martin, Pennant, Boswell, Johnston, Anderson, Macculloch, &c.; and I have found a considerable number scattered through the Old and New Statistical Accounts. Between twenty and thirty have been furnished

to me by Archibald Gibson, Esq., and I have extracted a few from the recent returns to the Scottish Meteorological Society, while others, so far as I am aware, are nowhere recorded.—(5.) I could give more than one authority for the greater number, and numerous authorities for many, so that I have thought it best to give no references where the prognostics are popular ones.—(6.) When I fell in with any popular prognostic in the course of reading I noted it down, and made inquiries on the subject when I reached the locality in which it was said to hold. Thus I established its existence, and at the same time put myself in the way of hearing of others.—(7.) The notes must be regarded as simply suggestive, and their accuracy must not be assumed.—(8.) It is possible that errors may have been made in taking down some of the prognostics.—A. M.

I. Hills or Mountains—Local Prognostics.

(1.) In Kilconquhar and Elie, a cloud resting on Largo Law is a sign of coming rain, while one on Kellie Law presages clear weather. This is interesting, because the first hill lies to the west, and the last to the east of the district. The popular version of this prognostic is as follows :—

1. When Largo Law puts on his hat,
Let Kellie Law beware of that.
2. When Kellie Law gets on his cap,
Largo Law may laugh at that.

(2.) To the people of Sorbie, in Wigtownshire, a cloud on Cairnsmuir after dry weather indicates coming rain, and in the adjoining parish of Whithorn the same prognostic runs thus :—

“ When Cairnsmuir wears a hat,
The Machers’ rills may laugh at that.”

Cairnsmuir (1737) hill lies to the N.N.E. of the district in which this prognostic prevails.

(3.) To the people about Stranraer there is a prognostic thus expressed in rhyme :—

“ There is a high wooded hill, above Lochnaw Castle,
Take care when Lady Craighill puts on her mantle ;
The Lady looks high and knows what is coming,
Delay not one moment to get under covering.”

This hill lies to the north-west of the district in question.

(4.) In Dumfries, Kirkpatrick-Fleming, and several of the in-

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tervening parishes, the rolling of clouds landward, and their gathering about the summit of Criffel, is regarded as a sure indication of approaching foul weather.

Criffel lies to the south-west of these places.

(5.) Heavy clouds on Skiddaw, especially with a south wind, the farmer of Kirkpatrick-Fleming looks on as an indication of coming rain.

Skiddaw lies to the south.

(6.) A cloud on the top of the Sidlaw Hills foretells rain to the people of Carmylie.

The hills lie to the west of that district.

(7.) In New Cumnock there is a saying that, "If Corsancone puts on his cap, and the Knipe be clear, it will rain within twenty-four hours." This sign, it is said, never fails.

Corsancone Hill (872) is to the east, and the Knipe (1260) to the south-west of the New Cumnock district.

(8.) When the summit of the Bin Hill, to the south-west of Cullen, is covered with mist, rain is thought to be not far off.

(9.) The natives of Gigha and Cara anticipate rain when the Paps of Jura (to the north-west) are capped, and if the cloud be white, they expect wind with it. The Mull of Kintyre to the south, is used in the same way.

(10.) The capping of the Paps of Jura serves also as a prognostic of rain to the people of Kilcalmonell and Kilberry, which lie to the east of these peaks.

Note.—When a thick cloud on the top of a hill is observed to be in motion, the certainty of the prognostic is regarded as increased.

II. *Mists and Fogs.*

(1.) In the evenings of autumn and spring vapour arising from a river is regarded as a sure proof of coming frost.

(2.) Hazy weather is thought to prognosticate frost in winter, snow in spring, fair weather in summer, and rain in autumn.

(3.) Thin, white, fleecy, broken mist slowly ascending the sides of a mountain whose top is uncovered, predicts a fair day.

(4.) The following is a well-known *saw*, common among anglers in Balmacclellan, and generally in Kirkcudbrightshire:—

" When the mist creeps up the hill,
Fisher out and try your skill,
When the mist begins to nod,
Fisher then put past your rod."

(5.) White mist in winter indicates frost.

III. *Appearance of the Sky.*

(1.) A small cloudless space in the north-east horizon, especially if the clouds generally are moving to the south or south-west, and if the weather has previously been wet, is regarded all over Scotland, among seamen and landmen, as a very certain precursor of fine weather, or a *clearing up*.

(2.) In winter when the sky about mid-day has a greenish appearance to the east or north-east, snow and frost are expected.

(3.) "The evening red, and the morning grey,
Is the sign of a bright and cheery day;
The evening grey, and the morning red,
Put on your hat, or you'll wet your head."

Note.—The red after sunset must have a crimson tinge, and must last for some time. The red about sunrise may be a mere glare, and short lived. In fact, if it hold out till the sun is fairly above the horizon, many look for a fine day—though this is apparently opposed to the popular prognostic.

(4.) When, in the morning, the dew is heavy and remains long on the grass, when the fog in the valleys is slowly dissipated, and lingers on the hill sides, when the clouds seem to be taking a higher place, and when a few loose cirro-strati float gently along, serene weather may confidently be expected for the greater part of that day.

(5.) Continuous cirro-strati gathering into unbroken gloom, and also the cloud called "Goat's Hair" or the "Grey Mare's Tail," presage wind.

(6.) Light fleecy clouds in rapid motion below compact dark cirro-strati foretell rain near at hand.

(7.) When, after a shower, the cirro-strati open up at the zenith, leaving broken or ragged edges pointing upwards, and settle down gloomily and compactly on the horizon, wind will follow, and will last for some time.

(8.) When, after a clear frost, long streaks of cirrus are seen with their ends bending towards each other as they recede from the zenith, and when they point to the north-east, a thaw and south-west wind may be expected.

(9.) Cumulous clouds, high up, are said to show that south and south-west winds are near at hand; and stratified clouds, low down, that east or north winds will prevail.

(10.) "I first observed this kind of cloud (cumulous-like festoons of drapery) on 5th March 1822, when the barometer fell from 29.5 at 10 A.M. to 28.3 at 7 P.M., and it was immediately followed by a storm. Since then I have seen it several times, and,

when properly developed, it was always followed by a storm or gale within twenty-four hours. It is called "Pocky Cloud" by our sailors. On this occasion, the gale began early next morning, and continued till 2 P.M."—*Rev. C. Clouston, Sandwick Manse.*



Pocky Cloud, from drawing by Mr Clouston.

(11.) Cirrus at right angles to the wind is regarded as a sign of rain.

(12.) The farmers in Berwickshire say that a long stripe of cloud sometimes called by them a salmon, sometimes called Noah's ark, when it stretches through the atmosphere in an east and west direction is a sign of stormy weather, but when it stretches in a north and south direction, is the sign of dry weather.

(13.) Along the north shore of the Solway, from Dumfries to Gretna, a lurid appearance in the eastern or south-eastern horizon, called from its direction "a Carlisle Sky," is thought a sure sign of coming rain. They describe it as lurid and yet yellowish, and the common saying is,—

" The Carle sky
Keeps not the head dry."

(14.) In Kincardine of Monteith, and in all that district of country, the reflection from the clouds of the furnaces of the Devon and the Carron (to the east) foretells rain next day.

(15.) The glare of the distant Ayrshire ironworks being seen at night from Cumbrae or Rothesay, rain is expected next day. Similar prognostics are common all over Scotland.

(16.) A mackerel sky denotes fair weather for that day, but predicts rain a day or two after.

IV. *Moon and Sun, Rainbow, Aurora Borealis, Falling Stars, and Thunder.*

(1.) A few days after full or new moon, changes of weather from good to bad or bad to good, are thought more probable than at other times.

(2.) In winter, when the moon's horns are sharp and well defined, frost is expected.

(3.) When the moon has a white look, and when her outline is not very clear, rain or snow is looked for.

(4.) "Clear moon,
Frost soon."

(5.) If the old moon embraces the new moon, stormy weather is foreboded. Great confidence is placed in this old prognostic :

"I saw the new moon late yestreen
Wi' the auld moon in her arm,
And if we gang to sea, master,
I fear we'll come to harm."—*Sir Patrick Spens.*

(6.) Haloes predict a storm (rain and wind, or snow and wind) at no great distance, and the open side of the halo tells the quarter from which it may be expected.

(7.) Mock-suns predict a more remote and less certain change of weather.

(8.) "A rainbow in the morning—
Sailors take warning ;
A rainbow at night
Is the sailor's delight."

NOTE.—In the former case the rainbow will appear in the west, and in the latter in the east. In studying the prognostic this should be borne in mind.

(9.) The aurora borealis indicates change, especially if lurid and fiery. It is thought by many that a south-west wind often follows. Others, however, look for cold and storm when the aurora is very vivid. By some it is less, and by others more dreaded when seen low in the horizon. Many speak of it simply as indicating change—good to bad, or bad to good.

(10.) Numerous falling stars presage wind next day.

(11.) Sheet lightning, without thunder, during night, having a whitish colour, announces unsettled weather. In the west of Scotland, vivid flashes of lightning early in the morning are regarded as an unfavourable sign of the weather for that day.

(12.) A thunderstorm from the south is said to be followed by warmth, and from the north, by cold. When the storm disappears in the east, it is a sign of fine weather.

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(13.) The fragment of a rainbow in the north, which is called the "Boar's Head," makes the boatmen keep a look-out for wind. Stubs, however, are very variously interpreted.

(14.) People speak of the new moon "lying on her back, and being ill made," as a prognostic of wet weather.

(15.) "For a period of between thirty-five and forty years I have never known an exception to the rule, that the first great aurora, after a long tract of fine weather in September or beginning of October, is followed on the second day, and not till the second, about one o'clock on the east coast, and about eleven o'clock in Nithsdale, by a great storm; and that the next day after the aurora is fine weather, fit for all agricultural purposes."—*Professor Christison. (Communicated by Thomas Stevenson, Esq.)*

Note.—Professor Christison's character as an observer gives great importance to this prognostic.

V. Distant Objects seen with unusual Clearness.

(1.) When to the people about Arbroath the Bell Rock light is particularly brilliant, rain is expected.

(2.) About Cape Wrath, and along that part of the coast, when the Orkney Islands are distinctly seen, a storm or a continuation of bad weather is prognosticated.

(3.) When from Ardersier and the adjoining parish on the south-east side of the Moray Firth, the distant Ross-shire hills are distinctly seen in the morning, rain is expected that day.

(4.) To the people in Eaglesham, when the Kilpatrick hills appear near, a change to wet is looked for, but when they appear remote, dry weather will continue.

(5.) When Ailsa Craig is distinctly seen, and seems near at hand, the people of Cumbrae look for change. When the weather is going to be fine, it lies flat, but when rain is coming it assumes the form of a mushroom.

Note.—Darwin, in his "Zoonomia," thinks that the presence of vapour in the air increases its transparency, on the same principle as saturating a white opaque sheet of paper with oil renders it transparent.

There is a saying in Shetland,—*Everything looks large in an east wind.*

VI. Unusual Sounds.

(1.) In the Kilconquhar district of Fifeshire, when the noise of the sea is heard coming from the direction of St Andrews or St Monance, which lie to the north and east, bad weather from that quarter is expected. And when, afterwards, the sound is heard

coming from Kinraig, or from up the Firth, or in other words from the west, the wind is expected to fall.

(2.) At Roseneath, when the tide-stream at the ferry flows gently, yet resounds like a cataract, though the sky be starry and cloudless, rain may be looked for in the morning.

(3.) When the people of Monzie hear the sound of the water-falls of Shaggie, or the roar of the distant Turret clearly and loudly, a storm is expected; but if the sound seems to recede from the ear till it is lost in the distance, and if the weather be thick, a change to fair may be looked for speedily.

(4.) In Fortingall, if in calm weather the sound of the rapids on the Lyon is distinctly heard, and if the sound descends with the stream, rainy weather is at hand; but if the sound goes up the stream, and dies away in the distance, it is an omen of continued dry weather, or of a clearing up if previously thick.

Note.—The course of the Turret and Lyon is from west to east. This note refers to the two preceding prognostics.

(5.) Along the Dornoch coast, when the sound of the sea is distinctly heard as if breaking on the sandbank called the Gizzen Briggs, at the mouth of the Firth of Dornoch, it forebodes a storm, which will probably be from the east or north-east. When this is reckoned on as a prognostic, it is said "that the weather must be calm, and the sea with nothing more than a swell on."

(6.) In winter, when the sound of the breakers on the shore is unusually distinct, frost is indicated. This is very generally believed in.

(7.) If the noise of a steamer or a railway train is heard at a great distance, bad weather is predicted.

VII. *Underground Prognostics.*

(1.) In the collieries about Dysart, and in those of other districts in Scotland, it is thought by the miners that before a storm of wind, a sound not unlike that of a bagpipe, or the buzz of a bee, comes from the mineral, and that previous to a fall of rain the sound is more subdued.

(2.) Before wind and rain, it is also said, that the black damp, extinguishing the lights, is observed at the bottom of ironstone pits, and through the waste.

(3.) In Midlothian, the miners think that approaching changes of the weather are preceded by an increased flow of water, and the issue of gases and foul air from the crevices; and when very bad weather is at hand, these last escape with a characteristic sound, like the buzz of insects.

VIII.—*Plants.*

(1.) The *Convolvulus arvensis* (small bind-weed), *Anagallis arvensis* (scarlet pimpernel), and *Calendula pluvialis*, shut up their flowers before approaching rain. (The pimpernel has been called the poor man's weather-glass.) The same is believed to be true of the *Anemone nemorosa* (wood anemone), the *Oxalis Acetosella* (wood sorrel), *Nymphæa alba* (white water-lily), and *Bellis perennis* (common daisy).

Note.—Light probably determines the opening of most of these flowers, and their being found shut at 10 or 11 A.M. tells of cloud and gloom, and so predicts rain.

(2.) If *Stellaria media* (common chickweed) fully opens its flowers, no rain will fall for four hours.

Note.—Linnæus in his "Botanica Philosophia" shows that, with reference to the opening and closing of their petals, many flowers display an extreme sensibility; and it is quite probable that they may be influenced by those minor atmospheric changes which precede by a greater or less length of time the greater changes, and that they thus become real prophets of the approach of the last.

(3.) "Mony haws,
Mony snaws,"

is a common saying all over Scotland, and in some districts is otherwise expressed thus:—

"A haw year
Is a snaw year."

(4.) When the bramble blossoms early in June, an early harvest is expected.

(5.) If the oak is out in leaf before the ash, it will be a dry summer. If the ash first, wet.

IX. *Conduct, Movements, &c. of Animals.*

(A.) *Birds.*

(1.) The low flight of crows indicates coming rain. If they feed busily, and hurry over the ground in one direction and in a compact body, a storm (wind and rain) will soon follow. When they sit in rows on dykes or palings, wind is looked for. When going home to roost, if they fly high, the next day will be fair, and *vice versa*. If, when flying high, they suddenly dart down and wheel about in circles, wind is prognosticated. In autumn and

winter, if, after feeding in the morning, they return to the rookery and hang about it, rain is expected soon.

(2.) The landward flight and flocking of sea-gulls presage wind. When their cry is frequently repeated, when it is more lengthened and dismal than usual, and when it is heard in an inland place, rain or snow is prognosticated.

(3.) The frequently repeated cry of the ptarmigan low down on the mountains during frost and snow, indicates more snow and continued cold.

(4.) The gathering of grouse into large flocks indicates snow. Their approach to the farm-yard is a sign of severe weather—frost and snow. When they sit on dykes in the moor, rain only is expected.

(5.) When the cock crows at unusual times, wet or snowy weather is expected.

“If the cock crows on going to bed,
He’s sure to rise with a watery head.”

(6.) When the fieldfare, redwing, starling, swan, snowfleck, and other birds of passage, arrive soon from the north, it indicates the probability of an early and severe winter.

(7.) Swallows and swifts fly close to the ground or water before rain.

Note.—As a rule they fly lower in the evening and morning than during the day. Before thunder, however, they are often seen flying high. Such birds probably move about in that stratum of air in which the insects on which they feed are at the time, and that stratum is selected for warmth and dryness.

(8.) The drumming of the snipe in the air, and the call of the partridge, indicate dry weather and frost at night to the shepherds of Garrow.

(9.) “When dotterel do first appear,
It shows that frost is very near;
But when the dotterel do go,
Then you may look for heavy snow.”

Common also among Hampshire shepherds.

(10.) In severe winters the white swan visits the Orkneys. Its coming foretells continued severity.

(11.) When hens are observed to pick and pluck themselves more than usual, rain is near.

(B.) *Quadrupeds.*

(1.) Goats leave the high and exposed grounds, and seek shelter in a *bield* or in some recess, before a storm.

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(2.) Old sheep and ewes (less faith is placed in hogs) are said to eat greedily before a storm and sparingly before a thaw. When they leave the high grounds, and bleat much in the evening and during the night, severe weather is expected. In winter when they feed down the hill, a snow storm is looked for. When they feed up the burn, wet weather is near.

(3.) Swine carry straw in their mouths, and toss about their bedding before a storm.

(4.) Cats are observed to scratch the wall or a post before wind, and to wash their faces before a thaw. They sit with their backs to the fire before snow.

(5.) A day or two before rain, moles raise more hillocks than usual, and it is a sign of thaw when, after a long frost, they begin to work again.

(6.) Weasels, stoats, &c., seen running about in the forenoon, foretell rain in the after part of the day.

(7.) Hares take to the open country before a snow storm.

(C.) *Insects, Worms, &c.*

(1.) When gnats bite keenly, and when flies keep near the ground, we look for wind and rain.

(2.) Insects fly low before rain.

(3.) The earthworm appearing in large numbers on the surface indicates rain.

(4.) A leech in a bottle is considered a good weather prophet by many people. It is thought that in dry, calm weather, be it cold or hot, the leech will remain at the bottom; before rain or snow that it will cling to the side of the bottle at the top of the water; and that before wind it will be found in active motion.

(5.) When the colour of frogs is observed to be dark, wet weather is thought to be very close.

(6.) When the frog spawns in the middle of the water it is a sign of drought, and when at the side it indicates a wet summer.

(7.) When bees fly to the hive and none leave it, rain is near.

(D.) *Sensations experienced by Man.*

(1.) Headache, toothache, pain in corns, rheumatism, neuralgic pains, &c., are felt by some people before change from dry to wet, or mild to cold. Before thunder a feeling of listlessness, oppression, and uneasiness, is often complained of.

X. *Unclassified Prognostics.*

(1.) An honest man and a north-west wind generally go to sleep together.

Note—This is a common saying, and implies that if the wind is from the north-west in the early part of the day, it will veer or fall in the evening. It is common also in Iceland. The *Westing* appears to be important, for in Orkney the saying is: *The west wind is a gentleman, and goes to bed.*

(2.) Much undulation in the air near the surface of the earth on a hot day in May or June foretells a lowering of the temperature. This is called *Startling Jack*.

(3.) It is said to be a sign of continued good weather when the wind so changes during the day as to follow the sun.

(4.) There is a peculiar rippling of the wind, or broken way of blowing, which is said always to prognosticate heavy rain within a few hours.

(5.) Easterly gales, without rain, during the spring equinox, foretell a dry summer.

(6.) Rain, with a south-east wind, is expected to last for some time.

(7.) Hail, after long-continued rain, indicates a clearing up.

(8.) Frost suddenly following heavy rain seldom lasts long."

(9.) Short, slight showers, during dry weather, are called a *hardening of the drought*. Dust rising from the road in dry weather, when there is little wind, predicts change.

(10.) The Tweed, near its mouth, sometimes rises when no rain has fallen there. Rain, however, may be expected, and a south-west wind.

(11.) On the west coast, a gale from the west or south-west is frequently prognosticated by a great swell of the sea occurring during calm weather. The same phenomenon on the east coast would indicate a storm from the east or north-east, but the comparative narrowness of the waters on that side of our country probably makes this sign less valuable and less likely to occur.

Note.—That veteran observer, the Rev. C. Clouston, has directed attention strongly to this, which he has himself often verified.

This prognostic implies the existence of a storm in a distant part of the ocean, which is travelling in the direction of our country, and it involves the notion that the agitation caused by the wind on the water's surface travels faster than the wind itself. Mr Clouston also concludes from it that the breeze begins to windward, and takes some time to reach the point to which it proceeds to leeward.

The distant storm, which causes this agitation, may not actually reach our coasts, and the sign may thus fail.

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(12.) When the tide begins to ebb before high-water mark, and after receding a short distance, returns to high-water mark, or when, before low water, it flows for a short time and then ebbs again beyond the point from which the irregular flow started, a storm is believed to be near. These phenomena are vulgarly called *Leakies*. This prognostic is more particularly trusted on the shores of the Firth of Forth above Burntisland.

(13.) "Lang foul, lang fair,"

Note.—This is a calculation of probabilities, and springs from a faith in the constancy of averages in climate, as is the case with many other popular prognostics.

(14.) An unseasonably fine day in winter or spring is called a *pet day* in Scotland. The fate of pets, they say, awaits it, and they look for bad weather on the morrow.

(15.) "Who doffs his coat on a winter day
Will gladly put it on in May."

(16.) "If the grass grows in Janiveer
It grows the worse for 't all the year."

(17.) A green Yule makes a fat kirkyard.

(18.) "Winter thunder
Bodes summer hunger."

(19.) "Long foretold, long last;
Short notice, soon past."

(20.) March dust is worth its weight in gold.

(21.) "April showers
Bring summer flowers."

(22.) "In April a dove's flood
Is worth a king's good."

(23.) "A shower of rain in July,
When the corn begins to fill,
Is worth a plough of oxen,
And all that belongs theretill."

(24.) The sailor notes the tightening of the cordage on his ship as a sign of coming rain.

(25.) A lump of kelp acts as a good hygrometer, and prognosticates rain when it becomes damp.

(26.) When walls, built of stones which have been quarried below high-water mark, become damp, wet weather is near.

(27.) When from drains or soil-pipes a bad odour proceeds, foul weather is thought to be predicted.

(28.) When chimneys smoke and soot falls, bad weather is at hand. The whistling of the wind heard within doors denotes rain. When fires burn faster than usual, and with a blue flame, frosty weather may be expected.

(29.) When milk becomes suddenly and inexplicably sour, a thunder storm is at hand.

(30.) When spiders' webs are seen floating about in the air, farmers regard it as a sign of coming rain.

(31.) Heavy dews in hot weather indicate a continuation of fair weather, and no dew after a hot day foretells rain.

(32.) Addressed by Dr Jenner, in 1810, to a Lady who asked him if he thought it would rain to-morrow.

“ The hollow winds begin to blow,
The clouds look black, the glass is low :
The soot falls down, the spaniels sleep,
And spiders from their cobwebs creep :
Last night the sun went pale to bed,
The moon in halos hid her head :
The boding shepherd heaves a sigh,
For see a rainbow spans the sky ;
The walls are damp, the ditches smell,
Closed is the pink-eyed pimpernel ;
The squalid toads at dusk are seen
Slowly crawling o'er the green ;
Loud quack the ducks, the peacocks cry,
The distant hills are looking nigh ;
Hark, how the chairs and tables crack,
Old Betty's joints are on the rack ;
And see yon rooks how odd their flight,
They imitate the gliding kite,
Or seem precipitate to fall
As if they felt the piercing ball ;
How restless are the snorting swine,
The busy flies disturb the kine ;
Low o'er the grass the swallow wings,
The cricket too, how sharp she sings,
Puss on the hearth with velvet paws
Sits wiping o'er her whiskered jaws ;
The wind, unsteady, veers around,
Or settling in the south is found ;
The whirling wind the dust obeys
And o'er the rapid eddy plays ;
The leech disturbed is newly risen
Quite to the summit of his prison ;—
‘Twill surely rain, I see, with sorrow,
Our jaunt must be put off to-morrow.”

APPENDIX (A).

The Shepherd of Banbury's Rules to Judge of the Change of the Weather.—Extracts from the Second Edition, printed in London in 1748.

In the preface it is said, “ Most of our Shepherd's observations give us a day's notice, many a week's, and some extend to several months' prognostication of the changes of weather ; and of how great use these may be to all ranks and degrees of people,—to the sedentary valetudinarian as well

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as the active traveller,—to the sportsman who pursues his game, as well as the industrious husbandman, who constantly follows his labour,—in short, to every man in every situation in some degree or other, is so very clear and intelligible, that it would be a mere waste of words, and a very idle display of rhetoric, to attempt the making it clearer. Every man living would be glad to foresee the alterations of the weather if he could; and, consequently, to most people, if not to all, these observations, grounded on no less than forty years' experience, cannot but be acceptable."

I. SUN.—(1.) If the sun rise red and fiery—*Wind and rain.* (2.) If cloudy, and it soon decrease—*Certain fair weather.*

II. MOON.—(1.) Horns of the moon obscure—*Rain.* (2.) When the moon is red—*Wind.* (3.) On the fourth day of the new moon, if bright, with sharp horns—*No winds nor rain till the month be finished.*

III. STARS.—(1.) When stars shoot precipitant through the sky—*Approaching wind.*

IV. CLOUDS.—(1.) Clouds small and round, like a dapple grey with a north wind—*Fair weather for two or three days.* (2.) Large like rocks—*Great showers.* (3.) If small clouds increase—*Much rain.* (4.) If large clouds decrease—*Fair weather.* (5.) Clouds in summer or harvest, when the wind has been south two or three days, and it grows very hot, and you see clouds rise with great white tops like towers, as if one were upon the top of another, and joined together with black on the nether side—*There will be thunder and rain suddenly.* (6.) If two such clouds arise, one on either hand—*It is time to make haste to shelter.* (7.) If you see a cloud rise against the wind or side wind, when that cloud comes up to you, the wind will blow the same way that the cloud came. And the same rule holds of a clear place, when all the sky is equally thick, except one clear edge.

V. MISTS.—(1.) If mists rise in low ground and soon vanish—*Fair weather.* (2.) If they rise to the hill tops—*Rain in a day or two.* (3.) A general mist before the sun rises, near the full moon—*Fair weather.* (4.) If in the new moon—*Rain in the old.* (5.) If in the old—*Rain in the new.*

VI. WINDS.—(1.) Observe that in eight years' time there is as much south-west wind as north-east, and consequently as many wet years as dry. (2.) When the wind turns to north-east, and it continues two days without rain, and does not turn south the third day, nor rain the third day, it is likely to continue north-east for eight or nine days—*all fair*; and then to come to the south again. (3.) If it turn again out of the south to the north-east with rain, and continue in the north-east two days without rain, and neither turns south nor rains the third day—*It is likely to continue north-east for two or three months.* The wind will finish these turns in three weeks. (4.) South-west winds. After a north wind, for the most part two months or more, and then coming south—*There are usually three or four fair days at first, and then on the fourth or fifth day comes rain,* or else the wind turns north again,

and continues dry. (5.) If it return to the south within a day or two without rain, and turn north with rain, and return to the south in one or two days as before, two or three times together after this sort—*Then it is like to be in the south or south-west two or three months together, as it was in the north before. The winds will finish these turns in a fortnight.* (6.) Fair weather for a week, with a south wind, is like to produce a great drought, if there has been much rain out of the south before. The wind usually turns from north to south, with a quiet wind without rain, but returns to the north with a strong wind and rain; the strongest winds are when it turns from south to north by west. (7.) When the north wind first clears the air, be sure of a fine day or two.

VII. RAINS.—(1.) Sudden rains never last long; but when the air grows thick by degrees, and the sun, moon, and stars shine dimmer and dimmer—*Then it is like to rain six hours usually.* (2.) If it begin to rain from the south, with a high wind, for two or three hours, and the wind falls, but the rain continues—*It is like to rain twelve hours or more, and does usually rain till a strong north wind clears the air.* These long rains seldom hold above twelve hours, or happen above once a year. (3.) If it begins to rain an hour or two before sun-rising—*It is like to be fair before noon, and so continue that day;* but if the rain begin an hour or two after sun-rising—*It is like to rain all that day,* except the rainbow be seen before it rains.

VIII. SPRING and SUMMER.—If the last eighteen days of February and ten days of March be for the most part rainy, then the spring and summers quarters are like to be so too; and I never knew a great drought but it entered in that season.

IX. WINTER.—If the latter end of October and beginning of November be for the most part warm and rainy, then January and February are like to be frosty and cold, except after a very dry summer. If in October and November there be snow and frost, then January and February are like to be open and mild.

X. ANIMALS.—(1.) Cormorants swiftly returning from sea to land, making a great noise—*Winds.* (2.) The heron forsaking the fens and soaring aloft—*Winds.* (3.) Cranes forsaking the valleys, heifers snuffing the air, swallows fluttering about the lakes, frogs croaking, ants conveying their eggs from their cells, ravens flocking together and making a great noise, bees in clusters humming about the hive—*Rain.*

APPENDIX (B).

The three following popular Prognostics are discussed by

M. ARAGO :—

1. If the horns of the lunar crescent, on the third day after new moon, are sharply and clearly defined, the weather may be expected to be fair during the ensuing month.

2. If on the fourth day the moon project no shadow, we are to expect bad weather during the month.

3. A change of weather accompanies a change of the moon.

On Revolving Storms. By JOSEPH JOHN MURPHY, Esq.*

When the temperature of the air is in a state of convective equilibrium, or, in other words, when the difference between the temperatures of any two strata is that which is due to the difference of pressures, it is obvious that any decrease in the specific gravity of the lower stratum, whether from expansion by heating, or by mixture with watery vapour, which is lighter than air, will give that stratum a tendency to rise; but as an entire stratum cannot rise in mass, the air will flow upwards in vertical columns, the place of which may be determined by the smallest inequality in the ground, or motion on it. If the ascending force is powerful and the ground very dry, the ascending columns of air will be marked by clouds of dust or sand, and it is thus that the "dust-storms" of Western India, and the "sand-spouts" of the African desert, are produced.

Professor Hennesy has observed that such vertical circulation is shown to exist in our climate by the rapid fluctuation of a thermometer exposed to the free air in warm sunshiny weather; and cumulous cloud is produced when the watery vapour contained in such an ascending column is condensed by the cold of diminished pressure.

When condensation takes place in an ascending column, the cooling from diminished pressure will be retarded by the heat liberated in condensation, and the ascending column, at any altitude higher than that at which condensation has begun, will be warmer than the surrounding air on the same level, supposing, as at first, that the temperatures of the surrounding air are in a state of convective equilibrium.

When unity of weight of vapour is condensed, heat enough is liberated to raise the temperature of an equal weight of water by 1178° Fahr., minus the temperature at which condensation takes place. To estimate the effect of this in heating air, the quantity must be divided by $\cdot 2377$, which is the specific heat of air at constant pressure; and to estimate the effect of the condensation of unity of *volume*

* Communicated to the Belfast Natural History and Philosophical Society, 29th October 1862 and 7 January 1863.

of vapour, we take $\frac{5}{8}$ ths of the quotient, as watery vapour has only $\frac{5}{8}$ ths of the specific gravity of air. Supposing condensation to take place at 80° , the heating effect will be

$$\frac{5}{8} \cdot \frac{1178 - 80}{\cdot 2377} = 2877$$

In other words, the condensation of any given volume of vapour will liberate as much heat as, if concentrated, would raise the temperature of an equal volume of air by 2877° Fahr.

This heat will expand the air. The addition of a degree Fahr. of temperature expands air at 32° by a 492d of its volume, and at 80° by a 540th. Consequently the expanding effect at 80° will be, after deducting the destroyed volume of the condensed vapour,

$$\frac{2877}{540} - 1 = 4.35$$

In other words, for every cubic foot of vapour condensed, 4.35 cubic feet will be added to the volume of the air. This expansion will increase the ascending force of the column, and produce an outward flow of air at its top, a fall of the barometer at its base, and an inward flow *towards* its base to fill up the void.

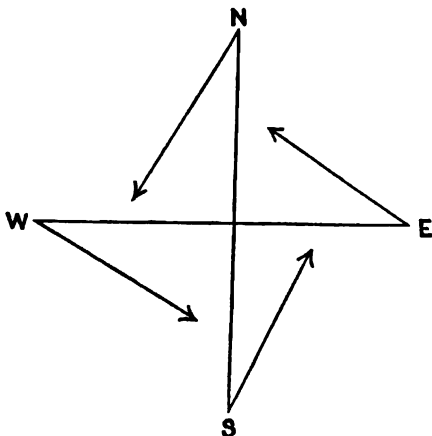
The foregoing reasoning is identical with that of "Espy's Philosophy of Storms." In order, however, to estimate the force of indraft at the base of such an ascending column, it would be necessary to know not only the temperature at every height in the column itself, which is a matter of calculation when that at its base is known, but also the temperature of the surrounding air at every height, a subject on which very little is known for any given time and place; for the ascending force of one mass of fluid in another depends on the difference of specific gravity, and this with air depends almost entirely on the temperature.

It has been stated by Professor Thomson of Glasgow, in a paper lately read at the Manchester Philosophical Society, that the temperature of air *saturated* with moisture, when in a state of convective equilibrium (as an ascending current of such air must necessarily be), will diminish at the rate of

1° Fahr. for every 294 feet of ascent ; but the temperature of dry or not saturated air will diminish at the rate of 1° for every 183 feet. Suppose that the air is nearly saturated in its lowest strata, and cloudless above, and that for the first few thousand feet of ascent the temperature diminishes at the rate due to convective equilibrium in dry air (conditions that sometimes occur in summer weather), the slightest increase of the temperature of the lowest stratum will produce an ascending current, in which the cold of diminished pressure will cause condensation to begin at a moderate height, and from that height upwards, the decrease of temperature will be at the rate of 1° Fahr. for 294 feet, while in the surrounding air it is at the rate of 1° Fahr. for 183 feet ; so that, at any given level, the air in the ascending column will be warmer and lighter than the surrounding air at the same level, and a powerful upward current will be produced, with a powerful indraft at its base. Of this nature, apparently, are many violent storms of small extent ; they are usually, perhaps always, accompanied by heavy rain or hail from the vapour condensed above.

Revolving storms or cyclones differ from these, primarily, in their larger extent. A cyclone cannot be formed unless the radius, within which the indraft due to the central barometric depression is felt, has a sensible magnitude in comparison with the earth's quarter-circumference. At its centre is an area of dense cloud, great barometric depression, and no wind. The air revolves round this, and has also an inward motion, producing a spiral resultant. The wind is most violent near the calm centre, and grows less so outward. They rotate in opposite directions in the northern and southern hemispheres ; in each hemisphere they rotate in the same direction in which the earth rotates round an axis drawn perpendicular to its surface, as shown in Foucault's pendulum experiment, that is to say, *against* the direction of the hands of a watch in the northern hemisphere, and *with* it in the southern. This is caused by the earth rotating under the currents of air, as it does under Foucault's pendulum, and deflecting their paths on its surface to the right in the northern hemisphere, to the left in the southern ; thus, in the northern hemisphere, the current from N towards an area

of rarefaction will be deflected toward W, that from W towards S, that from S towards E, and that from E towards N. As these deflections are all in the same direction round a circle, they combine and produce a vortex. All these directions are, of course, reversed in the opposite hemispheres. *The rotation of the storm is in the opposite direction to the deflection*, or what Dové calls the gyration, *of the separate winds*, as will be obvious from the diagram, where the cross represents the converging paths which the winds would take in the absence of any deflecting force, and the arrows the directions in which they are deflected. Of course it is not



accurate to draw the arrows straight. Were there no friction, the velocity would be regulated by the law of the conservation of areas, and its tangential component in any part of the cyclone would be inversely as the distance from the centre. Cyclones never cross the equator, and are never formed on it, because the deflecting force is in opposite directions in opposite hemispheres, and null on the equator.

Such are the forces that move the cyclone. But the manner in which it is originally set agoing has not hitherto been fully explained. The following explanation is founded on that contained in Dové's recently translated "Law of Storms," and is partly identical with it:—

Cyclones are local and temporary phenomena: they occur chiefly in, 1st, The West Indian Seas; 2^d, The Indian Ocean west of India; 3^d, The South Indian Ocean; 4th, The Chinese Sea; and 5th, The Bay of Bengal: in all these they occur oftenest in the end of summer, except in the last, where they are most frequent twice a year, at the change of the

monsoons. They are certainly not formed in the South Atlantic, and we have no evidence of their existence in the Pacific, except perhaps off the coast of California.

They are formed only over tropical seas, because there only the air contains watery vapour enough to furnish their motive power by its condensation. But they originate in *eddies formed in the general circulation of the atmosphere.*

The heated air that rises up during summer over the warm regions of Asia and Africa flows out over the Atlantic as an upper current, where it meets the upper return trade wind. The direction of the former is nearly towards W., that of the latter nearly towards N.E., so that the resulting current will be N.N.W.; and on its right-hand side, towards Europe, eddies will be formed rotating *with* the hand of a watch, and on its left hand, or West Indian side, will be eddies rotating in the opposite direction. Any one may make such a pair of eddies by moving a teaspoon forward through a cup of liquid. At the centre of every eddy is a depression, produced by centrifugal force, and in an aerial eddy this will be marked by a diminution of barometric pressure. When the eddy rotates in the opposite direction to that of the cyclones of the hemisphere in which it occurs, no cyclone can be produced; but when it rotates in their direction, and other circumstances are favourable, the barometric depression at the centre produces cold—the cold of diminished pressure: this condenses part of the vapour in the air: heat is liberated, and an upward current produced, as explained at the beginning of this notice: an outflow of air takes place above and an indraft below: these radial motions combine with the rotatory motion of the eddy, and thus the eddy is converted into a cyclone.

The cyclones of the Indian Ocean west of India are produced in exactly the same way, by the meeting of the upper currents from the heated regions of Asia and Africa: and those of the South Indian Ocean, by the upper current from the heated regions of Australia meeting the upper return trade wind.

In these cases the eddy is formed originally in the upper strata of the air. In the Chinese Sea and the Bay of Bengal, on the contrary, it is formed on the surface of the sea,

by the meeting of the N.E. trade wind and the S.W. monsoon; this takes place over the Chinese Sea in summer, and over the Bay of Bengal after the equinoxes.

Besides their rotation, cyclones have a motion of translation, and this, within the tropics, is always *towards W.*; sometimes N.W., sometimes S.W., but always W. In the Chinese Sea this is contrary to the prevalent wind at the time they occur, which is the S.W. monsoon. This has not hitherto been accounted for. It is probably due to the fact that the rotation of the earth round an axis drawn perpendicular to its surface, as illustrated by Foucault's pendulum, varies as the sine of the latitude, and is consequently most rapid in the highest latitude; so that the higher the latitude from which the air in any part of a cyclone is drawn, the more it will have been deflected, and the greater will be its centrifugal force. Now, a glance at the diagram will show that the air on the west side of every cyclone comes from a higher latitude, and that on the east from a lower; the former has consequently the most centrifugal force, and determines the motion of the whole cyclone towards west.

It might be supposed that the motion in question is a resultant of the motions of the upper and lower currents of air, which in the tropical regions are usually different. This, however, could scarcely be reconciled with the fact that in the West Indian region the lower current (the trade wind) is from N.E., while in that of the Chinese Sea, during the prevalence of cyclones, it is from S.W. Little is known of the direction of the upper current in either case; it is probably rather fluctuating, but more nearly opposed to the lower currents than coincident with them. It appears probable that the resultant of the motions of the upper and lower currents in the West Indian region is more likely to be in the opposite direction to the same resultant in the Indian and Chinese region, than in nearly the same direction.

The cyclones of the West Indian Sea and South Indian Ocean, besides their motion to west, generally move towards the Pole. On reaching a latitude of about 28° , they lose much of their force, widen rapidly, and, losing their motion towards west, drift eastward and Pole-ward, with nearly the

general motion of the lower strata of the atmosphere in the middle latitudes. These changes have not been satisfactorily accounted for. *Perhaps* they are due to the fact that in tropical latitudes the general direction of the upper and lower air-currents is usually very different, while in higher latitudes it is usually nearly identical; in the former the cyclone, extending from the lower into the upper current, is as it were compressed between them: when it gets into the latter, being no longer thus compressed, it expands by its own centrifugal force, loses force by expansion, and drifts in the general motion of the air. Its motion, however, is more Pole-ward than this, and is perhaps a resultant of the general motion of the air, and a remaining westward tendency of the cyclone, due to the cause already explained.

Description of a Plant-house at Rockville, Blackrock, near Dublin. By Mr JAMES BEWLEY.*

My fern-house is a triple-span roof standing north and south, 60 feet wide—i.e., three spans of 20 feet each; the length is 48 feet; the height of the sides to the spring of the roof is 13 feet 6 inches—of this, 7 feet is a wall; in the lower part are ventilators near the ground, which are opened and shut from the outside; the rest of the sides is glazed with fine ribbed glass. I think it is called Hartley's rough plating. The centre span is supported by three pillars, 12 feet apart from centre to centre. These pillars are rough brown slate, square, about 3 feet each way. The stones are laid horizontally, with ample spaces between, from the mortar being picked out for plants. From pillar to pillar are Gothic arches of the same material, covered with ferns, lycopods, &c. Thus there are two rows of four arches each. Each side has in addition a row of eight arches of 6 feet each, planted in the same manner, each roof rises about 7 feet; so the highest part of the house is 21 feet. The north end is a wall, against which is a mass of

* Communicated by Mr N. B. Ward to the British Association Meeting at Newcastle-on-Tyne, August 1868.

irregular rockwork about 10 feet high, with concealed steps to the top, and from these a path from side to side to enable a view to be had of the plants below, there being a number of tree ferns standing on the floor. This path is depressed so as to allow a person to pass under the lowest part of the roof. On the north side of the path, the rockwork is carried up to the top of the house, so that when viewed from below, the rockwork is seen to extend from the ground to the glass. An irregular rockwork is also carried round the south end, and along the east and west sides, up to the glass (7 feet); but it is separated from the outer walls by a space of 6 inches to allow ventilation from the outside, and also for the circulation of heat, from a pair of 4-inch water-pipes which run round the house. This is the only heating power. In order to promote a circulation of air, there are openings, in fact small concealed arches in the rockwork, on the ground level, passing through it to the narrow space next to the outer wall. Thus the hot-water pipes being next to the outer walls, and there being ventilators in these, and openings on the ground level inside, through the rockwork, there is a perfect circulation of air maintained—a matter all important to the health of plants. The roof is one of ordinary construction; wooden rafters 4 feet apart, with three light iron sash bars between. (I may here remark, I prefer the combination of wood and iron to either exclusively. All iron has too uniform an appearance to please me. Wooden rafters 4, 5, or 6 feet apart break the dull uniformity into spaces, and are of great practical convenience, by enabling a nail, screw, or hook to be put in whenever wanted.) The inner glass is common 21 oz.; it is put in about 7 inches on the *under side* of the rafter; so that, the rafter being about 6 inches in depth, 5 inches of it are outside. Now, on these rafters the outer roof rests—simple framed sashes, 4 feet wide,—and for the convenience of moving, in two lengths. They are glazed with the same glass as the sides, and are fixed in their places by a screw bolt, so as to be removed when required. Thus the inner and outer glass are from 5 to 6 inches apart. I do not think the distance is important; anything from 4 to 6 inches will do; but what is all im-

portant is, that the outer sashes should fit close, so as to admit of no circulation of air between. I do not like moveable sashes for top ventilation in any house, and therefore in mine I carry up the glass only to within one foot from the top of the ridge; above this are wooden shutters or panels for ventilation, and by a very simple contrivance they can be opened or shut.

Now, as to temperature. My fern-house being 60 feet by 48, with a mean height of about 16 feet, contains 46,000 cubic feet of space; and there being a pair of 4-inch hot-water pipes running round it, the heating power is 430 feet of pipe, or about 1 foot of pipe to 100 of cubic contents. In a single roofed house, I do not think this heating power would do more than exclude frost in severe winter weather, but with the double roof, the thermometer rarely goes below 48. I think I *once* saw it at 46. My estimate is that I gain 14 or 15 degrees of heat from the double roof. The loss of heat is very slow in cold weather; I think it would take three or four nights of *severe* frost to bring the heat of the house down from 52 to 48; and probably thirty-six hours to cause the same reduction were the fire to be left out altogether. With a single roof, this would be expected in, I think, about six hours. It is a great comfort to know that if, in any house I have, the fire was let out, or neglected in the depth of winter for a whole night, its contents would suffer no damage. The reverse effects are equally interesting,—the very slow increase of heat in warm weather. This has been a hot summer, yet the heat of the fern-house has never, that I am aware of, gone up to 70; with a single roof, and the same amount of ventilation, the house would run up to 80 or 85 in a single hot morning. There is another interesting fact, and one of great importance in plant-growing: there is a steady uniform increase of temperature in the house according as we ascend from the floor, day and night. This arises from the non-transmission of heat through the roof. Thus, while I have native ferns and other temperate plants growing below, I have tropical plants, ferns, and palms, &c. growing on the higher portions of the rockwork. And while, in ordinary houses, we remove tender plants from near the glass in severe weather, the nearer I

place them to it, the warmer they are. This is curiously indicated by the little creeping *Ficus* and other plants clinging to the under side of the inner glass, and spreading over it, apparently in great enjoyment. Another important result takes place from the non-radiation of heat through the roof. In ordinary houses, where moisture forms a requisite of good plant-growing, no matter how freely we use the syringe in the evening, we find the house *dry* in the morning, arising from the great condensation of moisture on the roof, and running off when thus condensed. The cooled air having parted with its moisture in the morning when the temperature rises a little, it becomes absolutely dry, to the great detriment of the plants. With the double roof, there is very little condensation on the roof, the blanket of non-conducting air between the two roofs of glass preventing it; and consequently, as the air cools down a little at night, the condensation takes place on *the plants themselves*, exactly according to nature. I have gone into my orchid-house, which has a double roof and sides, of a morning, and have seen the plants as beautifully covered with dew as a plot of cabbages on a May morning. Is not this nearly the perfection of plant-growing? We generally commence fire heat in the fern-house when the cold weather sets in, in the latter end of October, and leave it off in March, when the house gets about 54. Thus we have fire for about five months only in the year, and during these five months, the consumption of fuel is not more than one-half of what it would be if the house had but a single roof. Now, using only half fuel for five months, and none during the other seven, causes a very large saving, and to this must be added the saving from no fire attendance during the same seven months,—no small item. These together I calculate to amount to about 20 per cent per annum, on the extra cost expended on the double roof. But this is trifling compared with the enhanced value of the plants grown in a double-roofed house. The effect is astonishing. Plants which with difficulty held their own, or but slowly improved in an ordinary house, have grown and improved with me in a way that would surprise plant-growers in general. I should add, there is one point on which great caution is requisite

by any one who adopts double-roofed houses: from the uniformity of temperature, and the consequent non-condensation of moisture, very moderate waterings and syringes are sufficient to keep the plants in health. A beginner will be very apt to overdo it. My fern-house has nothing done to it for weeks together beyond a very light syringing each morning in summer, two or three times a week in spring and autumn, and once every week or ten days in winter.

My large plant-house is about 130 feet long, 19 wide, and 11 high. It had shelves at either side 3 feet wide, then paths of equal width, and a central stand 7 feet wide; beneath each of the side shelves there were two 4-inch flow pipes and two returns, twelve pipes in all. When I double-roofed it, the heat was too much, so I built a low brick wall on either side, and also enclosed in the same way the central 7 feet, and filled up the spaces with gravel, &c., to about 3 inches over the pipes; the gravel is kept damp, and on it the plants stand—on a hotbed, in fact, and they delight in it. I think any one who will cover up all his heating power in this way will soon find that it is superior to exposure; the heat is so gradually diffused and the moisture also, the gravel beds being frequently watered. The peculiarity and advantage of double-roofed houses may be summed up thus:—Great economy of heating power, and great uniformity of temperature and moisture; and consequently remarkable facility in growing plants in perfect health and beauty. So far as my experience has gone—and I have now been trying it for some years,—I can have no hesitation in recommending the system to every person for the cultivation of *any* description of plants.

The Great Mixed Forests of North America in Connection with Climate. By J. B. HURLBURT, LL.D.*

This vast forest, composed principally of deciduous trees, covers an area of about 2,000,000 square miles, extending from the Gulf of Mexico to the northern boundary of Canada, and from the Atlantic to the prairies of the west,

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in some places beyond, and in others not to the Mississippi. From this to near the Pacific, within the limits of the United States, intervene the naked interior plains for nearly 4000 miles, succeeded on the Pacific coasts by the forests of Coniferæ. From about the 98th meridian west, the forests of deciduous trees extend in a north-west direction high up into British America, with a belt of low evergreens around Hudson's Bay, and to the east of the northern parts of the valley of Mackenzie's River.

This vast compact forest, made up mainly of deciduous trees, is interspersed with large areas covered with coniferæ, and with others covered with deciduous trees and coniferæ intermixed, and to the south with tropical evergreens.

These peculiar forests, prairies, and desert areas, no doubt express some distinctive features of climate. Two conditions of climate—heat and humidity—marked in a high degree, seem essential for the development of those gigantic forests of deciduous trees. High summer temperatures and abundance of summer rains, are characteristics of the whole of the area covered by these forests. The temperatures for the three summer months over all these forest areas within the United States, Canada, and British America, are above 65°, and mostly above 67° of Fahr.,—temperatures high enough and long enough continued to mature maize, or Indian corn, and the grape; for the wild vine (*Vitis cordifolia*) matures its fruit to the northern limits of the deciduous forests. Twenty inches of rain in the states bordering the Gulf of Mexico, and ten inches in the north and through Canada, give, in general terms, about the quantities that fall during the three hottest months—June, July, and August. Similar high summer temperatures prevail over the western prairies and interior desert areas, but with a partial or total failure of the summer rains, and here we find the absence first of all forests, and then of all vegetation. On the Pacific coast, in high latitudes especially, the summer rains are abundant, but the summer temperatures are low, falling to 57° and 60°; and here are the most extraordinary examples of the coniferæ, but the deciduous trees are wanting.

Within the limits of these three great divisions we meet

with many modifications, which, however interesting, as illustrating the effects of climate upon the flora of that continent, it forms no part of the objects of this short paper to particularise; but an example or two may be given. We have upon the White Mountains of New England an instance of the failure of all forest trees above an altitude of 5000 feet. The summits of the Black Mountains of North Carolina, as the *swartz wold*, or black forest of the hills of Badan, are covered with balsam firs, and the shores of Hudson Bay with low evergreen forests. In all such localities the results are what the lower summer temperatures might have been expected to produce, as the higher temperatures of the coasts of the Gulf of Mexico and the Atlantic as far north as Virginia have caused other varieties of forest trees, such as the evergreen oaks, magnolias, palms, cypress, giant laurels, wild orange, satinwood, mahogany, and mangroves.

The principal trees of these great mixed forests are of the *Deciduous* kind—the maple (*Acer saccharinum* and *A. dasycarpum*), the beech, the elm, ash, oaks, basswood or American linden (*Tilia americana*), chestnut, cherry, butternut, walnut, hickory, poplars, magnolias; and of the *Coniferae*—the pines, balsams (*Abies*), tamarac (*Larix americana*), cedars (*Thuja occidentalis*) and (*Juniperus virginiana*), hemlock spruce (*Abies canadensis*).

The most important of these great mixed forests are in Canada, extending from the northern shores of Lake Erie, in lat. 42°, to the northern limits of Canada. Some of these trees have a very extensive range north and south. The sugar maple (*Acer saccharinum*), is found near Lake Winnipeg, in lat. 50°, and the black sugar maple (*A. nigrum*), in lat. 32°, in Louisiana. The beech and elms have a range quite similar to the maple. The American linden also goes as far northward. The white pine (*Pinus Strobus*), prevails in the northern parts of these forests, and the red pine (*P. resinosa*), in the southern. The red cedar (*Juniperus virginiana*), has the greatest range, extending from lat. 26° in Florida to 67° within the arctic circle. The wild cherry, too (*Prunus americana*), a large forest tree, and the shad-bush (*Amelanchier canadensis*), extend over nearly as many degrees. The trembling poplar (*Populus tremu-*

loides), ranges from 37° to 69°. The red current (*Ribes rubrum*), which is found in all the American and Canadian forests, Sir John Richardson discovered growing on the shores of the Arctic Ocean, within the polar circle.

Through all this range of latitude, wherever these deciduous trees are found, these two conditions of climate prevail—high summer temperatures and abundant summer rains; the cold of winter, with the thermometer even at 40° below zero, seeming to produce no effect upon such plants.

On the Colour of the Salmon. By JOHN DAVY, M.D., F.R.S.,
London and Edinburgh, &c.*

All that relates to fish of so much importance as the salmon and its allied species naturally excites our interest. Amongst the many peculiarities of the genus, when the individuals are in their highest condition and best state for the table, is their colour—the colour of their muscles generally—the well-known salmon colour. No other fish that I am aware of, whether in the sea or fresh water, possesses this colour, not even those which are associated with it so often, and the food of which is very similar. Of fresh-water fish, the pike may be mentioned as an example; of salt-water, the *Perca marina*.

On what does this colour depend? Commonly I have heard it attributed to an oil. The circumstance, that the colour is seen only in fish in their highest condition, and that in river and brook-trout it is often absent, seem favourable to this opinion, and the more so, as we know that the same trout which have pale muscles in streams where there is but scanty feed, if removed into ponds or lakes where there is abundance of food, whilst they rapidly increase there in size, acquire also the peculiar hue distinctive of their improved condition. The fact, too, that the salmon loses this colour during its lengthened sojourn in fresh water after spawning, when it becomes lank and out of con-

* Read at the Meeting of the British Association for the Advancement of Science (Newcastle, 1863).

dition, eating during the period, if anything, but very little, seems likewise to favour the opinion. Yet I am doubtful that this opinion is correct, and will bear being inquired into. If an examination be made of the salmon in its highest condition and richest colour, when taken in the sea, or immediately after entering fresh water, what, in relation to colour, are its several parts? Whilst the muscles of the superior section of the body, the thick part, where, comparatively, there is very little fatty or oleaginous matter, exhibits the peculiar colour in perfection, those of the inferior section—the thin portion of the fish—are comparatively pale, and yet there there is a superabundance of oleaginous matter, and of a colourless kind. Moreover, there are certain muscles, those connected with the eyes and the jaws, and the great dorsal fin, which are altogether destitute of the colour in question. Those of the eye, in the midst of cellular tissue, loaded with white adipose matter, are almost colourless. Those of the jaws, where there is very little fatty matter, are of a brownish hue. Those connected with the fin, where, too, there is little fat, are of a dark brown. There is another fact, with the same negative bearing. This is, that the air-bladder of the charr is of a pink colour, often of a beautiful tint (a refinement, as it were, of the salmon-colour, and of the colour of its own muscles), though altogether destitute of oily or fatty matter; and I have seen it thus coloured in fish which, though in good condition, and cutting red, like the salmon, have not been remarkable for richness.

The conclusion I am disposed to come to is, that the salmon-colour is unconnected with oil or fatty matter, and that it belongs to the coloured muscles after the analogy of the colouring of the muscles of various other animals, as witnessed in the instances which come under our observation in the way of meats. In beef, mutton, venison, the hare of proper age, how dark is the muscular fibre. The same colour is seen in the goose, and still more strikingly in the swan, and in most wild birds, and also in certain fishes, especially of the tunny family.

The results of the few chemical experiments which I have made seem to favour the same conclusion. The coloured

muscle of the salmon is rendered paler by the action of a strong solution of ammonia—the aqua ammoniæ—the solution itself remaining colourless. By strong nitric acid it is rendered yellow; by strong sulphuric, brown; by a solution of potash—the liquor potassæ—brown; by strong muriatic acid a slight change of colour is produced; whilst rendered paler, it acquires a brownish tint. Both the strong acids, the nitric and the sulphuric, it may be remarked, and also the solution of potash, have a disorganising, decomposing effect, to which the change of colour they occasion may principally be attributed. Alcohol and ether render the muscle of a lighter hue, but do not deprive it entirely of colour. Both become very slightly coloured, and on cooling and evaporation deposit and leave a colourless liquid oil.

Granting that the colour belongs to the muscular fibre,—i.e., that the colouring matter is seated in the muscle irrespectively of fat,—the further conclusion I would submit is, that it is of an organised kind. It may be a peculiar colouring matter nowise essential to the muscle (some of the muscles, as I have pointed out, being without this colour), and analogous to those colouring matters which exist in plants, especially their leaves and flowers, and in the tegumentary parts of mammalia, birds, and fishes,—such as the hair of the first, the feather of the second, and the epidermis of the third.

Whether the peculiar colour or colouring matter owes its formation to some particular kind of food, or to a special secreting power, I am not aware of any well-marked facts to guide one to a conclusion. So far as probabilities are concerned, I am rather inclined to the belief that it depends partly on the one partly on the other.

Some Observations on the Eggs of Birds. By JOHN DAVY, M.D., F.R.S., Lond. & Edin., &c.*

Although few objects have received more attention from the physiologist and chemist than the egg of the common

* Read at the Meeting of the British Association for the Advancement of Science (Newcastle, 1868.)

fowl, the eggs of other birds have been much neglected,—indeed, I am not acquainted with any researches that have been published with the intent of ascertaining either the proportions of their constituents, or the qualities of these constituents, from a comparative point of view.

The observations I am about to make, I can offer only as a small contribution to so large a subject,—a subject of especial interest, considering the vast variety of products,—that is, the numerous species of the feathered tribe which are evolved, each distinct, yet as soon as hatched, though developed from similar elements, displaying the form, tastes, and habits characteristic of its kind. I shall first advert to some of the general properties of eggs.

Without exception, the laying birds, whatever their species, when in a healthy state and supplied with a sufficiency of lime, have their eggs enveloped in a resisting shell composed of membranes strengthened by an earthy incrustation, consisting chiefly of carbonate of lime. The thickness of this incrustation in eggs of different birds is very various. It seems to bear some relation to the weight of the incubating bird, and the time of incubation and hatching. Generally, the smaller the bird, and the shorter the period of foetal development, the thinner is the incrustation,—the elasticity of the shell increasing with its diminution of size. I shall give a few instances in illustration, in which, were the thickness carefully measured, it would bear, I believe, a near proportion to the circumstances mentioned,—of each of the following birds, the incubating time is given in days:—

The swan, ...	42 days	The pheasant, . . .	23 days
The common goose, . . .	35 „	The red grouse, . . .	23 „
The common duck, . . .	28 „	Pigeon, ...	14 „
The common fowl, . . .	21 „	Turtle-dove, . . .	14 „
The turkey, ...	28 „	Canary bird, . . .	13 „
The guinea-fowl, . . .	30-31 „	Wren, ...	10 „
The partridge, . . .	27 „		

These periods are given from such information as I have been able to collect from poulterers, bird-fanciers, and keepers.* They may not be all quite correct, were they

* The time of incubation of the following birds, is copied from H. L. Meyer's

so, the ratio referred to certainly would not be one of perfect exactness. Probably there is another specific element concerned—vital force; thus, though the egg of the bantam is very much smaller than that of the barn-door fowl, yet the same time is required for its hatching.

Whatever the degree of thickness of the shell, it is invariably pervious to air, and chiefly, I believe, through minute apertures—*foramina*—in the crust. In the egg of the common fowl these are tolerably conspicuous. In every instance that I have put an egg under water deprived of air by the air-pump, on fresh exhaustion air has been seen to rise in currents from particular points, affording proof of the existence of such *foramina*. Granting their existence, they are certainly covered internally with membrane, through which the air must pass, it may be inferred, by pores of such minuteness as to escape detection under the highest power of the microscope. In a solitary instance, on removing an abortive egg which had been twenty-one days under a hen, on breaking the shell I found it's inner membrane covered in part with mould (*Mucor mucedo*) the spores of which must have entered, it may be presumed, through the *foramina* in the crust and the insensible pores just referred to.

In accordance with the necessity for the aëration of the embryo and foetus in process of development, the egg of every kind of bird has an air-cell formed by the separation of the two layers of the internal lining membrane, and this at the end which is generally largest, and first presents in the act of being laid. The contained air, in every instance that I have examined it, has been found to differ but little from atmospheric air; and this whatever the age of the egg

“British Birds and their Eggs.” London, 1842.—The Martin (*Hirundo urtica*), from 12 to 18 days; Swift (*H. apus*), 16 to 17; Eagle-owl (*Bubo maximus*), 21; Goshawk (*Astur palumbarius*), 21; Sparrow-hawk (*Accipiter fringillarius*), 21; Stockdove (*Columbo oenas*) 17; Turtledove (*C. turtur*), 16 to 17; Pheasant (*Phasianus colchicus*), 24 to 26; Cock-of-the-wood (*Tetrao urogallus*), 28; Black Grouse (*T. tetrix*), 21; Partridge (*T. perdix*), 21; Swan (*Cygnus olor*), 85 to 42; Wild Duck (*Anas boschas*), 29. The variation as to time, as in the well-known instance of the common fowl, depends probably on the temperature of the atmosphere and the quality of the parent bird, whether a good or bad sitter.

or the time of its incubation,—another proof of the great porosity of the shell and its membranes.

Without any exception yet known, the eggs of all birds consist of albumen and yolk. These in the egg of the common fowl exhibit different and opposite reactions, and different electrical states,—the albumen being alkaline, the yolk acid; and in the instances of the eggs of the smaller birds, I have, as regards the reaction, observed the same; I have had no opportunity to test their electrical condition; but judging from analogy, that also may be inferred to be similar.

I shall now briefly advert to some points of difference. I need hardly notice that of form, and that each kind has commonly a specific shape as well as size, and that though one pole is commonly bigger than the other, the one, as already mentioned, which, in the act of laying, is first protruded, and which contains the air-vesicle, is never, that I am aware of, smaller than its opposite, seemingly a happy provision, securing the successful propulsion of the egg without delay under the influence of an adequate impelling power: did the smaller end first present, it might act as a wedge, and the processes of distension and of laying might be protracted, and the projecting pole in danger.

As is well known, the colour of eggs varies with the species. White may be mentioned as the prevailing colour; next, blue, or bluish-green of different shades; next, brown of different shades, passing into red. Even of the same species, the colour, as of the eggs of the common fowl, is not precisely the same—some being distinguished for their whiteness, as those of the dorking and game-fowl; some for their brown hue, as those of Cochinchina variety. In lustre and polish, too, the several kinds exhibit differences—some having a lustre approaching almost that of the pearl, as the egg of the common pigeon; others being destitute of lustre, giving them a dull appearance. The markings, spots, and partial colouring are so various as to almost baffle description, as any one may satisfy himself by referring to any work in which they are depicted.

These diverse markings—these different colours of eggs—from such experiments as I have made, I am induced to

attribute, not to any mineral colouring matter, but to an organic or animal colouring matter, and that, in part at least, connected with molecular arrangement, analogous to what is witnessed in flowers. As examples, I may give the results of the examination I have made of the dark-brown, almost black, colouring matter of the egg of the common grouse, and of the green of the egg of the starling, and of the green with dark-brown spots, these, too, almost black, of the common thrush. Of all these, the colouring matter has been destroyed before the blowpipe, the shell being rendered perfectly white. In the instance of the thrush's, it is remarkable that, after incandescence, in cooling it recovers in a slight degree its greenish hue, and yet loses it when quite cold; a circumstance this favourable to the idea of the colour, as before remarked, depending in part on molecular arrangement. The green of neither of the eggs is altered by the weak acids, nor by solutions of the alkalies. In strong nitric acid it changes to yellow. In strong muriatic acid it resists change for many hours, fading and disappearing under the continued action of the acid. Chlorine has a similar effect; and bright sunshine, after many days' exposure, renders it fainter: sulphurous acid does not bleach it. On the colour of the grouse's egg strong muriatic acid has no perceptible effect; strong nitric acid changes it to a light dirty yellow. That the colour of this egg is not in any way owing to the presence of iron seemed to be indicated by the following experiment:—A minute portion of blood was applied to the shell; on exposure to the blowpipe, the spot was marked by a slight ochrey stain, the cloured portions becoming, after the destruction of the animal matter, quite white.

The contents, the albumen and yolk, of the several kinds of eggs, though so very similar, are found, even on slight examination, to have points of difference. I shall now restrict myself to three,—viz., the proportional weight of the albumen and yolk, including the shell and its membranes; the density of the two, or rather the proportion of solid matter which each afforded after the dissipation of their aqueous portion by thorough drying; and the effects of heat and its degree in producing the coagulation of the

albumen. I shall give the results, merely premising that in each instance the yolk was separated as much as possible from the white, by rolling it, when that could be done, on a fine linen cloth, stretched; weighing it thus apart, weighing the shell next, after washing it carefully inside and drying it in the air. The weight of the albumen was inferred to be that which was required to complete the weight of the entire egg, previously ascertained. For the sake of comparison, the proportions in the egg of the common fowl, thus examined, may be given:—

1. Egg, newly laid, of a pullet of the *Barn-door fowl*.—Weight 764·5 grs.

			Solid matter, per cent.
Shell and membranes,	78·5 grs., or per cent.,	10·13	
Yolk, ...	192·8 " "	25·21	55·00
Albumen, ...	493·2 " "	64·51	10·19

2. *Jay's egg*.—Weight 127·3 grs.

Shell, &c., ...	6·4 grs., or per cent.,	5·03	
Yolk, ...	34·2 " "	26·86	30·4
Albumen, ...	86·7 " "	68·11	12·8

The liquid albumen, in a thin glass tube, immersed in water at 169° Fahr., falling to 140°, acquired a slight milky appearance, with little diminution of its fluidity. At 184° falling to 140°, a soft coagulation formed, milk-white and tremulous, hardly bearing inversion. At 195° falling to 80°, the coagulum became somewhat firmer, but much less than the albumen of the common fowl at the same temperature, and it was translucent.

3. *Hedge-Sparrow's*.—Weight, 34·5 grs.

			Solid matter per cent.
Shell, &c., 2·0 grs., or per cent.,	5·79		
Yolk, . 7·4 " "	21·45	58·1	
Albumen, 25·9 " "	72·46	12·2	

The albumen at 168° falling to 110°, became pretty firmly coagulated; the coagulum was of a distinct greenish hue; transparency but little impaired, nor was it lessened at 212°.

4. *Golden-crested Wren's*.—Of five from the same nest,

the weight of each was as follows: 14·8 grs., 14·4, 14·3, 14·35, 15·2. That which weighed 14·3 grs. was examined.

			Solid matter per cent.
Shell, &c.,	0·7 grs., or per cent.,	4·90	
Yolk, .	3·44 „ „	24·05	43·6
Albumen,	10·16 „ „	71·05	9·9

One of the eggs was put into water at 200° falling to 140°. On removal of the shell its albumen was found coagulated. The coagulum was soft, semi-transparent, slightly opalescent, and in water had a bluish tinge. The yolk was more firmly coagulated.

5. *Robin's*.—Two were examined; one weighed 38·8 grs., the other 73·8 grs. I believe they were from the same nest.

			Solid matter per cent.
Shell, &c., of the first,	2·1 grs., or per cent.,	5·43	
Yolk, ...	9·4 „ „	24·22	
Albumen, ...	27·3 „ „	70·33	
Shell, &c., of second,	2·0 „ „	2·71	
Yolk, ...	16·3 „ „	22·08	16·5
Albumen, ...	55·5 „ „	75·20	8·2

The albumen of the first at 170° falling to 145°, became of a milky whiteness, with little diminution of fluidity. At 187° falling to 150°, a soft coagulum formed, of the consistence of blanc-mange, and milk-white, nor was it rendered harder by boiling.

6. *Missel-Thrush's*.—Weight of egg, 124·6 grs.

Shell, &c.,	6·4 grs., or per cent.,	5·13
Yolk, .	16·9 „ „	12·84
Albumen,	101·3 „ „	31·30

The albumen at 169° falling to 148°, had its fluidity slightly impaired, acquiring a slight degree of milkiness. At 179° falling to 150°, a soft coagulum was formed, tremulous, with a bluish hue, and barely admitting of inversion. At 188° falling to 160°, it became firmer, remaining transparent, nor was its transparency destroyed by boiling; even then it was softer than the coagulated white of the hen's egg.

Its yolk at 190° yielded a soft coagulum, which was rendered only a little firmer by boiling.

7. *Starling's*.—Three eggs were obtained from the same nest; one weighed 115·1 grs., another 103·6 grs., the third 102·6 grs. The first of these was examined.

Shell, &c.,	8·2 grs., or per cent.,	7·12
Yolk, .	16·4 „ „	14·25
Albumen,	90·5 „ „	78·62

The yolk of the third egg yielded 45·1 per cent. solid matter; the white, 13·5.

The albumen of No. 1 at 165° falling to 135°, formed a soft milk-white coagulum. Another portion of the same white at 160° falling to 100°, became slightly thickened, flowing on inversion.

8. *Pigeon's egg*.—Weight, 278 grs.

			Solid matter per cent.
Shell, &c.,	22·4 grs., or per cent.,	8·05	
Yolk, .	50·0 „ „	17·94	39·3
Albumen,	205·6 „ „	73·95	11·5

The albumen at 158° falling to 130°, formed a soft coagulum, of a light bluish hue by reflected light, of a wine yellow by transmitted light. At 162° falling to 125°, the consistence of the coagulum was but little increased, it hardly bore inversion without flowing. At 189° falling to 140°, it became much firmer. At 212° it became tougher, scarcely harder, was translucent, and of a bluish hue.

By boiling for an hour the coagulum became transparent, and of a rich brown colour; a similar change is produced in the albumen of the egg of the common fowl, but hardly so strongly marked. On the albumen of the eggs of other birds I have not tried the effects of continued boiling; from analogy it may be inferred that it will be similar.

It may here be worthy of remark that the albumen of all the eggs I have experimented upon, when evaporated to dryness at a temperature below the coagulating point, or allowed to dry slowly in the open air, has always exhibited a symmetrical appearance, not unlike that of the beautiful microscopical object the *Arachneidiscus Ehrenbergii*, this organic-like arrangement evidently depending on the fissures formed from contraction of the solid parts with the loss of its aqueous portion.

The following is a tabular view of the results :—

	Weight	Shells, &c., per cent.	Yolk, per cent.	White, per cent.	Yolk Solid, per cent.	White, per cent.	Point of Coagulation.	Consistence.	Colour, &c.
Common Fowl's } Egg	764.5	10.13	25.21	64.5	55.0	10.19	—160°	Hard.	White.
Pigeon's }	278.0	8.05	17.94	78.95	39.3	11.5	—189°	Pretty firm.	Bluish.
Jay's	127.3	5.08	26.66	68.11	30.0	12.8	—184°	Soft.	{ White, transluc- cent.
Mistle Thrush's }	124.6	5.53	12.84	81.30	24.3	10.2	—188°	Soft.	Transparent.
Starling's }	115.	7.12	14.25	78.62	...	...	—195°	Firm.	White.
Robin's }	73.8	2.71	22.08	55.50	16.5	8.2	—187°	Soft.	White.
	38.8	5.43	24.22	70.33	...	...			
Hedge Sparrow's }	24.5	5.79	21.45	72.46	56.1	12.2	—168°	Pretty firm.	{ Greenish, trans- parent.
Golden-crested } Wren's	14.3	4.90	24.05	43.6	43.6	9.9		Soft.	{ Bluish, semi- transparent.

The differences, as shown by these results, are not great. So far as they allow of inference, they seem to show—1. (as already remarked), That the smaller the egg, the smaller the bird, the shorter the period of incubation, the thinner is the shell, and the more elastic and the smaller the proportion of calcareous incrustation, and in consequence the more pervious to the air, essential to life and development ; though even as to this there seems to be no exact ratio, and there appear to be exceptions.*

2. That the albumen in quantity greatly exceeds the yolk, but in no regular proportion ; whilst the quantity of solid matter in the yolk is proportionally much larger per cent. than in the albumen.

3. That the temperature at which coagulation takes place varies in almost every instance ; and that the firmness of the coagulum does not appear to be regulated by the proportion of solid matter obtained by evaporation.

4. That the coagulum of each has an aspect of its own, varying in the different instances as to kind and degrees of translucency.

Though all the experiments were made with care, I must

* Probably the size of the egg, the quantity of nutritive matter it contains (as suggested in my "Physiological Researches," p. 159), bears some relation to the condition of the young bird on leaving the egg, and its habits, especially as to feeding, i.e., whether it has to find its food after the manner of the offspring of the *Natatores* and *Rasores*, immediately on starting into active life, or is provided with food till fully formed and capable of taking wing, by the parents, after the manner of the *Columbæ*, *Passores*, and *Raptores*.

remark, in conclusion, that I can offer their results only as approximations, the sources of error being so many, and of these not the least, the difficulty of ascertaining how long the eggs had been laid. All that I examined, however, appeared to be fresh; none of them bore marks of having been sat on, judging from the total absence of the vascular membrane.*

Description of the Fruit and Seed of Clerodendron Thomsonæ. By Professor BALFOUR. (Plate V.)†

In the Transactions of the Society for 1862, I described a Verbenaceous plant which had been transmitted from Old Calabar by the Rev. W. C. Thomson, and which I named *Clerodendron Thomsonæ*. At that time the plant had flowered (fig. 2) in the Botanic Garden, but it had not perfected fruit. It has now ripened its seeds, and they have germinated in the hot-houses (fig. 1). I therefore complete my description of the species by giving a few details as to the fructification. The style in the young state is terminal, and the four achenes are concrete, so as to appear as a syncarpous tetracarpellary ovary (fig. 4). As the carpels advance in growth they separate, and the style falls off. The young fruit then appears as four green-coloured achenes, partially united and surrounded by the large persistent calyx, which has changed from a white to a pinkish hue (fig. 3). On cutting across the four achenes at their junction, we observe on the inner surfaces a bright red cellular coat (fig. 7). When ripe, the achenes assume a shining black colour externally, and the red cellular covering becomes much enlarged, and by its growth separates the achenes completely, which ultimately appear as four distinct seed-vessels, covered on their upper surface (which

* Those eggs in which this membrane appeared were laid aside. One of these was the Corncrail's; in it the vascular area had just become distinct; its vessels contained florid blood, the corpuscles in which were of an unusual magnitude, some elliptical, some circular without a nucleus; the elliptical of largest size were about $\frac{1}{16}$ inch in diameter by $\frac{1}{32}$.

† Read before the Botanical Society of Edinburgh, 9th July 1863.

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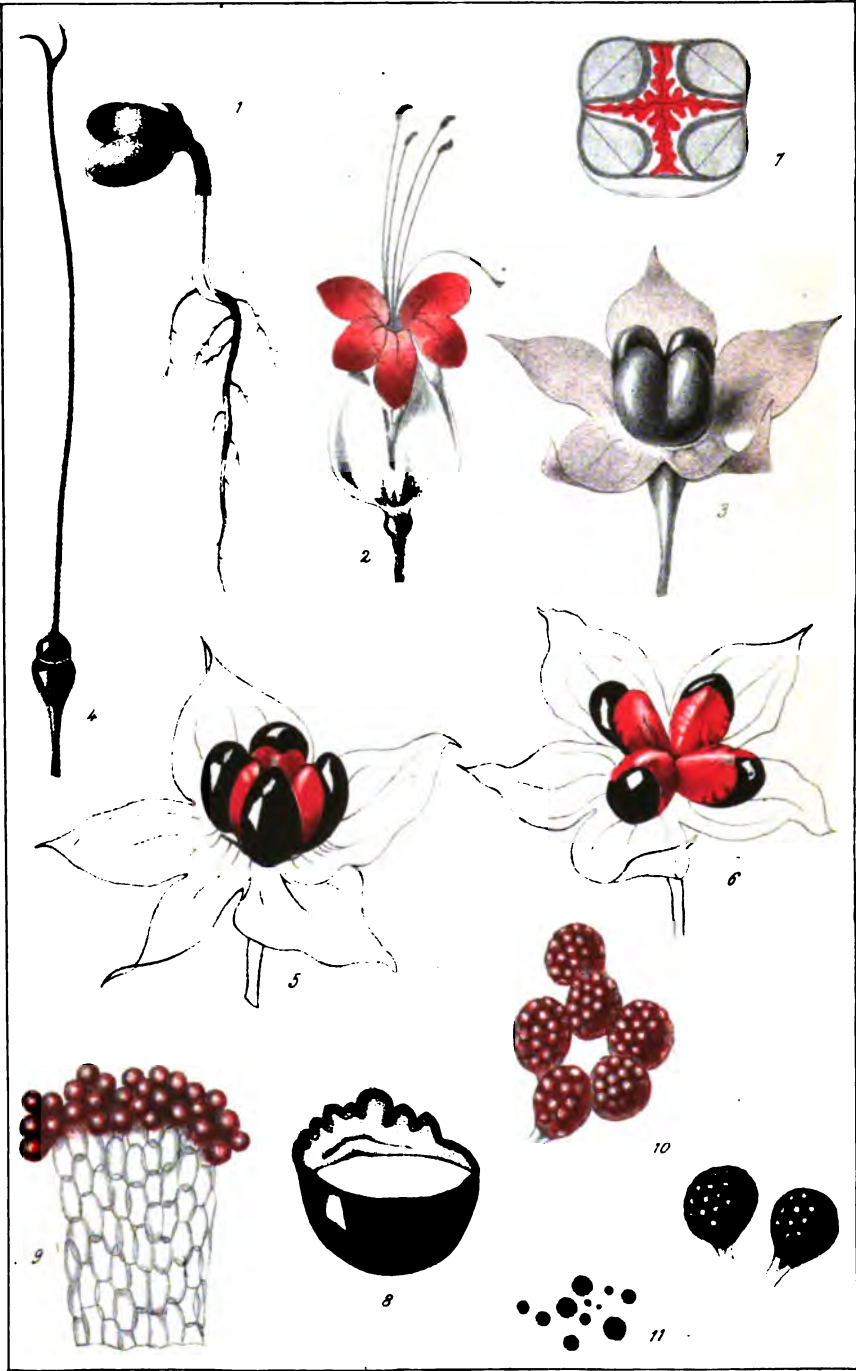
2

1.

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2



Fruit and Seed of *Clerodendron Thomsonae* (Balf.)

W H M^r Farlane, Lith^r Edin^r

corresponds to their commissure) with a succulent mass of cells of a bright scarlet colour (fig. 5). This mass covers two-thirds of the surface of each achene, and finally assumes an orange colour. The mass exhibits grooves and elevations on its surface, and has a rugose appearance, not unlike the convolutions of the brain (fig. 8). On examining this cellular coat, we find that the scarlet colour is produced by globules apparently of an oily nature, filling the interior of the external cells, and that the cells below are deficient in colour, being clear and transparent (fig. 9). This scarlet succulent mass, increasing very rapidly in the axis, separates the achenes from each other, so that they finally spread out in a cruciate manner, being united only at their bases (fig. 6). The cells of the succulent part of the pericarp are beautiful objects under the microscope, and they are represented in fig. 10. The rapid development of this cellular coat seems almost akin to that in some Fungi, *Bovista* for example, which is one of the marvels of vegetative power. The scarlet colour stains paper beyond remedy; nothing but scratching removes it. The oil-globule-bearing cells look like glands. The minute globules of oil (fig. 11), when pressed out of the cells in water, exhibit molecular movements. When the cells are put into water, the oil globules run together, so as to form a homogeneous mass. The beautiful scarlet covering of the achenes renders the plant scarcely less showy in fruit than it was in flower. (Specimens of the pistil and fruit in various states were exhibited.)

EXPLANATION OF PLATE V.

- Fig. 1. Seed of *Clerodendron Thomsonæ* germinating, showing epigeal cotyledons and radicle.
- Fig. 2. Flower with white-coloured calyx, scarlet corolla, and didynamous stamens.
- Fig. 3. Large calyx surrounding the young fruit, separated into four carpels. The style has fallen off. The calyx is at first white, and finally assumes a pinkish hue.
- Fig. 4. Young pistil, showing terminal style with the four united carpels.
- Fig. 5. Four achenes, with black shining outer coat, and succulent inner covering of a scarlet colour.
- Fig. 6. Four achenes still more separated, apparently by the rapid growth of the scarlet cellular mass at the commissure, which now becomes the upper surface. The achenes arranged in a cruciate manner.

- Fig. 7. Young ovary cut across, showing four ovules and scarlet coat in its early development.
 Fig. 8. One of the ripe achenes cut transversely, showing the dark episperm on the outside, and the succulent scarlet cellular mass on the inside.
 Fig. 9. Coloured cells of the commissure with oil globules, surmounting other cells which are colourless.
 Fig. 10. Cells containing scarlet or orange oil-like globules.
 Fig. 11. Oil-like globules shown separately.

Proposed Reform of Zoological Nomenclature.

Reform of the Nomenclature of Zoology was a subject which occupied much of the time of the late Hugh E. Strickland.* It was his object that this reform should be brought forward under the auspices of the British Association, and at a meeting of the Council of that body, held in London upon 11th February 1842, it was resolved—"That with a view of securing attention to the following important subject, a committee, consisting of Mr C. Darwin, Professor Henslow, Rev. L. Jenyns, Mr W. Ogilby, Mr J. Phillips, Dr Richardson, Mr H. E. Strickland (reporter), Mr J. O. Westwood, be appointed, to consider of the rules by which the nomenclature of zoology may be established on a uniform and permanent basis; the report to be presented to the Zoological Section, and submitted to its committee at the Manchester meeting."†

This committee met at various times in London, and the following gentlemen were added to it, and assisted in its labours: W. J. Broderip, Professor Owen, W. E. Shuckard, G. R. Waterhouse, and W. Yarrell. An outline of the proposed code of rules was drawn up and circulated, and many valuable suggestions were received from eminent zoologists at home and abroad. The "plan" was farther considered by the committee during the meeting at Manchester, "and the committee having thus given their best endeavours to maturing the plan, beg now to submit it to the approval of the British Association under the title of—'Series of Propositions for rendering the Nomenclature of Zoology uniform and permanent.' "‡

The propositions were printed in the Reports of the British Association, and a grant of money was voted to print copies for

* See Memoirs of Hugh Edwin Strickland, by Sir W. Jardine, Bart., p. clxxv.

† Report of Twelfth Meeting of British Association held at Manchester, June 1842, p. 106.

‡ Report of Twelfth Meeting, 1842, p. 106.

circulation. The rules thus laid down were very generally adopted by zoologists, both in this country and abroad; but having been only printed in the volumes of the British Association, "*Annals of Natural History*," and "*Philosophical Magazine*,"* or depending on private circulation only, it was deemed advisable that greater publicity should be given to them, and at the meeting at Oxford in 1860 it was resolved, that "The surviving members of the committee appointed in 1842—viz., Mr C. Darwin, Rev. Professor Henslow, Rev. L. Jenyns, Mr W. Ogilby, Professor Phillips, Sir John Richardson, Mr J. O. Westwood, Professor Owen, Mr W. E. Shuckard, and Mr G. Waterhouse—for the purpose of preparing rules for the establishment of a uniform zoological nomenclature, be re-appointed, with Sir W. Jardine, Bart., and Mr P. L. Sclater. That Sir W. Jardine be the Secretary, and that the sum of L.10 be placed at their disposal for the purpose of revising and reprinting the rules."†

From the difficulty of bringing such a committee together, nothing was done since the time of its appointment; but the resolution and the grant of money were again renewed at the late meeting in Newcastle, as follows:—"That Sir W. Jardine, A. R. Wallace, J. E. Gray, C. C. Babington, Dr Francis, P. L. Sclater, C. Spence Bate, P. P. Carpenter, Dr J. D. Hooker, Professor Balfour, H. T. Stainton, J. Gwyn Jeffreys, A. Newton, Professor T. H. Huxley, Professor Allman, and Mr Bentham, be a committee, with power to add to their number, to report on the changes which they may consider it desirable to make, if any, in the rules of nomenclature drawn up at the instance of the Association by Mr Strickland and others, with power to reprint these rules, and to correspond with foreign naturalists and others on the best means of insuring their general adoption.—L.15."

Accordingly the rules, as originally circulated, are now reprinted, and zoologists are requested to examine them carefully, and to communicate any suggestions for alteration or improvement, on or before 1st June 1864, to *Sir William Jardine, Bart., Jardine Hall, by Lockerby, N.B.*, who will consult with the members of the committee, and report upon the subject at the next meeting of the British Association appointed to be held at Bath.

Jardine Hall, 8th Sept. 1863.

* At the Scientific Congress held in 1843 at Padua, the late Prince C. L. Buonaparte submitted to the meeting an Italian translation of the "*British Association's Code of Rules*," which was generally approved of. A French translation of the report appeared in the scientific journal "*L'Institut*," in which paper much stress was laid on the importance of the measure. A review of it was also printed in the "*American Journal of Science*."

† Reports of the British Association held at Oxford, 1860, p. xlvii.

Series of Propositions for rendering the Nomenclature of Zoology uniform and permanent.

[Reprinted from the Report of the British Association for 1842.]

PREFACE.

All persons who are conversant with the present state of Zoology must be aware of the great detriment which the science sustains from the vagueness and uncertainty of its nomenclature. We do not here refer to those diversities of language which arise from the various methods of classification adopted by different authors, and which are unavoidable in the present state of our knowledge. So long as naturalists differ in the views which they are disposed to take of the natural affinities of animals there will always be diversities of classification, and the only way to arrive at the true system of nature is to allow perfect liberty to systematists in this respect. But the evil complained of is of a different character. It consists in this, that when naturalists are agreed as to the characters and limits of an individual group or species, they still disagree in the appellations by which they distinguish it. A genus is often designated by three or four, and a species by twice that number of precisely equivalent synonyms; and in the absence of any rule on the subject, the naturalist is wholly at a loss what nomenclature to adopt. The consequence is, that the so-called commonwealth of science is becoming daily divided into independent states, kept asunder by diversities of language as well as by geographical limits. If an English zoologist, for example, visits the museums and converses with the professors of France, he finds that their *scientific* language is almost as foreign to him as their *vernacular*. Almost every specimen which he examines is labelled by a title which is unknown to him, and he feels that nothing short of a continued residence in that country can make him conversant with her science. If he proceeds thence to Germany or Russia, he is again at a loss; bewildered everywhere amidst the confusion of nomenclature, he returns in despair to his own country and to the museums and books to which he is accustomed.

If these diversities of scientific language were as deeply rooted as the vernacular tongue of each country, it would of course be hopeless to think of remedying them; but happily this is not the case. The language of science is in the mouths of comparatively few, and these few, though scattered over distant lands, are in habits of frequent and friendly intercourse with each other. All that is wanted, then, is, that some plain and simple regulations, founded on justice and sound reason, should be drawn up by a competent body of persons, and then be extensively distributed throughout the zoological world.

The undivided attention of chemists, of astronomers, of anatomo-

mists, of mineralogists, has been of late years devoted to fixing their respective languages on a sound basis. Why, then, do zoologists hesitate in performing the same duty? at a time, too, when all acknowledge the evils of the present anarchical state of their science.

It is needless to inquire far into the causes of the present confusion of zoological nomenclature. It is in great measure the result of the same branch of science having been followed in distant countries by persons who were either unavoidably ignorant of each other's labours, or who neglected to inform themselves sufficiently of the state of the science in other regions. And when we remark the great obstacles which now exist to the circulation of books beyond the conventional limits of the states in which they happen to be published, it must be admitted that this ignorance of the writings of others, however unfortunate, is yet in great measure pardonable. But there is another source for this evil, which is far less excusable,—the practice of gratifying individual vanity by attempting, on the most frivolous pretexts to cancel the terms established by original discoverers, and to substitute a new and unauthorised nomenclature in their place. One author lays down as a rule, that no specific names should be derived from geographical sources, and unhesitatingly proceeds to insert words of his own in all such cases; another declares war against names of exotic origin, foreign to the Greek and Latin; a third excommunicates all words which exceed a certain number of syllables; a fourth cancels all names which are complimentary of individuals, and so on, till universality and permanence, the two great essentials of scientific language, are utterly destroyed.

It is surely, then, an object well worthy the attention of the Zoological Section of the British Association for the Advancement of Science to devise some means which may lessen the extent of this evil, if not wholly put an end to it. The best method of making the attempt seems to be, to entrust to a carefully selected committee the preparation of a series of rules, the adoption of which must be left to the sound sense of naturalists in general. By emanating from the British Association, it is hoped that the proposed rules will be invested with an authority which no individual zoologist, however eminent, could confer on them. The world of Science is no longer a monarchy, obedient to the ordinances, however just, of an Aristotle or a Linnæus. She has now assumed the form of a republic, and although this revolution may have increased the vigour and zeal of her followers, yet it has destroyed much of her former order and regularity of government. The latter can only be restored by framing such laws as shall be based in reason, and sanctioned by the approval of men of science; and it is to the preparation of these laws that the Zoological Section of the Association have been invited to give their aid.

In venturing to propose these rules for the guidance of all classes of zoologists in all countries, we disclaim any intention of dictating to men of science the course which they may see fit to pursue. It must of course be always at the option of authors to adhere to or depart from these principles; but we offer them to the candid consideration of zoologists, in the hope that they may lead to sufficient uniformity of method in future to rescue the science from becoming a mere chaos of words.

We now proceed to develop the details of our plan; and in order to make the reasons by which we are guided apparent to naturalists at large, it will be requisite to append to each proposition a short explanation of the circumstances which call for it.

Among the numerous rules for nomenclature which have been proposed by naturalists, there are many which, though excellent in themselves, it is not now desirable to enforce.* The cases in which those rules have been overlooked or departed from are so numerous and of such long standing, that to carry these regulations into effect would undermine the edifice of zoological nomenclature. But while we do not adopt these propositions as authoritative laws, they may still be consulted with advantage in making such additions to the language of zoology as are required by the progress of the science. By adhering to sound principles of philology we may avoid errors in future, even when it is too late to remedy the past, and the language of science will thus eventually assume an aspect of more classic purity than it now presents.

Our subject hence divides itself into two parts; the first consisting of *Rules* for the rectification of the present zoological nomenclature, and the second of *Recommendations* for the improvement of zoological nomenclature in future.

PART I.

RULES FOR RECTIFYING THE PRESENT NOMENCLATURE.

[*Limitation of the Plan to Systematic Nomenclature.*]

In proposing a measure for the establishment of a permanent and universal zoological nomenclature, it must be premised that we refer solely to the Latin or systematic language of zoology. We have nothing to do with vernacular appellations. One great cause of the neglect and corruption which prevails in the scientific nomenclature of zoology, has been the frequent and often exclusive use of vernacular names in lieu of the Latin binomial designa-

* See especially the admirable code proposed in the "*Philosophia Botanica*" of Linnæus. If zoologists had paid more attention to the principles of that code, the present attempt at reform would perhaps have been unnecessary.

tions, which form the only legitimate language of systematic zoology. Let us then endeavour to render perfect the Latin or Linnæan method of nomenclature, which, being far removed from the scope of national vanities and modern antipathies, holds out the only hope of introducing into zoology that grand desideratum, an universal language.

[Law of Priority the only effectual and just one.]

It being admitted on all hands that words are only the conventional signs of ideas, it is evident that language can only attain its end effectually by being permanently established and generally recognised. This consideration ought, it would seem, to have checked those who are continually attempting to subvert the established language of zoology by substituting terms of their own coinage. But, forgetting the true nature of language, they persist in confounding the *name* of a species or group with its *definition*; and because the former often falls short of the fulness of expression found in the latter, they cancel it without hesitation, and introduce some new term which appears to them more characteristic, but which is utterly unknown to the science, and is therefore devoid of all authority.* If these persons were to object to such names of men as *Long*, *Little*, *Armstrong*, *Golightly*, &c., in cases where they fail to apply to the individuals who bear them, or should complain of the names *Gough*, *Lawrence*, or *Harvey*, that they were devoid of meaning, and should hence propose to change them for more characteristic appellations, they would not act more unphilosophically or inconsiderately than they do in the case before us; for, in truth, it matters not in the least by what conventional sound we agree to designate an individual object, provided the sign to be employed be stamped with such an authority as will suffice to make it pass current. Now in zoology no one person can subsequently claim an authority equal to that possessed by the person who is the first to define a new genus or describe a new species; and hence it is that the name originally given, even though it may be inferior in point of elegance or expressiveness to those subsequently proposed, ought as a general principle to be permanently retained. To this consideration we ought to add the injustice of erasing the name originally selected by the person to whose labours we owe our first knowledge of the object; and we should reflect how much the permission of such a practice opens a door to obscure pretenders for dragging themselves into notice at the expense of original observers. Neither can an author be permitted to alter a name which he himself has once published, except in accordance with fixed and equitable laws.

* Linnæus says on this subject, "Abstinendum ab hac innovatione quæ nunquam cessaret, quin indies aptiora detegerentur ad infinitum."



It is well observed by Decandolle, "L'auteur même qui a le premier établi un nom n'a pas plus qu'un autre le droit de le changer pour simple cause d'impropriété. La priorité en effet est un terme fixe, positif, qui n'admet rien, ni d'arbitraire, ni de partial."

For these reasons, we have no hesitation in adopting as our fundamental maxim, the "law of priority," viz.,

§ 1. The name originally given by the founder of a group or the describer of a species should be permanently retained, to the exclusion of all subsequent synonyms (with the exceptions about to be noticed.)

Having laid down this principle, we must next inquire into the limitations which are found necessary in carrying it into practice.

[*Not to extend to authors older than Linnæus.*]

As our subject matter is strictly confined to the *binomial system of nomenclature*, or that which indicates species by means of two Latin words, the one generic, the other specific, and as this invaluable method originated solely with Linnæus, it is clear that, as far as species are concerned, we ought not to attempt to carry back the principle of priority beyond the date of the 12th edition of the "*Systema Naturæ*." Previous to that period, naturalists were wont to indicate species not by a *name* comprised in one word, but by a *definition* which occupied a sentence, the extreme verbosity of which method was productive of great inconvenience. It is true that one word sometimes sufficed for the definition of a species, but these rare cases were only binomial by accident and not by principle, and ought not therefore in any instance to supersede the binomial designations imposed by Linnæus.

The same reasons apply also to generic names. Linnæus was the first to attach a definite value to genera, and to give them a systematic character by means of exact definitions; and therefore, although the *names* used by previous authors may often be applied with propriety to modern genera, yet in such cases they acquire a new meaning, and should be quoted on the authority of the first person who used them in this secondary sense. It is true, that several of the old authors made occasional approaches to the Linnæan exactness of generic definition, but still these were but partial attempts; and it is certain that if in our rectification of the binomial nomenclature we once trace back our authorities into the obscurity which preceded the epoch of its foundation, we shall find no resting-place or fixed boundary for our researches. The nomenclature of Ray is chiefly derived from that of Gesner and Aldrovandus, and from these authors we might proceed backward

to Ælian, Pliny, and Aristotle, till our zoological studies would be frittered away amid the refinements of classical learning.*

We therefore recommend the adoption of the following proposition:—

§ 2. The binomial nomenclature having originated with Linnæus, the law of priority, in respect of that nomenclature, is not to extend to the writings of antecedent authors.

[It should be here explained, that Brisson, who was a contemporary of Linnæus and acquainted with the ‘*Systema Naturæ*,’ defined and published certain genera of birds which are *additional* to those in the twelfth edition of Linnæus’s works, and which are therefore of perfectly good authority. But Brisson still adhered to the old mode of designating species by a sentence instead of a word, and therefore while we retain his defined genera, we do not extend the same indulgence to the titles of his species, even when the latter are accidentally binomial in form. For instance, the *Perdix rubra* of Brisson is the *Tetrao rufus* of Linnæus; therefore as we in this case retain the generic name of Brisson and the specific name of Linnæus, the correct title of the species would be *Perdix rufa*.]

[*Generic names not to be cancelled in subsequent subdivisions.*]

As the number of known species which form the groundwork of zoological science is always increasing, and our knowledge of their structure becomes more complete, fresh generalisations continually occur to the naturalist, and the number of genera and other groups requiring appellations is ever becoming more extensive. It thus becomes necessary to subdivide the contents of old groups, and to make their definitions continually more restricted. In carrying out this process, it is an act of justice to the original author that his generic name should never be lost sight of; and it is no less essential to the welfare of the science, that all which is sound in its nomenclature should remain unaltered amid the additions which are continually being made to it. On this ground we recommend the adoption of the following rule:—

§ 3. A generic name, when once established, should never be cancelled in any subsequent subdivision of the group, but retained in a restricted sense for one of the constituent portions.

[*Generic names to be retained for the typical portion of the old genus.*]

When a genus is subdivided into other genera, the original

* “*Quis longo ævo recepta vocabula commutaret hodie cum patrum?*”—*Linnæus*.

name should be retained for that portion of it which exhibits in the greatest degree its essential characters as at first defined. Authors frequently indicate this by selecting some one species as a fixed point of reference, which they term the "type of the genus." When they omit doing so, it may still in many cases be correctly inferred that the *first* species mentioned on their list, if found accurately to agree with their definition, was regarded by them as the type. A specific name, or its synonyms, will also often serve to point out the particular species which by implication must be regarded as the original type of a genus. In such cases we are justified in restoring the name of the old genus to its typical signification, even when later authors have done otherwise. We submit therefore that

§ 4. The generic name should always be retained for that portion of the original genus which was considered typical by the author.

Example.—The genus *Picumnus* was established by Temminck, and included two groups, one with four toes, the other with three, the *former* of which was regarded by the author as typical. Swainson, however, in raising these groups at a later period to the rank of genera, gave a new name, *Asthenurus*, to the *former* group, and retained *Picumnus* for the *latter*. In this case we have no choice but to restore the name *Picumnus*, Temm., to its correct sense, cancelling the name *Asthenurus*, Sw., and imposing a new name on the 3-toed group which Swainson had called *Picumnus*.

[When no type is indicated, then the original name is to be kept for that subsequent subdivision which first received it.]

Our next proposition seems to require no explanation:—

§ 5. When the evidence as to the original type of a genus is not perfectly clear and indisputable, then the person who first subdivides the genus may affix the original name to any portion of it at his discretion, and no later author has a right to transfer that name to any other part of the original genus.

[A later name of the same extent as an earlier to be wholly cancelled.]

When an author infringes the law of priority by giving a new name to a genus which has been properly defined and named already, the only penalty which can be attached to this act of negligence or injustice, is to expel the name so introduced from the pale of the science. It is not right, then, in such cases to

restrict the meaning of the later name so that it may stand side by side with the earlier one, as has sometimes been done. For instance, the genus *Monaulus*, Vieill. 1816, is a precise equivalent to *Lophophorus*, Temm. 1813, both authors having adopted the same species as their type, and therefore, when the latter genus came in the course of time to be divided into two, it was incorrect to give the condemned name *Monaulus* to one of the portions. To state this succinctly,

§ 6. When two authors define and name the same genus, both making it exactly of the same extent, the later name should be cancelled *in toto*, and not retained in a modified sense.*

This rule admits of the following exception :—

§ 7. Provided however, that if these authors select their respective types from different sections of the genus, and these sections be afterwards raised into genera, then both these names may be retained in a restricted sense for the new genera respectively.

Example—The names *Cedemia* and *Melanetta* were originally co-extensive synonyms, but their respective types were taken from different sections which are now raised into genera, distinguished by the above titles.

[No special rule is required for the cases in which the later of two generic names is so defined as to be *less extensive* in signification than the earlier, for if the later includes the type of the earlier genus, it would be cancelled by the operation of § 4 ; and if it does not include that type, it is in fact a distinct genus.]

But when the later name is *more extensive* than the earlier, the following rule comes into operation :—

[A later name equivalent to several earlier ones is to be cancelled.]

The same principle which is involved in § 6 will apply to § 8.

§ 8. If the later name be so defined as to be equal in extent to two or more previously published genera, it must be cancelled *in toto*.

Example.—*Psarocolius*, Wagl. 1827, is equivalent to five or six genera previously published under other names, therefore *Psarocolius* should be cancelled.

If these previously published genera be *separately adopted* (as

* These discarded names may, however, be *tolerated*, if they have been afterwards proposed in a totally new sense, though we trust that in future no one will *knowingly* apply an old name, whether now adopted or not, to a new genus. (See proposition *q. infra*.)

is the case with the equivalents of *Psarocolius*), their original names will of course prevail; but if we follow the later author in combining them into one, the following rule is necessary:—

[*A genus compounded of two or more previously proposed genera whose characters are now deemed insufficient, should retain the name of one of them.*]

It sometimes happens that the progress of science requires two or more genera, founded on insufficient or erroneous characters, to be combined together into one. In such cases the law of priority forbids us to cancel *all* the original names and impose a *new* one on this compound genus. We must therefore select some one species as a type or example, and give the generic name which it formerly bore to the whole group now formed. If these original generic names differ in date, the oldest one should be the one adopted.

§ 9. In compounding a genus out of several smaller ones, the earliest of them, if otherwise unobjectionable, should be selected, and its former generic name be extended over the new genus so compounded.

Example.—The genera *Accentor* and *Prunella* of Vieillot not being considered sufficiently distinct in character, are now united under the general name of *Accentor*, that being the earliest. So also *Cerithium* and *Potamides*, which were long considered distinct, are now united, and the latter name merges into the former.

We now proceed to point out those few cases which form exceptions to the law of priority, and in which it becomes both justifiable and necessary to alter the names originally imposed by authors.

[*A name should be changed when previously applied to another group which still retains it.*]

It being essential to the binomial method to indicate objects in natural history by means of *two words* only, without the aid of any further designation, it follows that a generic name should only have one meaning,—in other words, that two genera should never bear the same name. For a similar reason, no two species in the same genus should bear the same name. When these cases occur, the later of the two duplicate names should be cancelled, and a new term, or the earliest synonym, if there be any, substituted. When it is necessary to form new words for this purpose, it is desirable to make them bear some analogy to those which they are destined to supersede, as where the genus of birds

Plectorhynchus, being preoccupied in Ichthyology, is changed to *Plectorhamphus*. It is, we conceive, the bounden duty of an author, when naming a new genus, to ascertain by careful search that the name which he proposes to employ has not been previously adopted in other departments of natural history.* By neglecting this precaution he is liable to have the name altered and his authority superseded by the first subsequent author who may detect the oversight, and for this result, however unfortunate, we fear there is no remedy, though such cases would be less frequent if the detectors of these errors would, as an act of courtesy, point them out to the author himself, if living, and leave it to him to correct his own inadvertencies. This occasional hardship appears to us to be a less evil than to permit the practice of giving the same generic name *ad libitum* to a multiplicity of genera. We submit, therefore, that

§ 10. A name should be changed which has before been proposed for some other genus in zoology or botany, or for some other species in the same genus, when still retained for such genus or species.

[*A name whose meaning is glaringly false may be changed.*]

Our next proposition has no other claim for adoption than that of being a concession to human infirmity. If such proper names of places as Covent Garden, Lincoln's Inn Fields, Newcastle, Bridgewater, &c., no longer suggest the ideas of gardens, fields, castles, or bridges, but refer the mind with the quickness of thought to the particular localities which they respectively designate, there seems no reason why the proper names used in natural history should not equally perform the office of correct indication, even when their etymological meaning may be wholly inapplicable to the object which they typify. But we must remember that the language of science has but a limited currency, and hence the words which compose it do not circulate with the same freedom and rapidity as those which belong to every-day life. The attention is consequently liable in scientific studies to be diverted from the contemplation of the thing signified to the etymological meaning of the sign, and hence it is necessary to provide that the latter shall not be such as to propagate actual error. Instances of this kind are indeed very rare, and in some cases, such as that of *Monodon*, *Caprimulgus*, *Paradisea apoda* and *Monoculus*, they have acquired sufficient currency no longer to cause error, and are therefore retained without change. But when we find a Batrachian reptile named in violation of its true affinities *Mastodon saurus*, a

* This laborious and difficult research will in future be greatly facilitated by the very useful work of M. Agassiz, entitled "Nomenclator Zoologicus."

Mexican species termed (through erroneous information of its habitat) *Picus cafer*, or an olive-coloured one *Muscicapa atra*, or when a name is derived from an accidental monstrosity, as in *Picus semirostris* of Linnæus, and *Helix disjuncta* of Turton, we feel justified in cancelling these names, and adopting that synonym which stands next in point of date. At the same time we think it right to remark, that this privilege is very liable to abuse, and ought therefore to be applied only to extreme cases and with great caution. With these limitations we may concede that

§ 11. A name may be changed when it implies a false proposition which is likely to propagate important errors.

[*Names not clearly defined may be changed.*]

Unless a species or group is intelligibly defined when the name is given, it cannot be recognised by others, and the signification of the name is consequently lost. Two things are necessary before a zoological term can acquire any authority, viz., *definition* and *publication*. Definition properly implies a distinct exposition of essential characters, and in all cases we conceive this to be indispensable, although some authors maintain that a mere enumeration of the component species, or even of a single type, is sufficient to authenticate a genus. To constitute *publication*, nothing short of the insertion of the above particulars in a printed book can be held sufficient. Many birds, for instance, in the Paris and other continental museums, shells in the British Museum (in Dr Leach's time), and fossils in the Scarborough and other public collections, have received MS. names which will be of no authority until they are published.* Nor can any unpublished descriptions, however exact (such as those of Forster, which are still shut up in a MS. at Berlin), claim any right of priority till published, and then only from the date of their publication. The same rule applies to cases where groups or species are published, but not defined, as in some museum catalogues, and in Lesson's "*Traité d'Ornithologie*," where many species are enumerated by name, without any description or reference by which they can be identified. Therefore,—

§ 12. A name which has never been clearly defined in some published work should be changed for the earliest name by which the object shall have been so defined.

[*Specific names, when adopted as generic, must be changed.*]

The necessity for the following rule will be best illustrated by

* These MS. names are in all cases liable to create confusion, and it is therefore much to be desired that the practice of using them should be avoided in future.

an example. The *Corvus pyrrhonorax*, Linn., was afterwards advanced to a genus under the name of *Pyrrhonorax*. Temminck adopts this generic name, and also retains the old specific one, so that he terms the species *Pyrrhonorax pyrrhonorax*. The inelegance of this method is so great as to demand a change of the specific name, and the species now stands as *Pyrrhonorax alpinus*, Vieill. We propose, therefore, that

§ 13. A new specific name must be given to a species when its old name has been adopted for a genus which includes that species.

N.B.—It will be seen, however, below, that we strongly object to the further continuance of this practice of elevating specific names into generic.

[*Latin orthography to be adhered to.*]

On the subject of orthography it is necessary to lay down one proposition,—

§ 14. In writing zoological names, the rules of Latin orthography must be adhered to.

In Latinizing Greek words there are certain rules of orthography known to classical scholars which must never be departed from. For instance, the names which modern authors have written *Aipunemia*, *Zenophasia*, *poioccephala*, must, according to the laws of etymology, be spelt *Æpynemia*, *Xenophasia*, and *pæoccephala*. In Latinizing modern words the rules of classic usage do not apply, and all that we can do is to give to such terms as classical an appearance as we can, consistently with the preservation of their etymology. In the case of European words whose orthography is fixed, it is best to retain the original form, even though it may include letters and combinations unknown in Latin. Such words, for instance, as *Woodwardi*, *Knighti*, *Bullocki*, *Eschscholtzi*, would be quite unintelligible if they were Latinized into *Vudvardi*, *Cnichti*, *Bulloci*, *Essolzi*, &c. But words of barbarous origin, having no fixed orthography, are more pliable, and hence, when adopted into the Latin, they should be rendered as classical in appearance as is consistent with the preservation of their original sound. Thus the words *Tochus*, *awsuree*, *argoondah*, *kundoo*, &c., should, when Latinized, have been written *Toccus*, *ausure*, *argunda*, *cundu*, &c. Such words ought, in all practicable cases, to have a Latin termination given them, especially if they are used generically.

In Latinizing proper names, the simplest rule appears to be to use the termination *-us*, genitive *-i*, when the name ends with a

consonant, as in the above examples; and *-ius*, gen. *-ii*, when it ends with a vowel, as *Latreille*, *Latreillii*, &c.

In converting Greek words into Latin the following rules must be attended to:—

Greek.	Latin.	Greek.	Latin.
α becomes	α .	θ becomes	th.
" "	i.	ϕ "	ph.
σ terminal, us.		χ "	ch.
σ "	um.	ζ "	c.
ω becomes	u.	$\gamma\chi$ "	nch.
α "	α .	$\gamma\gamma$ "	ng.
υ "	y.	" "	h.

When a name has been erroneously written and its orthography has been afterwards amended, we conceive that the authority of the original author should still be retained for the name, and not that of the person who makes the correction.

PART II.

RECOMMENDATIONS FOR IMPROVING THE NOMENCLATURE IN FUTURE.

The above propositions are all which, in the present state of the science, it appears practicable to invest with the character of laws. We have endeavoured to make them as few and simple as possible, in the hope that they may be the more easily comprehended and adopted by naturalists in general. We are aware that a large number of other regulations, some of which are hereafter enumerated, have been proposed and acted upon by various authors who have undertaken the difficult task of legislating on this subject; but as the enforcement of such rules would in many cases undermine the invaluable principle of priority, we do not feel justified in adopting them. At the same time we fully admit that the rules in question are, for the most part, founded on just criticism, and therefore, though we do not allow them to operate retrospectively, we are willing to retain them for future guidance. Although it is of the first importance that the principle of priority should be held paramount to all others, yet we are not blind to the desirableness of rendering our scientific language palatable to the scholar and the man of taste. Many zoological terms, which are now marked with the stamp of perpetual currency, are yet so far defective in construction, that our inability to remove them without infringing the law of priority may be a subject of regret. With these terms we cannot interfere, if we adhere to the principles above laid down; nor is there even any remedy, if authors

insist on infringing the rules of good taste by introducing into the science words of the same inelegant or unclassical character in future. But that which cannot be enforced by law may, in some measure, be effected by persuasion; and with this view we submit the following propositions to naturalists, under the title of *Recommendations for the Improvement of Zoological Nomenclature in future.*

[*The best names are Latin or Greek characteristic words.*]

The classical languages being selected for zoology, and words being more easily remembered in proportion as they are expressive, it is self-evident that

§ A. The *best* zoological names are those which are derived from the Latin or Greek, and express some distinguishing characteristic of the object to which they are applied.

[*Classes of objectionable names.*]

It follows from hence that the following classes of words are more or less objectionable in point of taste, though, in the case of *genera*, it is often necessary to use them, from the impossibility of finding characteristic words which have not before been employed for other genera. We will commence with those which appear the least open to objection, such as

a. *Geographical names.*—These words being for the most part adjectives can rarely be used for *genera*. As designations of *species* they have been so strongly objected to, that some authors (Wagler, for instance) have gone the length of substituting fresh names wherever they occur; others (e.g. Swainson) will only tolerate them where they apply *exclusively*, as *Lepus hibernicus*, *Troglodytes europæus*, &c. We are by no means disposed to go to this length. It is not the less true that the *Hirundo javanica* is a Javanese bird, even though it may occur in other countries also, and though other species of *Hirundo* may occur in Java. The utmost that can be urged against such words is, that they do not tell the *whole truth*. However, as so many authors object to this class of names, it is better to avoid giving them, except where there is reason to believe that the species is chiefly confined to the country whose name it bears.

b. *Barbarous names.*—Some authors protest strongly against the introduction of exotic words into our Latin nomenclature, others defend the practice with equal warmth. We may remark, first, that the practice is not contrary to classical usage, for the Greeks and Romans did occasionally, though with reluctance, introduce barbarous words in a modified form into their respective languages. Secondly, the preservation of the trivial names which

animals bear in their native countries is often of great use to the traveller in aiding him to discover and identify species. We do not therefore consider, if such words have a Latin termination given to them, that the occasional and judicious use of them as scientific terms can be justly objected to.

c. *Technical names*.—All words expressive of trades and professions have been by some writers excluded from zoology, but without sufficient reason. Words of this class, *when carefully chosen*, often express the peculiar characters and habits of animals in a metaphorical manner, which is highly elegant. We may cite the generic terms, *Arvicola*, *Lanius*, *Pastor*, *Tyrannus*, *Regulus*, *Mimus*, *Ploceus*, &c., as favourable examples of this class of names.

d. *Mythological or historical names*.—When these have no perceptible reference or allusion to the characters of the object on which they are conferred, they may be properly regarded as unmeaning and in bad taste. Thus the generic names, *Lesbia*, *Leilus*, *Remus*, *Corydon*, *Pasiphæ*, have been applied to a Humming bird, a Butterfly, a Beetle, a Parrot, and a Crab respectively, without any perceptible association of ideas. But mythological names may sometimes be used as generic with the same propriety as technical ones, in cases where a direct allusion can be traced between the narrated actions of a personage and the observed habits or structure of an animal. Thus when the name *Progne* is given to a Swallow, *Clotho* to a Spider, *Hydra* to a Polyp, *Athene* to an Owl, *Nestor* to a grey-headed Parrot, &c., a pleasing and beneficial connection is established between classical literature and physical science.

e. *Comparative names*.—The objections which have been raised to words of this class are not without foundation. The names, no less than the definitions of objects, should, where practicable, be drawn from positive and self-evident characters, and not from a comparison with other objects, which may be less known to the reader than the one before him. Specific names expressive of comparative size are also to be avoided, as they may be rendered inaccurate by the after discovery of additional species. The names *Picoides*, *Emberizoides*, *Pseudoluscinia*, *rubeculoides*, *maximus*, *minor*, *minimus*, &c., are examples of this objectionable practice.

f. *Generic names compounded from other genera*.—These are in some degree open to the same imputation as comparative words; but as they often serve to express the position of a genus as intermediate to, or allied with, two other genera, they may occasionally be used with advantage. Care must be taken not to adopt such compound words as are of too great length, and not to corrupt them in trying to render them shorter. The names *Gallopavo*,

Tetraogallus, *Gypaetos*, are examples of the appropriate use of compound words.

g. *Specific names derived from persons*.—So long as these complimentary designations are used with moderation, and are restricted to persons of eminence as scientific zoologists, they may be employed with propriety in cases where expressive or characteristic words are not to be found. But we fully concur with those who censure the practice of naming species after persons of no scientific reputation, as curiosity-dealers (e. g. *Caniveti*, *Boissoneauti*), Peruvian priestesses (*Cora*, *Amazilia*), or Hottentots (*Klassi*).

h. *Generic names derived from persons*.—Words of this class have been very extensively used in botany, and therefore it would have been well to have excluded them wholly from zoology, for the sake of obtaining a *memoria technica* by which the name of a genus would at once tell us to which of the kingdoms of nature it belonged. Some few personal generic names have, however, crept into zoology, as *Cuvieria*, *Mulleria*, *Rossia*, *Lessonia*, &c., but they are very rare in comparison with those of botany, and it is perhaps desirable not to add to their number.

i. *Names of harsh and inelegant pronunciation*.—These words are grating to the ear, either from inelegance of form, as *Huhua Yuhina*, *Crazirex*, *Eschscholtzi*, or from too great length, as *chirostrongylostinus*, *Opetiorhynchus*, *brachypodioides*, *Thecodontosaurus*, not to mention the *Enaliolimnosaurus crocodilocephaloides* of a German naturalist. It is needless to enlarge on the advantage of consulting euphony in the construction of our language. As a general rule it may be recommended to avoid introducing words of more than five syllables.

k. *Ancient names of animals applied in a wrong sense*.—It has been customary, in numerous cases, to apply the names of animals found in classic authors at random to exotic genera or species which were wholly unknown to the ancients. The names *Cebus*, *Callithrix*, *Spiza*, *Kitta*, *Struthus*, are examples. This practice ought by no means to be encouraged. The usual defence for it is, that it is impossible now to identify the species to which the name was anciently applied. But it is certain that if any traveller will take the trouble to collect the vernacular names used by the modern Greeks and Italians for the Vertebrata and Mollusca of southern Europe, the meaning of the ancient names may in most cases be determined with the greatest precision. It has been well remarked that a Cretan fisher boy is a far better commentator on Aristotle's 'History of Animals' than a British or German scholar. The use however of ancient names, *when correctly applied*, is most desirable, for "in framing scientific

terms, the appropriation of old words is preferable to the formation of new ones."*

l. Adjective generic names.—The names of genera are in all cases essentially substantive, and hence adjective terms cannot be employed for them without doing violence to grammar. The generic names *Hians*, *Criniger*, *Cursorius*, *Nitidula*, &c., are examples of this incorrect usage.

m. Hybrid names.—Compound words, whose component parts are taken from two different languages, are great deformities in nomenclature, and naturalists should be especially guarded not to introduce any more such terms into zoology, which furnishes too many examples of them already. We have them compounded of Greek and Latin, as *Dendrofalco*, *Gymnocornus*, *Monoculus*, *Arborophila flavigaster*; Greek and French, as *Jacamaralcyon*, *Jacamerops*; and Greek and English, as *Bullockoides*, *Gilbertsocrinites*.

n. Names closely resembling other names already used.—By Rule 10 it was laid down, that when a name is introduced which is identical with one previously used, the later one should be changed. Some authors have extended the same principle to cases where the later name, when correctly written, only approaches in form, without wholly coinciding with, the earlier. We do not, however, think it advisable to make this law imperative, first, because of the vast extent of our nomenclature, which renders it highly difficult to find a name which shall not bear more or less resemblance in sound to some other; and, secondly, because of the impossibility of fixing a limit to the degree of approximation beyond which such a law should cease to operate. We content ourselves, therefore, with putting forth this proposition merely as a recommendation to naturalists, in selecting generic names, to avoid such as too closely approximate words already adopted. So with respect to species, the judicious naturalist will aim at variety of designation, and will not, for example, call a species *virens* or *virescens* in a genus which already possesses a *viridis*.

o. Corrupted words.—In the construction of compound Latin words, there are certain grammatical rules which have been known and acted on for two thousand years, and which a naturalist is bound to acquaint himself with before he tries his skill in coining zoological terms. One of the chief of these rules is, that in compounding words all the radical or essential parts of the constituent members must be retained, and no change made except in the variable terminations. But several generic names have been lately introduced which run counter to this rule, and

* Whewell, Phil. Ind. Sc. v. i. p. lxxvii.

form most unsightly objects to all who are conversant with the spirit of the Latin language. A name made up of the first half of one word and the last half of another, is as deformed a monster in nomenclature as a Mermaid or a Centaur would be in zoology; yet we find examples in the names *Corcorax* (from *Corvus* and *Pyrrhocorax*), *Cypsnagra* (from *Cypselus* and *Tanagra*), *Merulaxis* (*Merula* and *Synallaxis*), *Loxigilla* (*Loxia* and *Fringilla*), &c. In other cases, where the commencement of both the simple words is retained in the compound, a fault is still committed by cutting off too much of the radical and vital portions, as is the case in *Bucorvus* (from *Buceros* and *Corvus*), *Ninon* (*Nisus* and *Noctua*), &c.

p. Nonsense names.—Some authors having found difficulty in selecting generic names which have not been used before, have adopted the plan of coining words at random without any derivation of meaning whatever. The following are examples: *Viralva*, *Xema*, *Azeca*, *Assiminia*, *Quedius*, *Spisula*. To the same class we may refer anagrams of other generic names, as *Dacelo* and *Cedola* of *Alcedo*, *Zapornia* of *Porzana*, &c. Such verbal trifling as this is in very bad taste, and is especially calculated to bring the science into contempt. It finds no precedent in the Augustan age of Latin, but can be compared only to the puerile quibblings of the middle ages. It is contrary to the genius of all languages, which appear never to produce new words by spontaneous generation, but always to derive them from some other source, however distant or obscure. And it is peculiarly annoying to the etymologist, who, after seeking in vain through the vast storehouses of human language for the parentage of such words, discovers at last that he has been pursuing an *ignis fatuus*.

q. Names previously cancelled by the operation of § 6.—Some authors consider that when a name has been reduced to a synonym by the operations of the laws of priority, they are then at liberty to apply it at pleasure to any new group which may be in want of a name. We consider, however, that when a word has once been proposed in a given sense, and has afterwards sunk into a synonym, it is far better to lay it aside for ever than to run the risk of making confusion by re-issuing it with a new meaning attached.

r. Specific names raised into generic.—It has sometimes been the practice in subdividing an old genus to give to the lesser genera so formed, the names of their respective typical species. Our Rule 13 authorizes the forming a new specific name in such cases; but we further wish to state our objections to the practice altogether. Considering as we do that the original specific names should as far as possible be held sacred, both on the grounds of justice to their authors and of practical convenience to naturalists,

we would strongly dissuade from the *further continuance* of a practice which is gratuitous in itself, and which involves the necessity of altering long established specific names.

We have now pointed out the principal rocks and shoals which lie in the path of the nomenclator; and it will be seen that the navigation through them is by no means easy. The task of constructing a language which shall supply the demands of scientific accuracy on the one hand, and of literary elegance on the other, is not to be inconsiderately undertaken by unqualified persons. Our nomenclature presents but too many flaws and inelegancies already, and as the stern law of priority forbids their removal, it follows that they must remain as monuments of the bad taste or bad scholarship of their authors to the latest ages in which zoology shall be studied.

[*Families to end in idæ, and Subfamilies in inæ.*]

The practice suggested in the following proposition has been adopted by many recent authors, and its simplicity and convenience is so great that we strongly recommend its universal use.

§ B. It is recommended that the assemblages of genera termed *families* should be uniformly named by adding the termination *idæ* to the name of the earliest known, or most typically characterized genus in them; and that their subdivisions, termed *subfamilies*, should be similarly constructed, with the termination *inæ*.

These words are formed by changing the last syllable of the genitive case into *idæ* or *inæ*, as *Strix*, *Strigis*, *Strigidæ*, *Buceros*, *Bucerotis*, *Bucerotidæ*, not *Strixidæ*, *Buceridæ*.

[*Specific names to be written with a small initial.*]

A convenient *memoria technica* may be effected by adopting our next proposition. It has been usual, when the titles of species are derived from proper names, to write them with a capital letter, and hence when the specific name is used alone it is liable to be occasionally mistaken for the title of a genus. But if the titles of *species* were *invariably* written with a *small* initial, and those of *genera* with a *capital*, the eye would at once distinguish the rank of the group referred to, and a possible source of error would be avoided. It should be further remembered that all *species* are *equal*, and should therefore be written all *alike*. We suggest, then, that

§ C. Specific names should *always* be written with a small initial letter, even when derived from persons or places, and generic names should be always written with a capital.

[The authority for a species, exclusive of the genus, to be followed by a distinctive expression]

The systematic names of zoology being still far from that state of fixity which is the ultimate aim of the science, it is frequently necessary for correct indication to append to them the name of the person on whose authority they have been proposed. When the same person is authority both for the specific and generic name, the case is very simple; but when the specific name of one author is annexed to the generic name of another, some difficulty occurs. For example, the *Muscicapa crinita* of Linnæus belongs to the modern genus *Tyrannus* of Vieillot; but Swainson was the first to apply the specific name of Linnæus to the generic one of Vieillot. The question now arises, Whose authority is to be quoted for the name *Tyrannus crinitus*? The expression *Tyrannus crinitus*, Linn., would imply what is untrue, for Linnæus did not use the term *Tyrannus*; and *Tyrannus crinitus*, Vieill., is equally incorrect, for Vieillot did not adopt the name *crinitus*. If we call it *Tyrannus crinitus*, Sw., it would imply that Swainson was the first to describe the species, and Linnæus would be robbed of his due credit. If we term it *Tyrannus*, Vieill., *crinitus*, Linn., we use a form which, though expressing the facts correctly, and therefore not without advantage in particular cases where great exactness is required, is yet too lengthy and inconvenient to be used with ease and rapidity. Of the three persons concerned with the construction of a binomial title in the case before us, we conceive that the author who *first* describes and names a species which forms the groundwork of later generalisations, possesses a higher claim to have his name recorded than he who afterwards defines a genus which is found to embrace that species, or who may be the mere accidental means of bringing the generic and specific names into contact. By giving the authority for the *specific* name in preference to all others, the inquirer is referred *directly* to the original description, habitat, &c., of the species, and is at the same time reminded of the date of its discovery; while genera, being less numerous than species, may be carried in the memory, or referred to in systematic works without the necessity of perpetually quoting their authorities. The most simple mode then for ordinary use seems to be, to append to the original authority for the species, when not applying to the genus also, some distinctive mark, such as (*sp.*), implying an exclusive reference to the *specific* name, as *Tyrannus crinitus* (Linn.) (*sp.*), and to omit this expression when the same authority attaches to both genus and species, as *Ostrea edulis*, Linn.* Therefore,

* The expression *Tyrannus crinitus* (Linn.) would perhaps be preferable from its greater brevity.

§ D. It is recommended that the authority for a specific name, *when not applying to the generic name also*, should be followed by the distinctive expression (*sp.*)

[*New genera and species to be defined amply and publicly.*]

A large proportion of the complicated mass of synonyms which has now become the opprobrium of zoology, has originated either from the slovenly and imperfect manner in which species and groups have been originally defined, or from their definitions having been inserted in obscure local publications which have never obtained an extensive circulation. Therefore, although under § 12, we have conceded that mere insertion in a printed book is sufficient for *publication*, yet we would strongly advise the authors of new groups always to give, in the first instance, a full and accurate definition of their characters, and to insert the same in such periodical or other works as are likely to obtain an immediate and extensive circulation. To state this briefly,

§ E. It is recommended that new genera or species be *amply* defined, and *extensively* circulated in the first instance.

[*The names to be given to subdivisions of genera to agree in gender with the original genus.*]

In order to preserve specific names as far as possible in an unaltered form, whatever may be the changes which the genera to which they are referred may undergo, it is desirable, when it can be done with propriety, to make the new subdivisions of genera agree *in gender* with the old groups from which they are formed. This recommendation does not, however, authorise the changing the gender or termination of a genus already established. In brief,

§ F. It is recommended that in subdividing an old genus in future, the names given to the subdivisions should agree in gender with that of the original group.

[*Etymologies and types of new genera to be stated.*]

It is obvious that the names of genera would in general be far more carefully constructed, and their definitions would be rendered more exact, if authors would adopt the following suggestion:—

§ G. It is recommended that in defining new genera the etymology of the name should be always stated, and that one species should be invariably selected as a type or standard of reference.

In concluding this outline of a scheme for the rectification of zoological nomenclature, we have only to remark, that almost

the whole of the propositions contained in it may be applied with equal correctness to the sister science of botany. We have preferred, however, in this essay to limit our views to zoology, both for the sake of rendering the question less complex, and because we conceive that the botanical nomenclature of the present day stands in much less need of distinct enactment than the zoological. The admirable rules laid down by Linnæus, Smith, Decandolle, and other botanists (to which, no less than to the works of Fabricius, Illiger, Vigers, Swainson, and other zoologists, we have been much indebted in preparing the present document), have always exercised a beneficial influence over their disciples. Hence the language of botany has attained a more perfect and stable condition than that of zoology; and if this attempt at reformation may have the effect of advancing zoological nomenclature beyond its present backward and abnormal state, the wishes of its promoters will be fully attained.

(Signed)	H. E. STRICKLAND.	J. S. HENSLOW.
	JOHN PHILLIPS.	W. E. SUCKHARD.
	JOHN RICHARDSON.	G. R. WATERHOUSE.
	RICHARD OWEN.	W. YARRELL.
	LEONARD JENYNS.	C. DARWIN.
	W. J. BRODERIP.	J. O. WESTWOOD.

June 27, 1842.

PROCEEDINGS OF SOCIETIES.

*British Association for the Advancement of Science, held at
Newcastle, August 1863.*

SECTION A.—MATHEMATICAL AND PHYSICAL SCIENCE.

President—Professor W. J. MACQUORN RANKINE.

“Report of the Committee on Electrical Standards.” By Mr FLEMING JENKIN.—The Committee report that the system of so-called absolute electrical units, based on purely mechanical measurements, is the only system consistent with our present knowledge, both of the relations existing between the various electrical phenomena, and of the connection between these and the fundamental measurements of time, mass, and space. The doubts felt as to the accuracy with which this system could be reduced to practice have been dispelled by the success of experiments made for the Committee by Prof. Maxwell, Mr Stewart, and Mr Jenkin on the measurement of the absolute resistance of a conductor, by a method due to Prof. W. Thomson. Standard resistance coils will shortly be issued based on these experiments, which will, however, be repeated, with entirely new data, before this final step is taken, so as to avoid every chance

of error. The other electrical measurements in the absolute system offer comparatively little difficulty.

"Researches on the Moon." By Prof. PHILLIPS.—The author having on previous occasions presented his views as to the methods and objects of research in the moon, was desirous now to state a few results, and exhibit a few drawings, the fruit of recent examinations of the moon by means of a new equatorial by Cooke, with an object-glass of 6 inches. In sketching ring mountains, such as Theophilus and Posidonius, the author has been greatly interested by the changes of aspect which even a small alteration in the angles of elevation and azimuth respectively produces in the shadow and lights. Taking an example from Cyrillus, with its rocky interior, and fixing attention on the nearly central mountain, it always appears in the morning light to have two principal unperforated masses. By a slight change in the direction of the light, the division of these masses is deeply shaded on the north or deeply shaded on the south, and the figure of the masses, *i.e.* the limit of light and shade, seems altered. A slight change in the angle of elevation of the incident light makes more remarkable differences. On Posidonius, which is a low, nearly level plateau, within moderately raised borders, the mid morning light shows with beautiful distinctness the shield-like disc of the mountain, with narrow broken walls, and in the interior, broad, easy undulations, one large and several smaller craters. In earlier morning more craters appear, and the interior ridges gather to form a broken terrace subordinate to the principal ridge. This circumstance of an interior broken terrace, under the high main ring of mountain, is very frequent, but it is often concealed by the shadow of the great ridge in early morning shadows. To see it emerge into half-lights, and finally to distinct digitations and variously directed ridges, as the light falls at increasing angles, is a very beautiful sight. But it is chiefly to the variations in the central masses of lunar mountains and their physical bearings that the author wishes to direct attention. Many smaller mountains are simply like cups set in saucers, while others contain only one central or several dispersed cups. In Plato is a nearly central very small cup, bright, and giving a distinct shadow on the grey ground, as seen by Mr Lockyer, Mr Birt, and Professor Phillips himself. But in the centre of many of the larger mountains, as Copernicus, Gassendi and Theophilus, is a large mass of broken rocky country, 5,000 or 6,000 feet high, with buttresses passing off into collateral ridges, or an undulated surface of low ridges and hollows. The most remarkable object of this kind which the author has yet observed with attention is in Theophilus, of which mountain two drawings are given, in which the author places equal confidence, except that the later drawing may have the advantage of more experience. The central mass is seen under powers of 200–400 (the best performance is from 200 to 300), and appears as a large conical mass of rocks about fifteen miles in diameter, and divided by deep chasms radiating from the centre. The rock-masses between these deep clefts are bright and shining, the clefts widen towards the centre, the eastern side is more diversified than the western, and like the southern side has long excurrent buttresses. As the light grows on the mountain, point after point of the mass on the eastern side comes out of the shade, and the whole figure resembles an uplifted mass which broke with radiating cracks in the act of elevation. Excepting in steepness, this resembles the theoretical Mont d'Or of De Beaumont; and as there is no mark of cups or craters in this mass of broken ground the author is disposed to regard its origin as really due to the displacement of a solidified part of the moon's crust. He might be justified by Prof. Secchi's drawing of Copernicus, in inquiring if the low excurrent buttresses may

indicate issues of lava on the southern and western sides? On the whole, the author is confirmed in the opinion he has elsewhere expressed, that on the moon's face are features more strongly marked than on our own globe, which, rightly studied, may lead to a knowledge of volcanic action under grander and simpler conditions than have prevailed on the earth during the period of subaërial volcanoes. The author also exhibited a drawing of Aristarchus, showing some undescribed features in the aspect of that, the highest part of the moon's surface.

"On Sun Spots and their Connection with Planetary Configuration." By Mr BALFOUR STEWART.—The author of this note having recently found in the course of conversation with Prof. P. G. Tait of Edinburgh, that they had both, independently, and by different trains of reasoning, been led to suspect the existence of a new principle in Natural Philosophy, which, however, must of course stand or fall by appeal to experiment and observation,—it fell to his lot to endeavour to ascertain if there be any connection between sun-spots and planetary configurations. In a preliminary inquiry with this object in view, he was led to observe the changes with regard to size which take place in sun-spots, from a remark by Mr Beckley of Kew Observatory, that, during a certain period, he did not observe any spots *break out* on the visible disc of our luminary. Besides about six months' records of these phenomena, made by means of the Kew photoheliograph at the Kew Observatory, the author has had the opportunity of investigating a year's records made by the same instrument at Mr De La Rue's private Observatory at Cranford. All of these are collodion negatives, and besides embracing a few months in the end of 1859, they give an almost continuous record of the state of the sun's disc between February 1862 and the present date. There is little difficulty in finding from these, by means of a comparison of two or three consecutive pictures, approximately, at what portion of the sun's disc any spot ceases to increase and begins to wane, or, on the other hand, breaks out into a visible appearance. Now it appears to be a law nearly universal, that if we divide the disc of the sun roughly into longitude by vertical diameters, and if there be a number of spots on the surface of the sun, these will all behave in the same manner as they cross the same longitude; that is to say, if one spot decreases another will decrease also, and so on. This law can, of course, be only approximately ascertained by means of a preliminary examination of this nature; but the impression produced upon the author is very strong, that if one spot decreases before coming to the central line, another does the same; if, on the other hand, one spot breaks out on the right half and increases up to the border, another will do the same. The author thinks, moreover, that he has noticed a connection between this behaviour of sun-spots, and the configuration of the nearer planets, Mercury and Venus, and it would seem to be of this nature. Remembering that all motions are from left to right, let us suppose that Mercury and Venus are both in a line considerably to the left of the Earth; then spots will decrease as they come round from the left-hand side, and before they reach the centre of the disc. On the other hand, if these two planets are considerably to the right of the Earth, there will be a tendency for spots to form on the right half of the disc, and to increase up to the border. The author would, however, guard himself against the supposition that he attributes all the phenomena of spots to the agency of these two planets.

"On the Path of a Meteoric Fireball relatively to the Earth's Centre." By Prof. COFFIN.

"On the changing Colours of the Star 95 Herculis." By Prof. C.
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PIAZZI SMYTH.—The star 95 Herculis is a double star, of which the two members are nearly of equal magnitude, about the 5th, and are six seconds apart, in R.A. $17^{\text{h}}. 55' 33''$ and D $21^{\circ} 25' 56''$ in 1860. It has hitherto been catalogued as a diversely coloured pair of stars to an extreme degree, one member being called "apple green" and the other "cherry red." These colours have moreover been looked upon, as are the colours of all ordinary stars, as constant features sensibly. Being observed, however, by the author, when he was on the peak of Teneriffe, in 1856, they were found nearly colourless, and without any diversity of tint, the one from the other. This observation appears to have been considered anomalous, and was so to a certain extent; but on examining older authorities the author has met with two other instances of an equality of pale colour being observed on the two components of 95 Herculis—one by Sestini, in 1844–5, and the other by Struve in 1832–53; and remarks that while these two epochs are separated by twelve years exact to a tenth, the latter of them precedes the Teneriffe observation in 1856–58 by almost exactly the same quantity. Now this looks like a regular periodic change, and in a very short period, for it is not improbable but that the twelve years is a multiple of a shorter period still, and during which the change of tint of the stars is so marked that from being merely grey at a certain time, one star has been described as becoming an "astonishing yellow green," and the other "an egregious red." Although this is the first instance of this kind yet detected in the sky, the author thinks that it will not be found a solitary one; and that its phenomena may bear some relation to the "eclipse" pink prominences of our sun, and to auroral displays about the earth.

"On a New Revolving Scale for Measuring Curved Lines." By Mr H. SCHLAGINTWEIT.—This instrument consisted of a small brass wheel revolving in a short handle, the circumference about two inches round, having a number of very short steel pins inserted radially, the number depending on the scale to which it was desired to measure the curve; and the side of the wheel having graduations corresponding to the pins on the circumference, the zero and other remarkable divisions being distinguished from the lesser graduations.

"On some Phenomena produced by the Refractive Power of the Eye." By Mr A. CLAUDET.—This paper was to explain several effects of the refraction through the eye, one of which is, that objects situated a little behind us, are seen as if they were on a straight line from right to left. Another, that the pictures of external objects which are represented on the retina, are included in an angle much larger than one-half of the sphere at the centre of which the observer is placed; from this point of view a single glance encompasses a vast and splendid panorama extending to an angle of 200° . This is the result of the common law of refraction.

Mr B. S. PROCTOR read a paper "On the Focal Adjustment of the Eye."

M. CLAUDET exhibited his "Star Chromatoscope," a small telescope fixed upon a stand, and revolving. It is used to observe stars, and the effect produced is, that on looking through the instrument a luminous ring is seen reflecting the particular colour or colours of the star observed.

Dr AKIN read a paper "On the Transmutation of Spectral Rays," including some curious speculations on the luminosity of the sun and stars.

The Abbé MOIGNO exhibited and explained the "Tenebroscope" invented by M. Soleil to illustrate the principle of the invisibility of light. The Abbé humorously observed that this term was somewhat of an Irishism, but nevertheless it was a fact that light is quite invisible, for celestial space

was perfectly dark until the appearance of a celestial body, and then it became perfectly illuminated.

The instrument is formed in the manner of a common spy-glass, with a break in the centre of the tube to admit light. Below is a spring, with a small ivory ball attached. The observer looks through one end of the instrument and sees that all is dulness; but when the spring is raised, the space becomes illuminated with light, showing that the introduction of the ball is necessary to render the light visible.

Professor C. PLAZZI SMYTH then entered into a history and explanation of the time-signals.

"On a Printing-Telegraph." By Professor D. E. HUGHES.—This instrument possesses special merits not only for land lines, but for long sub-marine lines, from the fact of its requiring but one wave to each letter, and the sensitiveness and simplicity of the electrical arrangements. The following rates of speed were obtained in different lengths of cables:—

Atlantic cable, ...	2500 miles, 4 words per minute.
Red Sea, do., ...	2000 " 6 "
Do. do., ...	1000 " 10 "
Do. do., ...	500 " 24 "
Denmark do., ...	360 " 22 "
Tasmania do., ...	240 " 30 "

On aerial lines the average speed of good operators is forty words per minute.

Mr W. COOK read a paper on the Printing-Telegraph invented by M. le Chevallier Bonelli (the former Director of Telegraph in Italy), in which the messages are printed by means of types.—The telegraph transmitted a message in six seconds, and it was quite possible to work it at a long distance.

Mr W. LADD exhibited a new form of Syren, adapted for the lecture-table, to illustrate acoustics and harmony.

Mr LADD exhibited an acoustic telegraph for the transmission of musical notes and sounds between distant points, this ingenious contrivance being the invention of a schoolmaster at Frankfort, whom Mr Ladd had met with in his travels.

Mr LADD also exhibited a small electromotive engine, constructed by himself on a new principle, and calculated to lift a weight of twenty-two lbs. eight inches per minute.

Professor CHEVALLIER introduced a new series of numbers, invented by Mr R. S. BROWNE, for expressing the distances between the planets.

Professor C. W. SIEMENS read a paper "On the Electrical Resistance and the Electrification of India-rubber under a pressure of 300 tons," detailing experiments he had made with the Malta and Alexandria submarine cable, &c.—The Professor explained that, as Mr Jenkin had previously discovered that electrification is not influenced by temperature, so his own experiments went to show that it was not affected by pressure; but the latter exercised this abnormal influence on these materials, that it increased the resistance in gutta serena, but decreased it in India-rubber.

"Report on Luminous Meteors." By Mr J. GLAISHER.

"Description of a Solar Eye-piece," made by the Rev. Mr Dawes, F.R.A.S. By Dr LEE.—The peculiarity of the instrument consisted in having a metallic slide, with perforations of different sizes, which crossed the eye-tube at right angles, just at the focus of the object-glass, and

though the slide would become greatly heated while viewing the sun, the conduction was cut off by interposing a plate of ivory.

"On the Relationship between the Variation of the Earth's Eccentricity and that of the Moon's Mean Motion in Longitude." By Dr HINCKS.

"On the Connection that exists between Admiral Fitzroy's 'Caution Telegrams' and the Luminosity of Phosphorus." By Dr MOFFAT.

"Report of Balloon Committee." By Dr LEE.

"Report on Balloon Ascents." By Mr J. GLAISHER.

"On the Selenographical Relations between the Chain of Lunar Mountains 'the Alps,' with the 'Mare Imbrium,' and the 'Mare Frigoris.'" By Mr W. R. BIRT.

Professor CHEVALLIER described a New Instrument for Ascertaining the Height of a Cloud by taking the Measure of the Shadow Projected—the instrument being formed something like a "two-foot rule" with an upright piece attached.

"On the Distribution of Heat on the Sun's Surface, and the Currents of his Atmosphere." By Mr J. J. MURPHY.

"On Ozone, more especially on Ozone Tests." By Mr E. J. LOWE.

"Comparisons of Curves afforded by Self-recording Magnetographs at Kew and Lisbon." By Mr BALFOUR STEWART.

"Description of the Experimental Series of Rain-Gauges erected at Calne." By Mr G. J. SYMONS.

"Meteorological Observations." By Rev. J. RANKINE.

"On a New Kind of Miniature, possessing Apparent Solidity by Means of a Combination of Prisms." By Mr H. SWAN.

Dr LEE read a paper descriptive of certain Mountains and Craters in the Moon, discovered by himself and Mr Birt. One they had named "Mare Smythii," in honour of Professor C. Piazzi Smyth. Another portion of the lunar landscape had been named "the Phillips Walled Plain," in honour of Professor Phillips, who had bestowed much attention on the physical features of the lunar surface. Thirdly, they had given to an elevated range the name of the "Percy Mountains," in compliment to His Grace the Duke of Northumberland, who had been the only nobleman to go through a complete course of university study of three years in astronomy. The Duke had also rendered the important service of presenting to the University of Cambridge their large Northumberland Telescope.

"On the System of Forecasting the Weather pursued in Holland." By Dr BUYS BALLOT.

"Account of Preliminary Experiments on Chalcescence." By Dr AKIN.

"On Spectral Analysis." By Professor PLUCKER.

"On Specific Refractive Energy." By Dr GLADSTONE and Mr Dale.

"On the Quantity and Centre of Gravity of Figures given in Perspective, or Homography." By Professor SYLVESTER.

"On the Augmentation of the Apparent Diameter of a Body by Atmospheric Refraction." By Mr S. ALEXANDER.

"On the Conditions of the Resolvability of Homogeneous Algebraical Polynomials into Factors." By Mr J. J. WALKER.

"On the Elasticity of the Vapour of Sulphuric Acid." By Mr T. TATE.

"On the Result of Reductions of Curves obtained from the Self-recording Electrometer at Kew." By Professor W. THOMSON.

"On the Mathematical Theory of Plane Water Lines." By Professor RANKINE.

"On a certain class of Mathematical Symbols." By Mr W. H. RUSSELL.

"On a Mercurial Air-Pump." By Mr J. SWAN.

L'Abbé MORENO exhibited the following instruments:—A Free Air Barometer, by the Abbé Jeannon; a Metallic or Holosteric Barometer, by M. Naudet; a New Micrometer, by M. H. Soleil.

L'Abbé MORENO also exhibited Specimens of Telegraphic Facsimiles, produced by Casselli's method.

L'Abbé MORENO communicated a paper by M. Oudry on Galvanic Copper and its Applications.

"On a New Marine Barometer, and on a Maximum Thermometer with a New Scale." By Mr W. SYMONS.

"Interim Report on the Vertical Motion of Currents of Air." By Professor HENESSY.

"On the Proof of the Dioptric and Actinic Quality of the Atmosphere at a High Elevation." By Professor PIAZZI SMYTH.

SECTION B.—CHEMICAL SCIENCE.

President—Professor A. W. WILLIAMSON.

"On Fire-Clay Goods." By Mr J. COWEN, jun.—Fire-clay is obtained in large quantities in the two counties of Durham and Northumberland. It usually lies beneath the Coal Measures in layers, varying in thickness from twelve inches to five or six feet. It is found in most abundance, and in the best quality, beneath the coal used for coking and manufacturing purposes. Silica and alumina are the two substances of which fire-clay is chiefly composed.

The trade in fire-clay goods is of comparatively recent origin. Its progress at first was slow, but during the last forty years it has extended rapidly, and is still on the increase. The extension during the last twenty-five years has been especially marked and important. Fire-bricks were first made on the Tyne about one hundred years ago.

For many years all that were made were consumed in the manufactories in the neighbourhood. Slowly but gradually the trade increased, and the goods made were sent in larger or smaller quantities to different parts of the United Kingdom, and to some of the chief ports in Europe and the British colonies. The extent of the trade in 1838 was as follows:—The number of bricks manufactured at that time per annum was about 7,000,000. The local consumption then was about 2,500,000. The quantity sent to other parts of the United Kingdom was about 3,000,000, and the quantity exported was about 1,500,000. The extent of the trade at the present time is as follows:—

Number of Fire-bricks made per annum about 80,000,000

The local consumption is estimated at 43,000,000

The quantity sent to other parts of the United Kingdom is estimated at..... } 27,500,000

And the quantity exported 9,500,000

"On Glass." By Mr R. W. SWINBURNE.—The art of casting plate glass by throwing the molten material on an iron or copper table, and rolling it into a sheet of equal thickness, was first adopted in England, in Lancashire, in 1771; but there is abundant proof that plate glass of smaller dimensions was made before that time at South Shields, where is situated the only manufactory of plate glass in this district. In the early part of the seventeenth century, an article called blown plate glass was made at that place, and the manufacture was continued by the family who originally established it until 1845, when the process was abandoned, being entirely superseded by the cast plate. In this is afforded an instance of the superiority of machinery over manual labour. Blown plate glass, which was the great original of the art in all countries, depended entirely in its manipulation on the strength of lungs and dexterity of muscle of the individual operator, whose *chef-d'œuvre* was about four feet long and two and a half feet wide, whereas the cast is made by the co-operative efforts of twenty men, who move from the furnace the crucible in which the material is melted, and by means of powerful machinery roll it into a plane of any required dimensions. The record of the daily manufacture of blown plate glass at South Shields, in 1750, is still extant, and affords a curious proof of the infancy of the art and of the difficulties of the operator. Up to the year 1845, the returns of the Excise duty show that there was more plate glass made at South Shields than at any other manufactory in the kingdom. In that year the Excise duty on glass was abrogated, and in consequence the produce of this manufactory has been quadrupled. Previously to 1845, the quantity of unpolished plate glass blown and cast at South Shields was 312,000 feet per annum: now its capability of produce is 1,240,000 feet per annum. A new kind of plate glass, called rolled plate, has been for some time manufactured at Sunderland, by the spirited firm of Messrs Hartley & Co. The invention is due to Mr James Hartley, who has had the honour of establishing a new branch of manufacture of great public utility. This new article somewhat resembles unpolished plate glass, but is lighter in substance and eminently fitted for roofing and other purposes of construction where translucency only is required.

"On Earthenware." By Mr C. T. MALING.—The manufacture of white earthenware was introduced into this district by Mr Warburton, at Carr's Hill Pottery, near Gateshead, about 1730 or 1740. Those works were very successfully carried on for seventy years, when they gradually declined, and in 1817 were closed; a small portion of the building is still used as a brown-ware pottery. The next manufactory was built by Mr Byers, at Newbattle, in the county of Durham, about 1755, where brown and white earthenware still continue to be made. There are now about twenty-five potteries in this district, of which, on the Tyne, six manufacture white and printed ware; four, white, printed, and brown ware; and three brown ware only—employing 1200 people, and manufacturing, yearly, about 12,000 tons of white clay, and 3000 tons of brown clay, and consuming in the process of manufacture about 34,000 tons of coals. On the Wear, there are two potteries manufacturing white and printed ware; two white, printed, and house ware; and two brown ware only—employing about 500 people, manufacturing yearly about 4000 tons of white clay, 1500 tons of brown clay, and consuming in the manufacture about 14,000 tons of coals. On the Tees there are four potteries, manufacturing white and printed ware, employing 500 people, manufacturing 5000 tons of white clay, and consuming 13,000 tons of coals. The description of goods manufactured in this district is that used by the middle and working

classes, no first-class goods being made here. The principal markets, in addition to the local trade, are the Danish, Norwegian, German, Mediterranean, and London, for exportation to the colonies. The trade to the United States being so very small from here, the American war has affected this district less than any other.

"On the Presence of a Salt of Baryta in Colliery Water." By Dr T. RICHARDSON.

"On the Oxidation of (β) Hexylic Alcohol." By Professor WANKLYN.

"On some Results of Experiments on Lucifer Matches and others ignited by Friction." By Professor ABEL.—Having mentioned the components, which chiefly consisted of ordinary phosphorus and gum or glue as a binding material, he went on to notice the possible causes of accident in the transport of matches. The result of experiments proved that no degree of heat to which, under all ordinary circumstances, matches were likely to be exposed in their transport or otherwise, would suffice to lead to their spontaneous ignition. It was quite within the range of possibility, however, that on board ship continuous concussion, combined with a degree of heat, might bring about accidental ignition of matches, while it might be granted that the accidental ignition of one or two boxes in securely-closed cases might frequently occur almost without a possibility of fear of it spreading to other boxes. A knowledge of the causes of the accidental ignition of gunpowder and other explosives rendered it advisable that such precautionary measures as were obvious and easily observed should be attended to in the shipment of matches, with the view of reducing such occurrences to the minimum extent. Some of those steps he specified. The *first* was the appropriation of a place for the reception of such packages, distinct from all other merchandise. *Secondly*, The efficient ventilation of that part of the vessel in which matches were stowed. *Thirdly*, The enforcement of rules to prevent fire being brought by sailors within the vicinity of the matches. *Fourthly*, The carefully packing of the match-boxes into cases, so as to prevent any independent motion. And, *fifthly*, The bestowal of more uniform attention on the production of stout and sufficiently stable match-boxes.

Dr PAUL believed a considerable proportion of the matches now used were dipped with paraffine—a substitute for the sulphur that was originally used, and the resinous coating that had been applied to them. The effect was very advantageous. Paraffine matches ignited readily. There was no smell from them, and they were otherwise preferable.

"On a New Gas Furnace for Scientific and Practical Purposes" By Mr G. GORE.

"On Disinfectants." By Mr H. B. CONDY.

"Report of the Committee on Gun-Cotton."—Dr GLADSTONE read the chemical portion of the report. It stated that during the year the committee had been put in possession of the fullest information on the subject, by Baron William Von Leuk, Major-General of the Austrian Artillery, who was the inventor of the system by which gun-cotton was made available for warlike purposes; and Professor Abel, chemist of the War Department, by permission of the Secretary of State for War, had communicated to them the information given by the Austrian Government to the Government of this country. The committee had made no experiments themselves. The subject might naturally be divided into two conditions—the chemical and mechanical. Taking the chemical first, that department included the manufacture of the gun-cotton itself, its liability or non-liability to spontaneous combustion, and

the nature and effects of the products into which it was resolved on explosion. As to the chemical nature of the material itself, the gun-cotton differed from the gun-cotton generally made, in its complete conversion into a uniform chemical compact. General Leuk secured the production of his gun-cotton by several precautions. Of these the most important were, the cleansing and perfect dessication of the cotton as a preliminary to its immersion in the acid; the employment of the strongest acids obtainable in commerce; the steeping of the cotton in a fresh strong mixture of acids, after its first immersion and consequent imperfect conversion into gun-cotton; the continuance of this steeping for forty-eight hours. Equally necessary is the thorough purification of the gun-cotton so produced from every trace of free acid. There is one part of the process of the manufacture, the value of which is not open to doubt—viz., the treatment of the gun-cotton with a solution of silicate of potash, commonly called water-glass. The chief advantages of the material were set forth in the mechanical report, but it was here stated that the fact that gun-cotton is not injured by damp like gunpowder, is one of its recommendations, while a still greater chemical advantage which it possesses arises from its being perfectly resolved into gases on explosion, so that there is no smoke to obscure the sight of the soldier who is firing, or to point out his position to the enemy, and no residue left in the gun to be get rid of before another charge can be introduced.

Mr SCOTT RUSSELL, F.R.S., submitted the mechanical report. After a long and careful examination, the committee were able to understand and reconcile themselves to the fact that greater mechanical effects are produced from gases generated by gun-cotton than by those generated by gunpowder. The same quantity of gases and the same number of atmospheres seemed to be produced from both materials, and it did not appear to mechanical men that there was a greater advantage in gun-cotton in that respect. The next inquiry was into the distinctive nature between the action of these gases in gunpowder and the action of those gases in gun-cotton. The great waste of force in gunpowder constituted an important difference between it and the gun-cotton, in which there was no waste. Gunpowder consisted of about 68 per cent. of solid matter, only 32 of which was useful gases. It might be seen, therefore, that one-third of gunpowder is not directly useful in producing gases. There was another peculiar feature of gun-cotton—it could be exploded in any quantity instantaneously. General Leuk had discovered the means of giving gun-cotton any velocity of explosion that is required by merely the mechanical arrangement under which it is used. Gunpowder in his hands had any speed of explosion, from one foot per second, to one in 1000th of a second, or to instantaneity. The spontaneous explosion of a large quantity of gun-cotton is made use of when it is required to produce destructive effects, and it is found that the condition necessary to produce instantaneous combustion is the absolute perfection of the closeness of the chamber containing the gun-cotton. On the other hand, if they desired gun-cotton to produce a different effect, they must provide for its slower combustion. It must be opened out mechanically, so as to occupy a large space, and in this state it can be made to act even more slowly than gunpowder, and the limits for the purposes of artillery. In general it is found that the proportion of 11lbs. of gun-cotton occupying one cubic foot of space, produce a greater force than gunpowder, and a force of the nature required for ordinary artillery. But each gun and each kind of projectile requires a certain density of cartridge. Practically, gun-cotton is most effective in guns, when used as a quarter to one-third

weight of powder, and occupying a space of one and one-tenth of the length of the powder cartridge. In regard to safety, it was a fact that during the ten years of the manufacture of General Leuk's gun-cotton at the imperial factory at Kirtenberg, and during ten years' storage of that material in the imperial magazines at Steinfeldt, in which thousands of cwts. were deposited, not one single accident occurred. The best temperature for gun-cotton was 136 centigrade, or between 277 and 338 Fahrenheit—a temperature sufficiently high to ensure safety for all practical purposes. The cost of production was considerably less than that of gunpowder, the price and quantities being compared which will produce equal effects. As to the mechanical purposes of the cotton, it is used for artillery in the form of gun-cotton thread or spun-yarn. In this simple form, it would conduct combustion slowly in the open air at the rate of not more than one second. This thread was woven into a texture or circular web. These webs were made of various diameters, and out of them cotton rifle cartridges were made by cutting them into the proper length. The cotton web was generally enclosed in India-rubber tubes, in which form it is most convenient. For the explosion of mines it is used in the form of ropes. *Conveyance and Storage of Gun-Cotton.*—One pound of gun-cotton produces effects somewhat exceeding 3 lbs. of gunpowder in artillery. This is a material advantage, whether it be carried by men, by horses, or in waggons. It may be placed in a store and preserved with great safety. The danger from explosion does not arise until it is confined. It may become damp, and even perfectly wet, and, without injury, may be dried by mere exposure to the air. *Practical Use in Artillery.*—The gun keeps clean, and requires less windage, and therefore performs much better in continuous firing. In gunpowder there is 68 per cent. of refuse, or the matter of fouling. In gun-cotton there is no residuum, and therefore no fouling. Experiments made by the Austrian committee proved that 100 lbs. could be fired with gun-cotton against 30 lbs. of gunpowder. From the low temperature produced by gun-cotton, the gun does not heat. Experiments showed that 100 lbs. were fired with a 6-pounder in thirty-four minutes, and the heat was raised by gun-cotton to only 122 degrees of Fahrenheit; whilst 100 lbs. of gunpowder took 100 minutes, and raised the temperature to such a degree that water was instantly evaporated. The firing with the gunpowder was therefore discontinued, but the rapid firing with the gun-cotton was continued up to 180 lbs. without any inconvenience. The absence of fouling allows the mechanism of a gun to have more exactness than where allowance is made for fouling. The absence of smoke permits rapid firing and exact time. The fact of smaller recoil from a gun-charged with gun-cotton is established by direct experiments. Its value is two-thirds of the recoil from gunpowder, the projectile being equal. *Practical Application to Destructive Explosion.*—It is ascertained that the same shell is exploded by the same volume of gas generated from gun cotton and gunpowder into more than double the number of pieces; and it is a startling fact, that the stronger and thicker the shell the smaller and more numerous are the fragments. *Mining Uses.*—The fact that the action of the gun-cotton is violent and rapid in exact proportion to the resistance it encounters, tells us the secret of the far higher efficacy of gun-cotton in mining than gunpowder. The stronger the rock, the less gun-cotton comparatively with gunpowder is found necessary for the effect—so much so that while gun-cotton is stronger than gunpowder, weight for weight, as three to one in artillery, it is stronger in the proportion 6:27 to that of strong solid rock, weight for weight. It is

the hollow rope form which is used for blasting. Its power in splitting up material is exerted exactly as you wish. With regard to the military and submarine explosion, it is a well-known fact that a bag of gun-powder nailed on the gates of a city will blow them open. A bag of cotton exploded in the same way produces no effect. To blow up the gates of a city with gun-cotton, it must be confined before explosion. Twenty pounds of gun-cotton carried in the hands of a single man would be sufficient, only he must know its value.

"On Chemical Manufactures on the Tyne." By Mr J. C. STEVENSON.
—The manufactures included Salt, Soda Ash, Caustic Soda, Hyposulphite of Soda, Manganese, French Limestone, Bleaching Powder, Soap, Prussiate of Potash, Alum, Epsom Salts, Carbonate of Magnesia, Superphosphate of Lime, Pearl Hardening, Sulphate of Iron, Venetian Red, Sulphate of Copper, Resin Size, Lamp Black, Grease.

QUANTITIES AND PRICES.—RAW MATERIALS.

	Tons.	Price.	Value.
Sulphur (included as Pyrites)...	72,800	£0 10 0	£169,200 0 0
(Copper value not included.)			
Salt, ...	90,000	0 15 0	67,500 0 0
Nitrate of soda, ...	2,500	14 15 0	36,875 0 0
Chalk, ...	144,000	0 2 6	18,000 0 0
Coals, ...	823,000	0 8 9	60,562 10 0
Manganese, ...	11,400	4 0 0	45,600 0 0
Rough Epsom salt, ...	1,500	2 5 0	3,875 0 0
Magnesian limestone, ...	700	0 8 6	122 10 0
French limestone, ...	14,000	0 4 6	3,150 0 0
Resin, ...	...		
Tallow, ...	...		

FINISHED PRODUCTS.

Alkali, ...	43,500	£8 10 0	£369,750 0 0
Crystals of soda, ...	51,800	4 15 0	243,675 0 0
Bi-carbonate of soda, ...	7,450	12 0 0	89,400 0 0
Caustic soda, ...	580	18 0 0	10,410 0 0
Hyposulphite of soda, ...	400	25 0 0	10,000 0 0
Oil of vitriol, ...	6,440	6 0 0	38,640 0 0
Epsom salts, ...	1,500	7 5 0	10,870 0 0
Hydrochloric acid, ...	180,000		
Sulphuric acid, used in the manufacture of soda, }	86,320		
Bleaching powder, ...	11,200	9 0 0	100,800 0 0
Soap, ...	6,000	34 0 0	204,000 0 0
Yellow prussiate of potash, ...	105	0 1 0 ^{per} lb.	11,760 0 0
Red ditto, ...	40	0 2 6 ^{per} lb.	11,200 0 0
Alum, ...	4,000	7 0 0	28,000 0 0
Carbonate of magnesia, ...	250		7,500 0 0
Superphosphate of lime, ...	15,000	5 0 0	75,000 0 0
Pearl hardening, ...	2,000	10 0 0	20,000 0 0
Sulphate of iron, ...	2,000	3 0 0	6,000 0 0
Venetian red, ...	4,000	5 0 0	20,000 0 0
Sulphate of copper, ...	100	85 0 0	8,500 0 0
Resin size, ...	100	7 0 0	700 0 0
Lamp black, ...	1,200	7 0 0	8,400 0 0
Grease, ...	2,800	8 0 0	22,400 0 0
Cements, ...	12,000	2 0 0	24,000 0 0

"On Fractional Distillation." By Professor WANKLYN.

"On the Constitution and Rational Formula of Narcotine." By Dr MATTHIESSEN, and G. C. FOSTER.

"Report on the Metallurgy of the District." By Mr I. L. BELL, Mr T. SOPWITH, Dr RICHARDSON, and Mr T. SPENCER.

"On the Separation of Lead and Antimony." By Dr RICHARDSON.

"On Titanium in Iron." By Dr RILEY.—Titanium appeared in small cubical crystals, and had long been observed in the hearts of blast furnaces, being always found in those that were used for making the best grey iron. Titanium ought no longer to be considered one of the rarer elements. It occurred very generally, and was a universal constituent of all clays. Stourbridge bricks contained 1·05 per cent. of titanium, and that estimation was much too low, because it was a difficult substance to determine. In mining shales he had found titanium—in some up to 3 or 4 per cent. The object of the paper, however, was to prove that under certain conditions it formed a constituent part of pig-iron, and appeared to have some beneficial effects in the manufacture of iron and steel. It acted somewhat similarly to manganese.

"On Thallium." By Mr I. L. BELL.

"On the Analysis of Chinese Iron." By Dr STEVENSON MACADAM.

"On Deposit in Blast Furnaces." By Mr J. PATTINSON.—The deposit was of a dark grey colour, and was palpably fine powder. On analysis, it was found to contain as follows:—

Protoxide of iron, ...	14·22 per cent.
Oxide of zinc, ...	10·08 "
Sulphide of zinc, ...	13·70 "
Alumina, ...	8·20 "
Lime, ...	12·32 "
Magnesia, ...	5·03 "
Chloride of sodium, ...	4·74 "
Ammonia, ...	0·70 "
Thallium, ...	trace
Sulphuric acid, ...	3·18 "
Free sulphur, ...	0·17 "
Silica, ...	22·60 "
Carbonaceous matter, ...	4·50 "
	99·84 "

"On Zinc, Nickel, and Cobalt in Cleveland Ironstone." By Mr J. PATTINSON.—The author has estimated the amounts of nickel and cobalt contained in pig-iron, malleable iron, and puddling furnace cinder, all of which were produced from Cleveland ironstone, without any admixture of other ores. The samples were kindly supplied by Messrs. Bolckow and Vaughan, of Middlesbro'. In each case, four ounces of the sample were operated upon. They contained as follows:—

	Pig Iron.	Malleable Iron.	Puddling Furnace Cinder.
Grains of nickel per lb., . . .	1·88	1·56	0·313
Grains of cobalt per lb., . . .	0·32	0·24	0·062
Per-centage of nickel, . . .	0·027	0·022	0·0045
Per-centage of cobalt, . . .	0·004	8·003	0·0009

"On the Manufacture of Aluminium." By Mr ISAAC L. BELL.—The author gave an account of the usual mode of manufacture of aluminium, and pointed out the presence of impurities which materially interfered

with the manufacture of aluminium-bronze. These considerations have led manufacturers both here and in France to adopt the use of another raw material for the production of aluminium, which either does not contain the impurities referred to as so prejudicial, or contains them in such a form as to admit of their easy separation. This material is Bauxite, so called from the name of the locality where it is found in France. It contains—

Silica, ...	2·8
Titanium, ...	3·1
Sesqui-oxide of iron	25·3
Alumina, ...	57·4
Carbonate of lime, ...	0·4
Water, ...	10·8

The Bauxite is ground and mixed with the ordinary alkali of commerce, heated in a furnace. The metal is so extensively used in the arts as to keep the only work in England, namely, that at Washington, pretty actively employed. As a substance for works of art, when whitened by means of hydrofluorine and phosphoric acid, it appears well adapted, as it runs into the most complicated patterns, and has the advantage of preserving its colour from the absence of all tendency to unite with sulphur, or become affected by sulphuretted hydrogen. A large amount of the increased activity in the manufacture referred to is due to the exceeding beauty of its compound with copper, which is so like gold as scarcely to be distinguishable from that metal, with the additional valuable property of being nearly as hard as iron.

"Report on the Chemical Nature of Alloys." By Dr MATTHIESSEN.

"On the Extraction of Thallium from the Flue Dust of Pyrites Burners." By W. CROOKS, F.R.S.

"On the Impurities contained in Lead, and their Influence on its Technical Uses." By W. BAKER, F.C.S.

"On the Slaking of Quick-Lime." By Dr JOHN DAVY.

"On Molecular Motion." By Dr ZENNER.

"On the Analysis of Chinese Iron." By Dr STEVENSON MACADAM.

"Définir par la végétation l'état moléculaire des corps. Analyser la force végétale par des essais raisonnés de culture." By M. GEO. VILLE.

"Report on Synthetic Researches on the Formation of Minerals." By Mr ALPHONSE GAGES.

"On a New Method of Measuring the Chemical Action of the Sun's Rays." By Dr F. L. PHIPSON.—The writer happened to notice, lately, that a solution of sulphate of molybdic acid (that is, a solution of molybdic acid, or molybdic ammonia, in excess of sulphuric acid) standing in his laboratory upon a shelf which is partly exposed to the sun for about three hours each day, became bluish-green in the daytime and colourless at night. He repeated the experiment several times, and found that the saline solution when exposed to the sun was reduced, but when withdrawn from its influence, it gradually became colourless again by oxidation. No evolution of gas, however, was observed. During the insolation a certain amount of the molybdic acid loses one equivalent of oxygen, which combines with the water and forms oxygenated water, and during the night, the latter gives back its oxygen to the molybdic acid produced; the whole being in presence of an excess of sulphuric acid. Nothing is easier than

to measure with the greatest accuracy the amount of reduction which takes place by the action in a given time.

"On Musical Sounds produced by Carbon." By Dr F. L. PHIPSON.—The author has found that a fragment of homogenous wood charcoal, about a foot long and an inch thick, suspended by a string, and struck with a hammer, emits a beautiful musical sound, resembling that given out by glass and aluminium.

"On New Zealand Lignites." By Dr MURRAY THOMSON.

"On the Chemical and Physical Principles in Connection with the Specific Gravity of Liquid and Solid Substances." By Dr OTTO RICHTER.

Mr W. SYMONS exhibited a new form of Gas Battery.

"On Oxidation by Ozone." By Dr T. WOOD.

"On the Manufacture of Superphosphates, and Dissolved Bones." By Dr S. MACADAM.

"Recent Applications of the Hydrocarbons derived from Artificial and Natural Sources." By Dr B. H. PAUL.

"On the Uses of Fuel in Marine Boilers." By Dr RICHARDSON and Mr T. W. BUNNING.—It being understood that mixtures of Hartley and Welsh coal were being burnt on board the Imperial navy in France, some experiments were recently tried in the Government Yard in this country upon those mixtures. Dr Richardson and Mr Theo. W. Bunning were deputed to be present on the occasion. In order to register the amount of the smoke, which had hitherto been considered the chief disadvantage in the use of the Hartley coal, they adopted a series of figures to represent the different intensities of the smoke. Under this arrangement, it was found that while the "Hartley" made 340 marks of smoke and the "Welsh" 40, an equal mixture of the Hartley and the Welsh made only one mark. That was, there was only smoke of the lightest description issuing from the chimney for one minute, during the whole of the experiments, which lasted six hours. The coal was burnt in an ordinary boiler, used for making experiments on the different cargoes of coals applied to the yard; the coals were stoked by an ordinary Government stoker without any precautions, and the doors were of the usual closed description employed in Her Majesty's Navy. The result of this application would be to furnish a remedy for the great nuisance of smoke which existed in all ocean-going steamers, and a great saving would at the same time be effected.

"On Impurities in Lead." By Dr ZENNER.

"Sur les procédés de gravure du verre à l'aide de l'acide fluorhydrique par impression de la réserve." By M. L. KESSLER.

"Sur des appareils nouveaux évaporant à multiple effet et à air libre nommés *Érorateurs*." By M. L. KESSLER.

"Sur les avantages commerciaux d'un nouveau sel de soude cristallisé." By M. L. KESSLER.

"On Photoelectric Engraving, and Observations upon Sundry Processes of Photographic Engraving." By Mr DUNCAN C. DALLAS.

"Are Nitrogen and Carbonic Oxide the Oxide of Carbon in different Allotropic or Isomeric states?" By Mr H. KILGOUR.

"Report on Synthetic Researches on the Formation of Minerals." By Mr ALPHONSE GAGES.

"On the Minerals and Salts found in Coal Pits." By Messrs R. C. CLAPHAM and J. DAGLISH.

"Researches on the Manufacture of Prussiate of Potash by the late John Lee and T. Richardson." By Dr RICHARDSON.

"On the Constant Increase of Organic Matter in Cultivated Soils." By Dr F. L. PHIPSON.

SECTION C.—GEOLOGY.

President—Professor WARRINGTON W. SMYTH.

"On Coal, Coke, and Coal Mining in Northumberland and Durham." By Messrs N. WOOD, J. TAYLOR, J. MARLEY, and J. W. PEASE.

"On the Fossil Teeth of a Horse found in the Red Clay at Stockton." By J. HOGG, M.A.

"On the Coal Measures of Sydney, Cape Breton." By Mr J. P. LESLEY.

"On the Magnesian Limestone of the County of Durham." By Messrs G. B. FORSTER and J. DAGLISH.

"On the Skiddaw Slate Fossils." By Professor HARKNESS.

"On the Hornblendic Greenstones and their relations to the Metamorphic and Silurian Rocks of the County of Tyrone." By Professor HARKNESS.

"On Two New Coal-Plants from Nova Scotia." By Dr DAWSON.—One of the plants, *L. Acadianus*, belonged to the genus *Lepidophloios* of Sternberg; the other was an example of a type of *Lepidodendron*, very characteristic in Nova Scotia of the lower coal measures associated with the lower carboniferous limestone. The author concluded that the original species of Sternberg, *L. caricinum*, was founded on the fragment of the bark of an old trunk, having the leaf bases flattened, and hence described as scales. It was evidently, in short, closely allied to the specimen described. The genus *Ulodendron* was, he thought, identical with *Lepidophloios*, but apparently founded on specimens having the leaf bases preserved, with the cone scars, but wanting vascular scars; but he was in doubt as to the claims of the name *Ulodendron* on the ground of priority. It appeared to him that the generic names *Ulodendron*, *Lomatophloios*, *Leptoxylum*, *Pachyphloeus*, and *Bothrodendron* should be abolished in favour of *Lepidophloios*, unless indeed it should appear that any of these names had priority in date. The second plant described was the *Lepidodendron corrugatum*, which was one of the most abundant in the lower coal measures of Nova Scotia and New Brunswick. The species was remarkable for its variability, and also for the dissimilar appearances of old stems and branches occasioned by the separation of the areoles in the growth of the bark, instead of the areoles themselves increasing in size, as in some other *Lepidodendra*.

"On Models illustrating the Contortions in Mica Schist and Slate." By Mr H. C. SORBY.

"On a Deposit of Sulphur in Corfu." By Professor ANSTED.

"On the Metamorphic Origin of the Porphyritic Rocks of Charnwood Forest." By Professor ANSTED.—The part of England referred to is a small district in the county of Leicester, east of the various other developments of igneous rock in England. The object of this paper was to show that the rocks, consisting of syenites, granites, and others of the appearance and character generally regarded as igneous, were really of the same date as the slates with which they seem to alternate.

"On the Laurentian Rocks in the Malvern Hills." By Mr E. HOLL.

"On the Equivalents of the Cleveland Hill Ironstones in the West of England." By Mr C. MOORE.

"On the Organic Contents of the Lead Veins of Allenheads and of Yorkshire." By Mr C. MOORE.

"Report on the Distribution of the Organic Remains of the North Staffordshire Coal-field."—Read by Mr MULLINS.

"On the Chronological Value of the Triassic Rocks of Devonshire." By Mr WILLIAM PENGELLY.—The red rocks of Devonshire are eminently detrital, and consist of conglomerates, sandstones, and marls, and belong to the Bunter or Lower Trias. They occupy the whole of the country east of a line from Torbay to Porlock in West Somerset. The general dip is about $15\frac{1}{2}$ degrees in the direction N. and $44\frac{1}{2}$ degrees E. (true), so that, judging from this horizontal extension in this direction, their volume must be enormous. The conglomerates and sandstone, in every layer, afford evidence of their littoral origin, and of their having been deposited on a tidal strand undergoing slow subsidence.

"On the Causes of Earthquakes and Volcanic Eruptions." By Mr J. ALEXANDER DAVIES.

"On the Physical Condition of the Earth in the Earlier Epochs of its History." By the Rev. JAMES BRODIE.

"On a Help to the Identification of Fossil Bivalve Shells." By Mr HARRY SEELEY.

"On the Penine Fault." By Mr W. BAINBRIDGE.

"On Coal in the Red Measures." By MATTHIAS DUNN, Government Mine Inspector.—After noticing generally the Cumberland Coal-fields and the New Red Sandstone, Mr Dunn said—I have been led to form the idea that the Red Sandstone is but the superior strata of the coal field, and that the neighbouring coal seams will be *found underneath*; and judging from the flatness of the country around Silloth Harbour, I have assumed that the bottom of the basin will exist in that quarter, for appearances of a coal outcrop exist in the neighbourhood of the Criffle mountains in Kirkcudbright. In pursuance of this theory, I assume that the coal-field of Canobie is similarly circumstanced, for the Red Sandstone there crops out contiguous to the pits, and the nature of the various seams of coal assimilate closely upon those of Cumberland. Another corresponding fact attends the colliery of Kirkhouse belonging to the Earl of Carlisle, although a portion of it also contains coals belonging to the Limestone formation.

"On the Recent Discovery of Gold near Bala Lake (Llyn Tegd), Merionethshire." By T. A. READWIN, F.G.S.

"On the Origin of Granite." By Mr ALEXANDER BRYSON.

"On the Deposit of the Gravel, Sand, and Loam, with Flint Implements at St Acheul." By Professor PHILLIPS.—One of the remarkable fruits of geological investigation was to invest almost every point on the earth's surface with a new interest. The small French village named had long been remarkable for the school of the Jesuits established there; but antiquarians had discovered that it was near a burial ground of great antiquity. In the course of excavation there were discovered the graves of people far more ancient than any known to have been buried there. Other memorials were also discovered; and on one he had obtained from the workmen he read the name of Constantius. A stone coffin was found,

and also an armlet which had been placed on the arm of a buried person. When they looked in front of the great face of excavation and saw overhead the Jesuit College, the ancient Cemetery, and the Roman and Pre-Roman graves, the question arose, "What could be the antiquity of the sand and gravel-deposit at the lower level?" In Sir Charles Lyell's recently published volume the situation was fully described. Concerning the deposits, there was no difference of opinion; they were to be reckoned among the later deposits of the geological time, and in the lower parts of these deposits a great number of interesting implements had been obtained, and some of these he exhibited. He described the deposits in detail, from illustrations, stating that fresh-water and land shells were found in sand, and splintered flints in an argillaceous deposit over it. For the fresh-water and land shells in the gravel it was not necessary to appeal to the action of the sea, which, however, was seen in the lower part of the level. There were, in different levels, cases of great agitation of water, comparative agitation, and comparative tranquillity. They might imagine a lacustrine deposit, against which there would be the objection that it would not produce gravel in such a form, it being twisted about in all ways. There ought to have been found lying parallel to the surface of the lake a great number of lacustrine shells; but that was not the case, and the explanation would not apply to the mixture of fresh-water and land and amphibious shells. The more ordinary explanation was to suppose the action of a river which had changed its position, so that the flint instruments found near the bottom might formerly have existed near the top. The arrangement of the sands was obviously of such a kind that they floated over the pebbles and covered all below. The whole question came finally to this:—Could they determine the age of the gravel beds? They could not escape the conviction that the flint instruments were of the same age as the gravel beds. Upon the supposition of strata having been deposited by river action, the upper surface of the deposits would continually tend to become level, and would be so when the deposits were of an argillaceous nature. In this case the slope varied from $2\frac{1}{2}$ to $1\frac{1}{2}$ degrees. In order to account for the present condition of things, it would be necessary to suppose that the country had been disturbed, and that there had been an elevation affecting the valley of Somme. On an examination of the locality, they would speedily arrive at the impression that it was requisite to remember that there was no period of geological history from which it was safe to exclude a movement of the earth's crust. The map of France showed the causes of the elevation. The rivers ran in parallel lines across the chalk, and it was impossible to separate the circumstance from the similar fact in this country where these phenomena had been discovered. As there was reason to think that the valley had been subject to upheaval, accepting the supposition, they would not be able to determine the question of age by the excavation of the river. If they followed the suggestion of Sir C. Lyell, and took their measure from Scandinavia, they might come to some determination as to time; but this was a case of a local disturbance of the earth's crust, affecting certain lines of country in a given direction, and apparently ceasing beyond that. As it would be to some purpose to ascertain the antiquity of these deposits, he trusted Sir C. Lyell would not think it otherwise than a compliment to hear an opinion differing from his own.

"On the Drift Beds at Mundesley, Norfolk." By Professor PHILLIPS. —Professor Phillips made a statement respecting the drift beds at Mundesley, Norfolk, remarking that he did so to confirm some views which

were of the greatest importance in reasoning with regard to the antiquity of mankind, and at the same time of suggesting a mode of consideration which he hoped could be followed up. The district on the coast of Norfolk, where the cliffs belong to the glacial, postglacial, and preglacial periods, had become famous owing to the investigations of Mr Taylor a few years ago. Some thirty years ago in Yorkshire, below the boulder clay, there was found a quantity of flint and chalk gravel which contained the bones of elephants, horses, and other creatures. Soon afterwards he became acquainted with this similar discovery in Norfolk. Having described these deposits, the Professor said he was inclined to think they must not venture to apply to this country any argument drawn from Scandinavia. They must have each country studied for itself, and then they might possibly arrive at a satisfactory conclusion. It was far better to take each class of glacial deposits by itself. He thought it possible to account for these deposits by the introduction of the tide at different levels, and that it was not at all necessary to suppose that the coast had been disturbed in order to account for the level of the marine shells. He was inclined to think that all those strata were to be put together as the deposit of one period; and he thought Norwich Crag was too local a name to apply to so remarkable a set of deposits.

"On the Alluvial Accumulations in the Valleys of the Somme and Ouse." By Mr R. A. GODWIN-AUSTEN.

Sir C. LYELL said he had expected to hear Professor Phillips and Mr Godwin-Austen express a wider divergence from his own conclusions than they had done. He took it for granted that Professor Phillips agreed with him in the important point, that not only the flint implements which he mentioned in the case of St Acheul were of the same age as the old river gravel, but also the extinct mammalia. It therefore appeared that they agreed in the important point of the co-existence of man with those extinct animals. The new view which he had attempted to explain was, that the upper valley gravel, some 80 or 100 feet above the level of the sea, was not now in the position it was when the river flowed there and formed this extensive deposit of sand and gravel. If he understood the argument, there was such a slope of the gravel covered with loam towards the Somme as there would not be if it was the deposit of a considerable river in its original state; in that case the slope would be the other way, from the river towards the bluffs, as in the case of the Rhine and the Mississippi. He was not prepared to say whether it was possible to calculate on the identity of the present state of that surface with what it was at the very remote period when it was formed, and since which it must have had so many washes by rain during many thousand years. He was not prepared to say whether they could reason in that manner as to a change of position. What he said was, that there was nothing in his speculations on the river gravels hostile to the conclusions which Professor Phillips had proposed, of there having been possible local movements, or, at any rate, a considerable movement of that country since the old river flowed. He thought it was almost impossible that that should not be the case. Indeed, when he found two levels of river gravel, one higher and the other lower, it generally appeared to him that that must be in consequence of some great movement, that there must have been probably some stationary period, when great accumulations took place, and that there must have been a period of movement, the waters eroding and cutting away the country, until they settled down at a lower level, and there was a formation of gravel there. This was a most probable thing; but they must bear in mind that though they talked of these

appearances at two different levels, there were occasionally intermediate levels and deposits of gravel even higher than St Acheul. It would be difficult to suppose that it was always strictly at two levels that these gravel beds occurred; but there was a prevalence of them at a higher level and at a lower level, that lower level being necessarily higher than that of the present Somme. He therefore had no objection to suppose that, after the country had been for some time in that state at which the gravels and sand were formed, there was some movement or elevation during which the river was able to cut the land down, and then form the inferior or lower level gravels; and it did not appear to him that if that view were adopted it made any very essential difference. Professor Phillips thought it made this difference—that the time would be much shorter if there were such a movement, and certainly it would; but he could hardly conceive any movement would enable the river to destroy so much older strata, as it must have destroyed to produce such reiterated river beds. If Professor Phillips could bring evidence of such a movement it would be a great assistance; but that would not alter at all any views at which Mr Prestwich and himself had arrived with regard to the manner in which the higher and the lower levels were formed. There were other proofs besides the fresh-water shells and the absence of marine animals of the fluvial origin of the St Acheul gravels. The gravel in the Somme, the Seine, and their tributaries was composed of rock that belonged to the hydrographical beds of those rivers. In addition there was the presence of fluvial shells as well as of land animals. He could receive the views of Mr Prestwich that these gravels were remains of an old river; and he could admit that there might have been such a movement as Professor Phillips had supposed. Mr Austen, in speaking of the Bedford section, had endeavoured to do away with the argument in favour of the antiquity of man by supposing that the remains of extinct lions, rhinoceroses, and other animals, taken out of the gravel, which was about thirty feet above the level of the sea, were derived from an older gravel. He supposed some pre-existing formation, out of which the bones were taken, and then deposited in the present, so that that formation which contained the flint instruments would not be proof of the co-existence of man with those extinct mammalia, and that the mammalia existed before and were washed out into the beds containing the flint instruments. Such an objection might be made to almost every river bed, because rivers were constantly ploughing up their channels—doing and undoing. Therefore, if any animal remains had sunk in the channel, the chances were that they would be torn out again and rolled on before they got to their final resting-place. It was perfectly true that in some of our valleys, such as the Severn, the old drift containing distinct animals will be undermined, and occasionally bones in a state of integrity will be thrown down into the new river bed. There were such cases, and they were guarded with respect to them; but as a general rule, if they found remains buried in gravel, the inference was that they were formed during that long period when that ancient growth was deposited, bed after bed, and sometimes partly destroyed and re-deposited. If a geologist wished to draw a contrary conclusion, he was bound to show, first of all where was the old formation out of which these extinct bones were derived. To make out his theory, he would be bound to show that such a formation was under the drift of that country; which, however, was not the case. Under the circumstances, the hypothesis seemed a violent one, formed to get rid of a violent conclusion, to suppose that these bones had been derived from some other formation that existed in the neighbourhood, without a shadow of evidence of there having been such a one, and with all the existing evidence against it. He hoped the

conclusion was one which, on reconsideration, Mr Austen would not continue to maintain.

"On certain Markings on the Horns of *Megaceros Hibernicus*." By Mr J. B. JUKES.

"On the Discovery of Elephant and other Mammalian Remains in Oxfordshire." By Mr G. E. ROBERTS.—Part of the bones have been submitted to Dr Falconer, who has recognised *Elephas primigenius* of the Siberian tribe, teeth and other remains rather abundant; *Elephas antiquus*; a large species of *Bos*—(*primigenius?* or *priscus?*) top of radius, tibia, and horn core; many bones and teeth of *Equus caballus fossilis*, including a finely preserved tibia of great size, and a portion of another still larger; and some good fragmentary specimens of the horns of *Cervus elephas*.

"Some Facts relating to the Hydrography of the St Lawrence, and the Great Lakes." By Dr HUELBURT.

"Report on the Chemical and the Mineralogical Composition of the Granite of Donegal." By a Committee, consisting of Mr SCOTT, Sir R. GRIFFITH, and Professor HOUGHTON.

"On the Origin of the Jointed Prismatic Structure in Basalts and other Igneous Rocks." By Professor JAMES THOMSON.

"On a Synopsis of the Bivalved Entomostraca of the Carboniferous Strata of Great Britain and Ireland." By Professor T. RUPERT JONES and Mr J. W. KIRKBY.

"Notes on some Fossil and recent Foraminifera, collected in Jamaica, by the late Lucas Barrett, F.G.S." By Professor T. RUPERT JONES and Mr W. K. PARKER.

"On some Fossil Fishes from the Permian Limestone of Fulwell, near Sunderland." By Mr J. W. KIRKBY.

"Report of the Shetland Dredging Committee, in its Geological bearings." By Mr J. GWYN JEFFREYS.

"A List of the Upper Tertiary Fossils of Uddewalla in Sweden." By Mr J. GWYN JEFFREYS.

"On the Upper Tertiary Strata of the Bohuslän District." By Dr A. W. MALM.

"On a Wash or Drift through the Coal-Field of Durham." By Mr NICHOLAS WOOD and Mr EDWARD F. BOYD.

"Observations on the Permians of the N.W. of England." By Sir RODERICK I. MURCHISON and Professor HARKNESS.

"On a Salamander in the Rothengendes." By Dr GEINITZ.

"On the Reptiliferous and Footprint Sandstones of the N.E. of Scotland." By Professor HARKNESS.

"On some Remains of *Bothriolepis* from the Upper Old Red Sandstone of Elgin." By Mr G. E. ROBERTS.

"On the Occurrence of Rock Salt at Middlesbro." By Mr JOHN MARLEY.—He stated that a bed of salt, 100 feet thick, had been already bored.

"Description of a Sea Star (*Cribellites carbonarius*) from the Mountain Limestone of Northumberland, with a Notice of its association with Carboniferous Plants." By Mr GEORGE TATE.—Previously no species of *Asteroides* had been recorded from this formation. The specimen exhibited, found by Mr W. Wilson of Shilbottle, was an impression of the



upper surface only of the organism, in a yellow, fine-grained micaceous sandstone, and, although imperfect, yet being doubtless a sea star, and the first discovered in the formation, it deserved the attention of palaeontologists. The following characters could be observed:—Rays five, rounded, lanceolate, five times as long as the disk, ridged in the centre, covered with longitudinal rows of reticulating tubercles; disk small and tuberculated. The disk was only $\cdot 3$ of an inch in diameter, whilst the rays were 1·5 inch in length. The sandstone from which this asteroid was obtained was about twenty feet above the Shilbottle coal, and about ten feet below the “18-foot limestone,” which was the fifth limestone sill in the mountain limestone of Northumberland; it was, he estimated, about 600 feet below the base of the mill-stone grit; and as the formation was about 3000 feet in thickness, it was in the upper part of this series of beds.

“Some Facts observed in Weardale.” By Mr CHARLES ATTWOOD.

“On a Section of the Strata from Howneshill to Cross Fell.” By Mr T. SOPWITH.

“On the Neanderthal Skull, or Reasons for believing it to belong to the Clydian Period, and to be specifically distinct from Man.” By Professor WILLIAM KING.

“On some Fish Remains that have occurred in the Coal Measures of Durham and Northumberland.” By Mr THOS. ATHEY and Mr JAMES W. KIRKBY.

SECTION D.—ZOOLOGY AND BOTANY.

President—Professor BALFOUR.

Mr J. G. JEFFREYS read the “Report of the Committee appointed for Exploring the Coast of Shetland by Means of the Dredge.”—After explaining the nature of such researches, as well as their utility in a zoological and geological point of view, the author gave a short history of the marine invertebrata of the Shetland seas during the last half-century. He described the result of the expeditions which he had undertaken to this remote district since 1841. Several zoological problems of real interest were then discussed; viz. the colour of animals living at great depths,—the condition and composition of the sea-bed,—the idea that certain species were now “dying out,” which the author opposed,—the distribution of certain species,—the absence of the Gulf Stream on the eastern and northern coasts of Shetland,—the gradual sinking of the sea-bed since the glacial epoch,—the lively state of mollusca taken from the deep, and kept in a shallow vessel filled with sea-water from the beach,—and also the improbability of any marine strata being unfossiliferous except when they have been subjected to chemical action.

“On the Cultivation of Cinchona in India.” By Mr C. R. MARKHAM.

“On the Zoology of Hylton Dene, near Sunderland.” By Mr G. S. BRADY.

“On the Marine Cyclopoid Entomostraca (Calanidæ), with Notices of some Species new to Britain.” By Mr G. S. BRADY.

“Descriptions of New British Polyzoa, with Remarks on some imperfectly known Species.” By Mr J. ALDER.—The species forming the subject of this communication belong chiefly to the branched calcareous forms of the genera *Cellepora* and *Eschara*. The new species described were named *Cellepora lavigata*, *Eschara ligulata*, and *Palmicellaria*

elegans, the latter also a new genus, proposed for a beautiful little coral, dredged this year in Shetland by the Rev. A. M. NORMAN. The species remarked upon included the *Eschara levii* of Fleming, a species lost sight of by British naturalists for many years; *Eschara Landsborovi*, now first ascertained to be an *Eschara*, but the imperfect state of which had been described by Dr Johnston as a *Lepralia*: this was found on the coast of Northumberland by Mr Embleton; *Quadracellaria gracilis* of Sars, previously described from an imperfect specimen under the name of *Onchopora borealis*, by Professor Busk; *Scrupocelaria Delitii*, a species new to Britain, got on the Northumberland coast; and *Hornera borealis*, Busk, now introduced as British for the first time.

"On the Colour of the Salmon." By Dr DAVY.—This paper appears in the present Number of this Journal.

"On the Structure of the Fruit of *Clerodendron Thomsonæ* (Balf)." By the PRESIDENT.

"On some Elucidations of the Geological History of North Africa, supplied by its Lacustrine Fauna." By the Rev. H. B. TRISTRAM.

The Rev. A. M. NORMAN read the second part of the "Report of the Committee appointed to Dredge the Shetland Seas."—The first part of the Report, which was read on the previous day, had reference solely to Mollusca, while the second treated of Crustaceans and their Parasites. Mr Norman added a description of a great number of caves in a firth called Burrough Firth, to the extreme north of Shetland. These caves, which are the most beautiful in the British islands, possess the greatest interest to the naturalist. They are situated in rocks, shelving at an angle to the south-west, and marked by regular strata of various colours, and the interior of the caves is rendered brilliant by millions of zoophytes of every colour.

Captain WOODALL gave Reports of the Results of a Three Weeks' Dredging Cruise off Scarborough in 1863, by Mr Leckenby, which was undertaken upon the promises afforded by an examination last year, that it might be possible to define more satisfactorily than had yet been done, the area, extent and description of the invertebrates on the north-east coast of England. The examinations were made within ten miles of the shore, in from twenty to twenty-five fathoms, and near Flamborough Head, and the cost of the expedition was only about L.30. Cannon-balls were carried to weight the dredger in case the weather freshened, and the author recommended that the same precaution should be adopted in other similar expeditions.

"An Account of the Attempts to Transport Salmon to Australia." By Mr T. JOHNSON.—It was shown that ova might be kept in ice for ninety or more days, and afterwards produce young salmon.

"On the Roman and Imperial Crested Eagles." By Mr J. HOGG.

"On a New Species of Ione." By Mr C. S. BATE.

"Note on certain Influences regulating the Forms of Leaves," &c. By Dr M. T. MASTERS.

"Notes on Canadian Forests." By Dr HURLBURT.

"On the Syndactylous Condition of the Hand in Man and the Anthropoid Apes." By Mr C. CARTER BLAKE.

"Notes on the Occurrence of Foraminifera new to the British Seas." By Mr H. B. BRADY.—After some preliminary remarks on the various methods of separating recent *foraminifera* from bulky material, the

author proceeded to read notes upon the following forms not hitherto recorded as British, which had, with one exception, resulted from the examination of sands dredged by Mr Gwyn Jeffreys and Mr Waller round the Shetland Islands—the exceptional case, *Pulvinulina Menardii*, D'O., having been found in soundings from the Irish Sea:—

Biloculina sphaera, D'O.; *Biloculina contraria*, D'O.; *Triloculina tricarinata*, D'O.; *Quinqueloculina pulchella*, D'O.; *Lituola scorpiurus*, Montfort; *Lagena distoma*, P. and J.; *Glandulina laxiagta*, D'O.; *Bigenerina digitata*, D'O.; *Bolivina punctata*, D'O.; *Planorbulina Haidingerii*, D'O.; *Planorbulina Ungeriana*, D'O.; *Pulvinulina concentrica*, P. and J.; *Pulvinulina Karsteni*, R.; *Rotalia orbicularis*, D'O.; *Discorbina Berchelotiana*, D'O.; *Anomalina coronata*, P. and J.; *Polystomella crispa* var. *arctica*, P. and J.; *Nonionina stellifera*, D'O.; and *Pulvinulina Menardii*, D'O.

“Notes on some recent Foraminifera, dredged at Jamaica by the late Lucas Barrett, F.G.S.” By Professor T. RUPERT JONES and Mr W. K. PARKER.

“Notes on the Homologies of the Trilobites.” By Mr. C. SPENCE BATE.

“On the Geographical Distribution of Animal Life.” By Mr A. R. WALLACE.—The author called attention to the six geographical regions established by Dr Sclater (Proc. Lin. S., Feb. 1856) for ornithology,—viz., 1st, The Neotropical, comprising South America and West Indies; 2d, The Nearctic, including the rest of North America; 3d, The Palearctic, composed of Europe, Northern Asia to Japan and Africa, north of the Desert; 4th, The Ethiopical, which contains the rest of Africa and Madagascar; 5th, The Indian, containing Southern Asia and the western half of the Malay Archipelago; and 6th, The Australian, which comprised the eastern half of the Malay Islands, Australia, and most of the Pacific Islands. It was stated that these regions would apply almost equally well to mammalia, reptiles, land shells, and insects,—excepting some exceptional cases, which it was thought would render these regions inapplicable to zoology generally. These exceptional cases were—1st, That the Batrachians of Japan were Palearctic, agreeing with the birds, &c., but the snakes were altogether Indian, as pointed out by Dr Gunther in his paper on the geographical distribution of reptiles (Proc. Zool. Soc. 1858, p. 373); 2d, That the mammalia of North Africa were not European like the birds; 3d, That the insects of the Moluccas and New Guinea were generally of Indian forms, while the birds and mammals were Australian; and 4th, That the insects of Chili were of North Temperate and Australian forms, while the birds and mammals were mostly of true South American groups. These cases were treated successively; and it was shown that the statement as to the mammals of North Africa was incorrect, and that they really very strongly confirmed the evidence of the birds and reptiles as to that country being Palearctic. In the other cases, the anomalies of distribution were explained as being due to special exceptional circumstances, which should not invalidate the general accuracy and usefulness of these divisions. The discrepancies in the distribution of plants, which, while often agreeing with those of insects, were much greater, were supposed to be in a great measure due to the adventitious action of the glacial epoch and of floating ice.

“A Few Facts on the Variation of Species pointing to Western Asia as the Centre of the Palearctic Area of Creation.” By the Rev. H. B. TRISTRAM.—No one would suppose that the creation or development of

animals had gone on simultaneously all over one region, so as to exclude the idea of animals or insects first appearing in some particular centre under certain circumstances, and thence gradually extending themselves wherever the situation was best adapted to their habitation. His idea was, that they could trace through the whole of the Palearctic region, from Iceland to Japan, some predominant forms, varying in greater or less degrees, and that whilst the variations were generally longitudinal, the identical forms were preserved latitudinally. Amongst the instances he quoted were the grey wagtail, the yellow wagtail, the green woodpecker, and the azure-winged magpie. The universality of some species did not, he thought, tell much against his theory, because those which did occur universally, or almost universally, as the snipe and osprey, had such powers of locomotion that they could from one centre disperse themselves in all directions. In regard to land shells, the same rule appeared to apply. Of course, in shells it was very difficult to ascertain the whole of the species to be discovered in the Caucasian and North Persian district, because it had not been much worked; but taking the western region, there was not a single shell in Ireland which was not to be found in England, and not one in England that was not, with some few exceptions, to be found in Germany; but the number of species in England more than doubled the number in Ireland, and the number in Germany more than doubled the number in England. Thus they found the species largely increasing as they went towards the East, wherever there was sufficient lime for the creatures to form their shell; and while very few of our English species were lost, new ones were continually found. What he wished to submit was, that the variation of species, taking the instances of some of our most familiar birds, the general geography of zoology pointed out, that in the Palearctic region, Central Asia was the centre from which they had passed eastward and westward.

"Description of a New Plant-house." By Mr JAMES BEWLEY.

"A Brief Account of the Vegetation of the Cliffs of Mohir, County Clare." By Mr N. B. WARD.

"On the Occurrence of the Sperm Whale (*Physeter macrocephalus*) at Wick." By Mr CHARLES W. PEACH.

"Notice of a Monstrosity in a Whiting." By Mr C. W. ROSE.—The whiting had three eyes, two in their natural position and one between the two.

"On the Physical Geography of the Malay Archipelago." By Mr A. R. WALLACE.—It first becomes necessary to define accurately the limits of the Archipelago, pointing out exactly what islands we include within it; for, though "all the islands between south-eastern Asia and Australia" seem pretty definite, yet to the eastward this region blends insensibly into the vast extent of the Pacific Islands. According to my views, the Malay—or, as I should prefer to name it, the Indo-Australian—Archipelago extends from the Nicobar Islands on the north-west to St Christoval, one of the Solomon Islands, on the south-east, and from Luzon on the north to Rotti, near Timor, on the south. The eastern boundary is drawn at this particular point for reasons which will be explained further on. Though not geographically correct to include any part of a continent in an archipelago, it is necessary for our purpose to consider the Malay peninsula as not only almost but quite an island, since it cannot be physically separated from the region of which we are now treating. Thus limited, the Archipelago is of a somewhat triangular form, with an extreme length of about 5000, and breadth of rather more than 2000

English miles. Looking at a map on which the volcanic regions of the Archipelago are marked out—those which are subject to earthquakes which are of volcanic origin, and which abound more or less in extinct as well as active volcanoes,—we see at a glance that the great islands of Borneo and Celebes form the central mass around which the volcanic islands are distributed, so as rudely to follow their outline and embrace them on every side but one in a vast fiery girdle. Along this great volcanic band (about 5000 miles in length) at least fifty mountains are continually active, visibly emitting smoke or vapour; a much larger number are known to have been in eruption during the last 300 years; while the number which are so decidedly of volcanic origin that they may at any moment burst forth again, must be reckoned by hundreds. It is not now my object to describe the many fearful eruptions that have taken place in this region. In the amount of injury to life and property, and in the magnitude of their effects, they have not been surpassed by any upon record. Forty villages were destroyed by the eruption of Papandayang in Java, where the whole mountain was blown up by repeated explosions, and a large lake left in its place. By the great eruption of Tomboro in Sumbawa, 12,000 people were destroyed, and the ashes darkened the air, and fell thick upon the earth and sea for 300 miles round. Even quite recently, since I quitted the country, a mountain which has been quiescent for more than 200 years suddenly burst into activity. The island of Makian, one of the Moluccas, was rent open in 1646 by a violent eruption which left a huge chasm on one side, extending into the heart of the mountain. It was, when I last visited it, clothed with vegetation to the summit, and contained twelve populous Malay villages. On the 29th of December 1862, after 215 years of perfect inaction, it again suddenly burst forth, blowing up and completely altering the appearance of the mountain, destroying the greater part of the inhabitants, and sending forth such volumes of ashes as to darken the air at Ternate, 40 miles off, and almost entirely to destroy the growing crops on that and the surrounding islands. The island of Java contains more volcanoes, active and extinct, than any other known district of equal extent. They are about forty-five in number, and many of them exhibit most beautiful examples of the volcanic cone on a large scale, single or double, with entire or truncated summits, and averaging 10,000 feet high. It is now well ascertained that almost all volcanoes have been slowly built up by the accumulation of the matter—mud, ashes, and lava—ejected by themselves. The openings or craters, however, frequently shift their position; so that a country may be covered with a more or less irregular series of hills in chains and masses, only here and there rising into lofty cones, and yet the whole may be produced by true volcanic action. In this manner the greater part of Java has been formed. The great island of Sumatra exhibits in proportion to its extent a much smaller number of volcanoes, and a considerable portion of it has had probably a non-volcanic origin. Going northward, Amboyna, a part of Bouru, and the west end of Ceram, the north part of Gilolo and all the small islands around it, the northern extremity of Celebes, and the islands of Siau and Sanguir are wholly volcanic. The Philippine Archipelago contains many active and extinct volcanoes. In striking contrast with this region of subterranean fires, the island of Celebes in all its southern peninsulas, the great mass of Borneo, and the Malay peninsula, are not known to contain a single volcano, active or extinct. To the east of the volcanic band is another quiescent area of 1000 miles wide, the great island of New Guinea being free from volcanoes and earthquakes. Towards its eastern extremity,

however, these re-appear in some small islands off its coast, and in New Britain, New Ireland, and the Solomon Islands, which contain active volcanoes. The contrasts of vegetation and of climate in the Archipelago may be best considered together, the one being to some extent dependent on the other. Placed immediately upon the Equator, and surrounded by extensive oceans, it is not surprising that the various islands of the Archipelago should be almost always clothed with a forest vegetation from the level of the sea to the summits of the loftiest mountains. This is the general rule. Sumatra, New Guinea, Borneo, the Philippines, and the Moluccas, and the uncultivated parts of Java and Celebes, are all forest countries, except a few small and unimportant tracts, due perhaps, in some cases, to ancient cultivation or accidental fires. To this, however, there is one important exception in the island of Timor, and all the smaller islands opposite, in which there is absolutely no forest such as exists in the other islands, and this character extends in a lesser degree to Floris, Sumbawa, Lombock, and Bali. The changes of the monsoons and of the wet and dry seasons in some parts of the Archipelago are very puzzling; and an accurate series of observations in numerous localities is required to elucidate them. Speaking generally, the whole south-western part of the Archipelago, including the whole range of islands from Sumatra to Timor, with the larger half of Borneo and the southern peninsula of Celebes, have a dry season from April to November, with the south-east monsoon. This same wind, however, bends round Borneo, becoming the south-west monsoon in the China Sea, and bringing the rainy season to Northern Borneo and the Philippines. In the Moluccas and New Guinea the seasons are most uncertain. In the south-east monsoon from April to November, it is often stormy at sea, while on the islands it is very fine weather. There is generally not more than two or three months of dry hot weather about August and September. This is the case in the northern extremity of Celebes and in Bouru, whereas in Amboyna, July and August are the worst months in the year. In Ternate, where I resided at intervals for three years, I never could find out which was the wet and which the dry season. The same is the case at Banda, and a similar uncertainty prevails in Menado, showing probably that the proximity of active volcanoes has a great disturbing meteorological influence. In New Guinea a great amount of rain falls more or less all the year round. On the whole, the only general statement we can make seems to be, that the countries within about 3° on each side of the Equator have much rain and not very strongly contrasted seasons; while those with more south or north latitude have daily rains during about four months in the year, while for five or six months there is almost always a cloudless sky and a continual drought. He next considered the Malayan Archipelago in its geological and zoological relations to Asia and to Australia, mentioning the well-established fact, that one portion of it is almost as much Asiatic in its organic productions as the British Isles are European, while the remainder bears the same relation to Australia that the West India Islands do to America.

“On the Irruption of *Syrnhaptes paradoxus*.” By Mr A. NEWTON.—These birds, which are commonly known as Pallas' sand grouse, and which are of Chinese origin, have made recent visits to this country, but have been rapidly exterminated or driven away. It appeared that about 109 of these rare birds had been killed in the British Isles, of which 63 were shot in Norfolk and Suffolk.

“On the Generic Characters furnished by the different Modes of Mining
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Leaves adopted by the Larvæ of *Micro-Lepidoptera*." By Mr H. T. STAINTON.

"On British Holothuriadæ with reference to New Species." By Rev. A. MERLE NORMAN.

"On the Morphology of the Echinodermata of the Family Ophiuridæ." By Rev. A. MERLE NORMAN.

"Report on the Natural History of the Island Formosa." By Mr ROBERT SWINHOE.

"Report on the Mollusca of California." By Mr P. P. CARPENTER.—This consisted of corrections of the first report, and a *resumé* of the fresh sources of information which had accumulated since its date. The most important results of recent investigation were the geological evidence of a recent connection between the waters of the West Indies and the Pacific, and also of the comparatively recent elevation of many parts of the Rocky Mountains, and the probable connection of the North Pacific and North Atlantic during the Eocene and probably the Miocene periods.

"On the Great Division of the Pacific Ocean Fauna." By Mr W. HARPER PEASE.—The connection of the Hawaiian Islands with the northern part of Japan by a series of sunken islands, identical in fauna with the Hawaiian, and the non-existence of certain islands appearing on the charts between that group and the coast of America, were dwelt upon with reference to the peculiarities of the existing fauna.

"On the Proliferous Cones of the Common Larch." By Mr JOHN HOGG.

"List of Rarer Phænogamous Plants in the South-east of Durham discovered since 1829." By Mr JOHN HOGG.

SUB-SECTION D.—PHYSIOLOGY.

President—Professor ROLLESTON.

"Notes on certain Parts of the Anatomy of a young Chimpanzee." By Dr EMBLETON.

"Observations on the Eggs of Birds." By Dr DAVY.

"On the Investigation of Instinctive Actions." Dr W. MURRAY.

"On the Ventilation of Barracks and other Public Buildings in India." By Mr S. CLARK.

"Report on the Physiological Effects of the Bromide of Ammonium." By Dr G. D. GIBB.—The general conclusions may be stated as follow:—
1. In small doses more or less long continued, bromide of ammonium acts as a tonic and absorbent, and exerts its peculiar properties upon the skin and mucous membrane. 2. It diminishes the weight of the body in polysarcia, causing the absorption of fat, when combined with a regulated diet; and this is effected with greater certainty than by any other known substance. 3. It improves the intellectual powers, increases the bodily capacity, and promotes healthy function. 4. Locally, it possesses a soothing influence on the mucous membrane, and, according to the strength and mode of its application, so does it diminish sensibility. 5. In large frequently repeated doses, or given at intervals, it influences the entire mucous tract; it affects all the special senses, and produces anæsthesia or impaired sensibility of the various mucous outlets. 6. All the poisonous effects are produced by very large doses, as from the bromide of potassium, but in smaller doses it is more certain and reliable, causes no

diarrhoea or diuresis, nor anaphrodisiasis, and its special properties are exerted sooner and with less inconvenience.

"On the Physiological Properties of the Nitrate of Amyle." By Dr B. W. RICHARDSON.—The nitrate when inhaled produces an immediate effect on the heart, increasing the action of the organ more powerfully than any other known agent. As the action of the heart rises, the surface of the skin becomes red, and the face assumes a light crimson colour. Carried to an extensive degree, the nitrate excites the breathing, and produces a breathlessness like that caused by sharp running or rowing. On animals, when the agent is given in large doses, it produces death. The most remarkable effect produced by the nitrate was, that in the lower animals and frogs it led to suspended animation, which could be maintained for so long a time as nine days with perfect after-recovery.

"On the Blood in Relation to the Question, Is Ammonia one of its Normal Constituents?" By Dr DAVY.—The conclusions he arrived at are the following:—1. That they are confirmatory of the inference that the coagulation of the blood is not owing to the escape of ammonia. 2. That they are favourable to the conclusion that the blood generally contains a small proportion of ammonia. 3. That the ammonia which is found in the air, respired in respiration and in insensible cutaneous perspiration, is derived from the blood, and is yielded in union with carbonic acid. 4. That the proportion of the volatile alkali is greater in venous than in arterial blood. 5. That in the blood of the Batrachians, and of other animals in which the aeration of this fluid is less perfect than in birds and mammalia of higher temperature, the quantity of the alkali is proportionally greater.

"On the Reason why the Stomach is not Digested by its own Secretion during Life." By Dr PAVY.—How is it that the stomach, composed as it is of digestible materials, escapes being digested itself, whilst digestion is being carried on in its interior? The question here raised must be admitted to be one of the utmost interest and importance to us all, because it touches upon the means by which we escape after every meal we consume from the occurrence of an event which would inevitably prove fatal to life. Hunter noticed that the stomach was susceptible of being attacked by the digestive liquid after death, and accounted for its power of resisting destruction during life by reference to the "living principle." This statement, however, fails to stand the test of actual experiment. The "living principle" must be discarded, as insufficient to account for the state of security under which the living stomach exists. To replace the refuted influence of the "living principle," it has been suggested that it is the epithelial lining which gives to the stomach the immunity from destruction it enjoys during life. This view, however, like Hunter's "living principle," fails to stand when submitted to the test of experiment; for I have found that a considerable-sized patch of mucous membrane may be removed, and food will afterwards be digested without the slightest sign of attack being made upon the deeper coats of the organ. The problem, therefore, as to why the stomach is not susceptible of attack during life, as it is after death, still remains open for solution; and the view that I have to offer refers the immunity observed to the circulation within the walls of the organ of an alkaline current of blood. It will not be disputed that the presence of acidity is one of the necessary circumstances for the accomplishment of gastric digestion. Now, alkalinity is a constant character of the blood, and as, during life, the walls of the stomach are everywhere permeated by a current of this alkaline blood, we have here an opposing influence, the effect of which

would be to destroy, by neutralizing its acidity, the solvent properties of the digestive fluid tending to penetrate and act upon the texture of the organ. The blood being stagnant after death, the opposing influence is lost that is offered by the circulating current. Should life happen to be cut short at a period of digestion, there is only the neutralizing power of the blood actually contained in the vessels of the stomach, to impede the progress of attack upon the organ itself; and the consequence is, that digestion of its parietes proceeds, as long as the temperature remains favourable for the process, and the solvent power of the digestive liquid is unexhausted. There is therefore no want of harmony between the effect that occurs after death, and the explanation that refers the protection afforded during life to the neutralizing influence of the circulation.

"On the Renal Organ—the so-called Water System—in the Nudibranchiate Molluscs." By Mr ALBANY HANCOCK.

"On the Renal Organ of the Aplysia." By Professor ROLLESTON.

"On Cranial Deformities, more especially on the Scaphocephalic Skull." By WILLIAM TURNER, M.B.

"On Life in the Atmosphere." By JAMES SAMUELSON.—The author described the occurrence of living germs in great quantity in the dust conveyed by the atmosphere and in distilled water, and showed that these germs retained their vitality for a long time under various vicissitudes. He opposed the theory of spontaneous generation; and he suggested whether the great rapidity with which these germs are multiplied might not account for the spread of epidemic diseases.

"On the Means of Passing Unharmed through Noxious Gases or Vapours." By Dr WHITE.—Dr White remarked—The apparatus which I have had constructed for "enabling a person to breathe in noxious gas" differs essentially from any heretofore used. If any person will keep his mouth closed, and by closing and opening either nostril so that he shall inhale by one alone, and exhale by the other alternately, and then insert a pipe into the nostril by which he inhales, he will understand the method of breathing with my apparatus as soon as it is described. It consists of two pipes fixed in a metal covering, which is placed over the nose and mouth. Each of these pipes is furnished with a valve of vulcanised india-rubber, one of which is fixed so that it can be moved only inwards, and the other only outwards. These valves are elastic, and of so light a material that they are opened and shut by the force of the air, moved to and fro in the act of breathing; and, therefore, when the air passes through these pipes the person inhales only through one pipe, and exhales through the other. Having the end of that by which he breathes in a pure atmosphere, the air which enters the lungs is pure, though the person is surrounded by noxious vapour. This inhaling pipe may draw its supply from the open air at any distance from the body, or it may be supplied from a bag carried on the body. By this plan the supply is limited to the size of the bag; by the other the supply is only limited by the length of his inhaling pipe. Sometimes one method and sometimes the other will be most convenient. The exhaled air passes through its pipe into the surrounding foul atmosphere. The flexible valve of the exhaling pipe completely prevents ingress of the surrounding gas; and therefore the pipe is short; it need not be more than half an inch in length. The part of the apparatus which I have described, and which I will call the "orinasal cover" and air pipes, is fixed in that part of a hood which covers the face. A tippet, made of water-proof cloth, is joined to the hood. Two circular pieces of glass are fixed in the hood for

enabling the man to see. Every part of this air-proof hood where the metal is fixed is water-tight. When this apparatus is about to be used, a soft, thick kerchief is to be wrapped smoothly round the neck. The hood and tippet is then put on. A band having been bound round the head and face over the hood, to keep the orinatal cover in its place, with a kerchief bound round the neck over the tippet, to prevent any foul air passing under it, the waistcoat and coat should then be buttoned and secured over the tippet for the same purpose. The means are so simple, and seem so slight, that a man may naturally hesitate to enter a poisonous gas with such protection; but if the apparatus be water-tight (which should be tested by trial with water, just before using it) he need not fear; for I have tried it repeatedly in the fumes of burning sulphur, in the sight of many witnesses, and it has never failed me. When the apparatus is used with an inhaling pipe extending into the pure air, the end may be fixed there, or held by an assistant, and conversation at the same time can be carried on between the wearer and his assistant through the pipe. When it is used in this manner a cord may be fastened round the person using the apparatus, and the other end held by the assistant outside, so that the man may be hauled out, if, through any accident, such a proceeding should be necessary. When the apparatus is used with an air-bag, it is attached to the back, and one end of the inhaling pipe communicates with it. A smaller air-bag is also connected by means of its own pipe, having a stop-cock, with the common inhaling pipe. The use of this small air-bag is, that the man may have its contents in reserve as soon as his larger store is exhausted; that he may know it is time to replenish his bags when his larger bag is empty. It might be useful to have one or more bags of air furnished with stop-cocks, and proper unions kept in reserve in places such as coal mines, where they may be required, so that a man using the apparatus could attach a full bag to it when necessary. I again state that it is necessary to examine the apparatus before using it, and to fix it on carefully; for if a man should enter carbonic acid gas with it, and through any imperfection of the apparatus shall inhale for some time a poisonous gas (though only a small quantity), he may fall down insensible, and may lose his life, unconscious of his danger.

"On the Practicability of Arresting the Development of Epidemic Diseases by the Internal Use of Anti-zymotic Agents." By Dr ROBINSON.

"On the Physiological Effect produced by several apparatuses contrived for the purpose of causing a Vacuum upon the entire body, or a part thereof." By Dr JUNOD.—Several important effects had been produced, and among them the power of occasioning syncope at will, and thereby producing the condition most likely to arrest inflammation without the loss of power consequent upon the abstraction of blood. Besides the apparatus for an exhausting effect, Dr Junod proposed the converse of this remedial influence, and applied compressed air. For this purpose a large air-tight chamber is used, having the form of a diving-bell. The patient is seated within, and air forced in until the necessary amount of pressure is produced—say of two, three, or more atmospheres. In the numerous experiments made of this treatment, it would appear that in the proportion that the powers of vitality are diminished in the method of exhaustion, they are increased and rendered more active by that of compressed air. The kind of maladies which have been relieved or cured by the latter process are precisely those in which an atonic condition of the system exists.

"On the Dietary of the Lancashire Operatives." By Dr EDWARD SMITH.

"On the Coal Miners of Durham and Northumberland, their Habits and Diseases." By Dr WILSON.

"On the Dietaries of the Labouring Classes." By Dr EDWARD SMITH.

"On the Calabar Bean." By Mr THOMAS NUNNELLY.

"On a Parasitical Acarus of the Anodon." By Mr R. GARNER.

"On a Miners' Safety-mask for supporting Life in Fire-Damp and other Noxious Vapour." By Mr B. W. RICHARDSON.

"On How to restore Drowned Persons, Patients in Chloroform Accidents, &c." By Dr KIDD.

"On the Normal Position of the Epiglottis." By Dr GEORGE D. GIBB.

"On Voluntary Closure of the Glottis, independently of the Act of Breathing." By Dr GEORGE D. GIBB.

"Note on the Change of Attitude which takes place in Infants beginning to Walk." By Dr CLELAND.

"On the Reciprocal Action between Plants and Gases." By Mr R. GARNER.—In this paper the author brought forward the subject of the natural inhalations and exhalations of plants, and of the effects on vegetation of certain non-natural contaminations of the atmosphere, such as occur in coal and mining districts, consisting for the most part of sulphurous and hydrochloric acids, and of ammonia. Different plants have different susceptibilities for such influence, and a kind of rude metrical table may be constructed, in which the greater or less impurity of the atmosphere may be shown from the effects on plants. Thus the rhododendron, or yucca, or aucuba will flourish in an air fatal to the common laurel; wheat will luxuriate when a holly or oak will die. Annuals suffer least, evergreens much. The paper gave the result of many experiments on plants by means of gases, &c., applied to the leaves and roots. Some plants which appear naturally to luxuriate in the coal strata, as the oak, holly, or some ferns, soon die, he stated, when the mines began to be worked. Trees lose their leaves earlier than in other districts, and, as a rule, evergreens suffer much. One thing was fortunate: annuals suffer less, for instance, corn and wheat do well when nothing else could, and perhaps the exhalations in question might even tend to ripen them.

"On the Physiological Action of the Uterus in Parturition." By Dr DONKIN.

"On the Condition of the Uterus after Delivery in Certain of the Mammalia." By Professor ROLLESTON.

"On the Ligamentous Action of the long Muscles in Man and other Animals." By Dr CLELAND.

SECTION E.—GEOGRAPHY AND ETHNOLOGY.

President—Sir R. I. MURCHISON.

"Proposed Inter-Oceanic and International Transit Route across Central America." By Captain BEDFORD T. PIM.

"Journey from Tientsin (North China) to the Capital of Mantchu Tartary." By Captain G. FLEMING.

"On the Commixture of the Races of Man as affecting the Progress of Civilisation in the New World." By Mr J. CRAWFURD.

"On Anthropological Classification." By Dr J. HUNT.

"A few Notes on Sir Charles Lyell's Antiquity of Man." By Mr JOHN CRAWFURD, F.R.S.—While the author agrees with Sir Charles Lyell as to the great antiquity of the appearance of man on the earth, he objects to his view of the unity of the human race and his doctrine of transmutation. He also differs from him as to the origin of languages. In speaking of Professor Huxley's views, he expresses his conviction that whatever actual resemblance there may be between man and apes, they are completely removed from each other in most important particulars.

"Geographical Notes on the Island of Formosa." By Mr ROBERT SWINHOE, Vice-Consul at Taiwan.

"Some Facts respecting the Great Lakes of North America." By Mr J. A. LAPHAM, of Milwaukee, Wisconsin, U.S.

"On the Physical and Mental Characters of the Negro." By Dr JAMES HUNT.—The general deductions which he attempted to draw were—1st, That there is as good reason for classifying the Negro as a distinct species from the European, as there is for making the ass a distinct species from the zebra. 2d, That the Negro is inferior intellectually to the European. 3d, That the analogies are far more numerous between the Negro and apes, than between the European and apes.

No man, he said, who investigates with an unbiassed mind, can doubt that the Negro belongs to a distinct type of man from the European. This word species, in the present state of science, is not satisfactory; but we may safely say that there is in the Negro that assemblage of evidence which would, *ipso facto*, induce an unbiassed observer to make the European and Negro two distinct types of man. My second and third proposition must be equally patent to all who have examined the facts.

We must, for the present, leave apart all questions of the origin of the Negro, and simply take him as he exists, and not as poets or fanatics paint him. We shall then learn, that it is only by observation and experiment that we can determine the exact place in nature which the Negro race should hold, and that it is both absurd and chimerical to attempt to put him in any other.

Mr CRAFT said that though he was not of pure African descent, he was black enough to attempt to say a few words in reference to the paper which had just been read. With regard to the origin of the Negro, he for one believed that black and white men were all descended from a common parent. As Africans were very dark, and the inhabitants of Northern Europe very fair, and as, moreover, the nations of Southern Europe were much darker than those of Northern Europe, it was perfectly fair to suppose that climate had a tendency to bleach as well as to blacken. The thickness of the skull of the Negro had been wisely arranged by Providence to defend the brain from the tropical climate in which he lived. The woolly hair was not considered by Africans as a mark of inferiority, though some of them shaved it off, but it also answered the purpose of defending the head from the sun. He had recently been to Africa on a visit to the King of Dahomey. He found there considerable diversities even among the Africans themselves. Those of Sierra Leone had prominent, almost Jewish features. Their heels were quite as short, on the whole, as those of any other race, and upon the whole they were well formed. Persons who had any knowledge

of Africans knew that, when they enjoyed advantages, they were capable of making good use of them. He might refer to the instance of the little girl brought to this country by Captain Forbes. This child was presented to the Queen, who had her carefully educated. When she grew up, she mingled in good society, and interested every one by her proficiency in music, and recently she had been married to a commercial gentleman of colour at Lagos. Another case was mentioned by Mr Chambers in one of his works, and another case was that of Mr Crowther, who was well-known to many gentlemen in this country. One word with reference to the ancient Britons. When Julius Cæsar came to this country, he said of the natives that they were such stupid people that they were not fit to make slaves of in Rome. It had taken a long time to make Englishmen what they now were, and therefore it was not wonderful if the negroes made slow progress in intellectual development. It was, however, proved that they made very rapid progress when placed in advantageous circumstances. He pointed to Hayti as furnishing an instance of independence of character and intellectual power on the part of the Negro; and contended that in America the degraded position which he was forced to occupy gave him no chance of proving what he really was capable of doing. He was sorry that scientific and learned men should waste their time in discussing a subject that could prove of no benefit to mankind. He spoke with great deference to their opinions, but, for his own part, firmly agreed with Cowper, that

Fleecy locks and black complexion
Cannot alter nature's claim;
Skins may differ, but affection
Dwells in white and black the same.

Professor Wilson claimed for the author of the paper and those gentlemen who supported him, the credit of being influenced by a desire to search out facts which could throw light on the important subject under discussion. At the same time he differed considerably from them in some of their conclusions. It was very important to have sufficient data before forming a theory, and he thought that Sir Charles Lyell and others, who contended that the intellectual progress of the Negro stopped at the age of fourteen, had fallen into the error which a person would who went into a workhouse among the most degraded and wretched of its occupants to find intellectual culture and capacity. The fact was that very few, if any, black children had any opportunity of pursuing their education after the age of fourteen; and in addition to that drawback, they had the misfortune to belong to a degraded and oppressed class, which was crushed and held down in the social world. The wonder was, that any of these unfortunate people had energy enough to make their escape and to acquire knowledge sufficient to enable them to carve a way for themselves through life, as not many of them did. The English character was made up of many elements, but a few hundred years ago the inhabitants of this island were cruel, unlettered, barbarous people. When put under good training and subjected to certain influences, the Anglo-Saxon proved capable of remarkable development, owing chiefly to the native energy of his character. The same quality was observable in some tribes of the present day, such as the New Zealander; and it was not wise, therefore, to argue as though the absence of combination denoted a natural and primary inferiority of race.

"On the Indian Tribes of Vancouver's Island." By Captain EUSTACE W. JACOB.

"On some Curiosities of Physical Geography in the Ionian Islands."—By Professor ANSTED.

"On a Central Argentine Railway from Rosario to Cordova, and across the Cordillera of the Andes." By W. WHEELWRIGHT.—The Central Argentine Railway commences at the city of Rosario, in the province of Santa Fe, on the right bank of the La Plata, in the latitude of 32° 56' southward, the longitude of 61° 30' west, and about 250 miles above Buenos Ayres by the channel route, which is navigable for ships of a large size, and has a depth of 16 feet of water; it possesses a very fine harbour, and all the elements of prosperity, and is the great commercial *entrepôt* of the interior provinces. Here the steamers which ply between Monte Video, Buenos Ayres, and Paraguay, and those engaged in commerce with Corrientes and other commercial points, stop, while almost a daily intercourse by steamers is kept up between this port and Buenos Ayres. From Rosario the railway will pursue its course in a north-west direction over those vast and fertile plains to Cordova, the central city of the plains, 247 miles, and thus will form the great trunk line, having upon its south and west the provinces of Mendoza, San Juan, San Luis, and the interior of the province of Buenos Ayres, whose high roads all concentrate upon the line of railway about midway; on the north are the provinces of Tucuman, Santiago del Estero, Jujury, Catamarca, and Rioja, with all their roads concentrating at Cordova, and thus forming one of the most extraordinary combinations to be found in the annals of railways.

"On the Physical Geography of Guatemala." By M. OSBERT SALVIN.

"On the Manganza in Africa." By the Rev. L. J. PROCTOR.

"On the so-called Celtic Languages in Reference to the Question of Race." By Mr JOHN CRAWFURD.

"On Celtic Languages." By Mr R. S. CHARNOCK.

Mr Crawford argued that the Gaelic and Welsh languages were quite distinct, while Mr Charnock took an opposite view.

"On some Points in the Cranioscopy of the Nations of South America." By Mr C. CARTER BLAKE.

"On the Gulf Stream and the Influence of Atmospheric Currents." By Captain MAURY.

"On his Travels with Captain Speke from Zanzibar to the Sources of the Nile." By Captain GRANT.

"A short Account of old Maps of Africa, placing the lakes (Nyanza and Tanganzika) nearly in their true positions." By Mr JOHN HOGG.

"An Account of his Disasters in ascending the Nile." By Vice-Consul PETHERICK.

"On his Travels towards the Sources of the Nile." By Signor MIANI.

"On his Exploration of certain Affluents of the Nile." By Baron von HEUGLIN.

"The Ethnology of Ceylon, referring especially to its Singalese and Tamil Inhabitants." By MUTU COOMARA SWAMY.

"On the Varieties of Man in the Malay Archipelago." By Mr ALFRED WALLACE.—In the Malay Archipelago are found two very strongly contrasted races—the Malays and the Papuans. The former inhabit the great Western Islands—Sumatra, Java, Borneo, and Celebes; the latter, New Guinea and the adjacent small islands. The typical Malays are of a light brown colour, resembling cinnamon or lightly roasted coffee; they have constantly straight black and rather coarse hair, little or no beard,

and generally smooth hairless bodies; they are of a low stature, rather strongly made, with short thick feet, and small delicate hands. The face is broad, the eyebrows flat, the nose small, well formed, with the nostrils somewhat exposed; the lips broad and well cut, the mouth large but not projecting. In character, the Malay is impassive, reserved, and bashful. His feelings of surprise, admiration, or fear, are not readily manifested, and he has little appreciation of the sublime or beautiful. He is somewhat taciturn, is deliberate when he speaks; he but seldom laughs, nor does he openly express his gratitude for a favour. He revenges an insult more quickly than an injury. He is honest and trustworthy in many matters, but prides himself upon his capacity of lying. His intellect is but mediocre, he is deficient in the energy necessary to acquire knowledge, and his mind seems incapable of following out any more than the simplest combinations. He is quick in acquiring mechanical arts, and therefore makes a good servant for simple routine duties. The Papuan is in many respects the opposite of the Malay. In colour he is a deep sooty-brown or black; his hair is very peculiar, being harsh, dry, and frizzly, growing in little tufts, which in youth are short and compact, but which in adults often grow out so as to form a compact frizzly mop, nearly a yard in diameter. He is bearded, and his arms, legs, and breast, are more or less hairy. The Papuan is taller than the Malay, and perhaps equal to the average of Europeans; the face is elongate, and the hands and feet rather large; the forehead is flat, the brows very prominent, the nose large, long, and arched, with the nostrils hidden by the overhanging tip. The face has thus a Semitic character, which is perceptible even in the children. The moral characteristics of the Papuan separate him widely from the Malay. He is impulsive and demonstrative in speech and action. His emotions and passions are expressed in shouts and laughter, in yells and frantic leapings. He is noisy and boisterous in speech and action, both at home and before strangers. Of his intellect less is known, but it seems at least equal and probably superior to that of the Malay. He has a love of art, decorating his canoe, his house, and almost every domestic article with elaborate carving. It must be granted, therefore, that these two races are most strongly contrasted; and if mankind can be classed at all in distinct varieties, the Malay and the Papuan must certainly be kept separate. Besides these well-marked races, are the inhabitants of the intermediate islands of the Moluccas and Timor, which, though differing in some degree from both, may yet, in almost every case, be classed with one or the other of them. The Negritos of the Philippines and the Lemangs of Malacca differ in most important characters from the Papuan races with which they have hitherto been classed, and must be considered to have Asiatic rather than Polynesian affinities. The recent evidence of the antiquity of man, and his having survived geological changes and the extinction of many species of mammalia, introduces a new element into ethnographical researches, and enables us to speculate more freely on the distribution and origin of races. Mr Darwin's researches on the structure and origin of the coral reefs of the Pacific, render it highly probable that great islands, or even continents, have recently sunk beneath its waters. The present distribution of animals in the Pacific islands leads us to conclude that this subsidence is geologically recent. The inhabitants of all the Pacific islands as far west as New Guinea and Australia have much in common, while they differ greatly from other races. Combining these facts, and boldly following their indications, we may divide the Malay Archipelago by a virtual waving line through the Moluccas, so that all the tribes to the west of the line will be Malayan and of Asiatic origin, and all to the east Papuan or of Polynesian origin. This division is in

harmony with that which has been shown to exist in the animal productions of the same regions, and obviates the difficulties attending every theory hitherto proposed as to the affinities and derivation of the Malayan and Polynesian races.

"On the Recent Discovery of Lacustrine Human Habitations in Wigtonshire." By Lord LOVAIN.

"Two Ascents of the Volcano of Misti." By the Hon. R. MARSHAM.

"On the Rivers of the Interior of Australia." By Rev. J. E. WOOD.

"Ethnology of Eastern Mantchuria." By Captain FLEMING.

"On the Human Cranium found at Amiens." By Mr HENRY DUCKWORTH.

"On the Anatomical Characters of the Human Cranium found at Amiens." By Mr WILLIAM TURNER.

"On the Tribes, Trade, and Resources around the Shore Line of the Persian Gulf." By Colonel PELLY.

"On the Commixture of the Races of Man as affecting the Progress of Civilisation in Eastern Asia and the Polynesian Islands." By Mr J. CRAWFORD.

"On his Visit to Dahomey." By Mr CRAFT (an African Gentleman).

"On the Aboriginal Occupation of North Tynedale and Western Northumberland. An Illustration of the Social Life of the Northumbrian Celts." By Rev. G. R. HALL.

"The Origin of Gipsies." By Mr J. CRAWFORD.

"Antiquities of the Orkneys." By Mr GEORGE PETRIE.

"Notice of the Discovery of Three additional Runic Inscriptions in St Molio's Cave, Holy Island, Argyleshire." By Professor DANIEL WILSON.

"On Ethnographical Casts." By HERMANN DE SCHLAGINTWEIT.

"Ethnology of the Island of Formosa." By Vico-Consul SWINHOE.

"The Extinction of Races." By Mr RICHARD LEE.

"Note upon the Opening of a Cist of the Stone Age near the Coast of the Moray Firth." By GEORGE E. ROBERTS and Professor BUSK.

SECTION F.—ECONOMIC SCIENCE AND STATISTICS.

President—Mr W. TITE.

"On the Vital and Sanitary Statistics of our European Army in India, compared with those of the French Army under like Conditions of Climate and Locality." By Dr J. BIRD.

"On the Coventry Freehold Land Society." By Mr C. H. BRACEBRIDGE.

"On the Decrease of the Agricultural Population of England, A.D. 1851-61." By Mr PURDY.

"On the Effects of the Recent Gold Discoveries." By Mr H. FAWCETT.

"On the Opening and Extension of Durham University Academical Endowments." By Mr HEYWOOD.

"On the Sanitary Condition of the Troops in India." By Dr CAMPS.

"On Transportation in connection with Colonisation." By Colonel TORRENS.

"On Mortality in Lancashire." By Mr FREDERICK PURDY.

"Remarks on Native Colonial Schools and Hospitals, from the Sanitary Statistics of Miss Florence Nightingale." By Mr JAMES HEYWOOD.

"Statistics of the Tanning Trade of Newcastle-on-Tyne (communicated by Mr JAMES POTTS). By the late Mr T. C. ANGUS.

"Military Budgets of English and French Armies, for 1863-4, Statistically compared." By Colonel SYKES.—He showed by a series of elaborate returns that the total effective English army was 147,118; that of the French, 355,187: the cost per head of the effective and non-effective English, numbering 147,118 men, was £94, 1s. 1½d., while the French effective and non-effective forces, of 400,000, was £43, 9s. 4d. per head. The cost of the British manufacturing department was £6, 10s. per head, against £2, 15s. 10d.; military stores (British) per head, £5, 14s., French, £3, 0s. 2d.; purchase of small arms (British), 14s. 4½d., against 5s. 8d.; British military education, £1, 3s. 5d., French, 7s. 1d.; administration of the British army (Secretary of State and Commander-in-Chief's department), £1, 8s. 11d., French, 6s. 11½d.; Government staff (British) per individual, £304, 5s., French, £390; clothing (British), £4, 0s. 2d., against £1, 19s. 11d. French.

"Report of the Committee on Technical and Scientific Evidence in Courts of Law." By Mr THOMAS WEBSTER.

"A Statistical Account of the Parish of Belingham." By Mr WILLIAM HENRY CHARLTON.

"On the Difference between Irish and English Poor Law." By WILLIAM NEILSON HANCOCK, LL.D.

"The Statistics connected with the Architectural Improvements in the City of Paris." By Mr WILLIAM TITE.

"The Volunteer Force; its Comparative Cost, Development, present State, and Prospects." By Mr HENRY C. ALLHUSEN.

"On the Origin of the Stockton and Darlington Railway." By Mr W. FALLOWS.

"Observations on Criminals." By Mr THOMAS ROBINS.

"On the Reduction of the Death-rate in Gateshead by Sanitary Measures." By Mr JOHN LAMB.

SECTION G.—MECHANICAL SCIENCE.

President—Rev. R. WILLIS.

Mr C. T. PORTER read a paper describing "Richards's Indicator for Steam-Engines."

Mr P. WESTMACOTT read a paper, prepared by himself and Mr J. H. SPENCER, "On the Engineering Manufactures of the Tyne and Neighbouring Districts."

Mr J. JAMIESON described a new form of Air-Engine of his invention.

A paper by Messrs HAWTHORN, describing their new method of working railways by stationary engines, was read.

"On a New Plan for Hanging Dock-Gates." By Mr R. A. PEACOCK.

Mr G. FAWCUS described a plan for building boats so that any number may be packed one within the other, thus enabling a larger number of boats to be carried on board ship, and effecting a great economy of room.

Mr D. PUSELEY read a paper descriptive of Thompson's Universal Stopper for bottles, casks, &c.,—an ingenious and effective mechanical arrangement for this purpose, by which many of the inconveniences from the use of corks are obviated.

Mr W. H. RICHARDSON read a paper "On the Paper Manufacture of Northumberland and Durham."

Mr C. W. SIEMENS then described his Regenerative Gas Furnaces for Iron Works.

Mr D. D. MAIN described the Newcastle and Gateshead Water-supply.

Admiral Sir E. BELCHER described a Spirit-level Telescope for observing altitudes and obtaining latitudes independently of natural or artificial horizons, and exhibited a telescope thus constructed.—This telescope carries within itself a spirit-level, the bubble of which is brought to the level wire to which the object is brought.

The Abbé MOIGNO exhibited and explained an instrument invented by Messrs Bourdon and Salleron, termed "Injecteur des Corps solides." The instrument is well adapted for the lecture-table, and illustrates in a remarkable manner the apparently paradoxical action of Gifford's Injector, now so largely used as a self-acting fuel-supplier for steam-engine boilers.

"On Extinguishing Fires." By Mr C. B. KING.

"On the Prevention of Fouling of Ships' Bottoms." By Dr WHITE.—The composition proposed for the covering of iron ships' bottoms is made of equal parts of powdered quick lime, of fat, and of oil, mixed and rubbed together. It is laid on the first time, when cold, by means of a short-haired painter's brush, on the surface, while high and dry; but when afloat it must be applied by means of a diver's hand, either naked or covered with a glove. This composition is a kind of soap, which is insoluble in water, and which undergoes a slow chemical change, the result of which is that, after a few months, it has become rather less soft, and more easily separable in the form of flakes or scales from the submerged surface of the ship than it was when fresh applied.

"Self-acting Valve Motion for Steam-Hammers." By Mr JOHN STURGEON.

"Iron Ship-Building on the Tyne and Neighbouring Districts." By Mr C. M. PALMER.

"Description of the large Gyroscope used by Sir William Armstrong in the invention of Rifled Projectiles." By Professor POLE.

"On Skin Resistance as Affecting the Speed of Ships." By Professor RANKINE.

"An Investigation of Plane Water Lines for Ships." By Professor RANKINE.

"The Diagonal Principle of Iron Ship-Building." By Mr ROBERT TAYLORSON.

"A Mode of rendering Timber-built Ships impregnable and unsinkable under moderate Crew-power, as in a leaky vessel." By Admiral Sir EDWARD BELCHER.

"On an Improved Caisson Gate." By Admiral Sir E. BELCHER.

The Abbé MOIGNO exhibited the "Ventilateur à réaction" of Mons. Perigault de Rennes, and the "Balance aerostatique" of Mons. Seiler.

"Improvements in Waggons and Gun-Carriages." By Mr GEORGE FAWCUS.

"On Bridge Foundations." By Mr THOMAS PAGE.

"An Improved Valve and Apparatus for Atmospheric Railway Propulsion." By Mr W. SMITH.

"A novel method of covering Boilers, Pipes, and Cylinders of Steam Engines, for preventing the Radiation of Heat." By Mr W. SMITH.

"On Boiler Explosions." By Professor AIRY.

"On the Improvements now being carried out in the River Tyne." By Mr J. F. URB.

"On Targets for Gunnery Experiments." By Captain DOUGLAS GALTON.

"On Rifled Ordnance." By Mr GEORGE RICHARDS.

"On the Decortication of Cereals." By Mr ROBERT DAVIDSON.

"On Improvements in Machinery and Apparatus for Cleansing and Purifying Casks." By Mr ROBERT DAVIDSON.

"The Application of Machinery to Coal-Cutting." By Mr SAMUEL FIETH.

"Caselli's Auto-Telegraph from Paris to Marseilles." By the Abbé MOIGNO.

"Oudry's Galvano-Copper and Galvano-Copper Paint applicable to Buildings, Armour Plates for Ships, &c." By the Abbé MOIGNO.

"Report of the Committee on Steam-Ship Performances." By Mr W. SMITH.

"Portable Machinery Apparatus for Rivetting, Chipping, &c." By Mr W. SMITH.

"Novel Arrangement of Direct Acting Steam-Engines." By Mr W. SMITH.

"Bown's Tyre Fastening." By Mr BENJAMIN FOTHERGILL.

Botanical Society of Edinburgh.

Thursday, 14th May 1863.—Professor MACLAGAN, President,
in the Chair.

The following Communications were read :—

1. *Descriptions of New Genera and Species of Diatoms from the South Pacific.* Part I. By R. K. GREVILLE, LL.D., F.R.S.E. With Plate.

(This paper appears in this volume of the Journal, p. 34.)

2. *Experiments on the Fertilisation of Orchids in the Royal Botanic Garden of Edinburgh.* By Mr JOHN SCOTT.

After some introductory remarks on hybridisation, and on Darwin's experiments, the author says :—"Having occasion to require a few capsules from the Vandeeæ tribe, I carefully fertilised for some time various species which flowered in the Royal Botanic Garden here. From certain of these I had the following somewhat singular and anomalous results. On different plants of the *Oncidium sphacelatum*, I fertilised a number of flowers with their own pollen, yet in no case did I ever succeed even in causing the capsule to swell. The only external signs the flowers afforded of being affected by the pollen were the closing of the stigmatic orifice twenty-four hours or so after its application, and the slightly earlier withering of the flower. On the large and vigorous plant of *O. sphacelatum*, which, along with others subsequently noticed, Mr M'Nab has kindly permitted me to place before the Society, I impregnated between

100 and 200 flowers, yet every capsule aborted. I dissected the column of many of these flowers as they dropped off, and invariably observed an abundance of pollen-tubes, which in most cases I traced into the ovary. Thus if the function of the stigma be simply to excite the emission of pollen-tubes, and that of the style their conduction to the ovary, we have evidence of its accomplishment, and might then attribute abortion to some inappreciable change in the sexual elements preventing normal conjunction.

"Having thus failed in fertilising *O. sphacelatum* with its own pollen, I determined to try crosses with other species. With this end in view, I first crossed it reciprocally with the neighbouring species, *O. altissimum*, under the impression that the probabilities for successful results would be inversely proportionate with the more or less immediate systematic affinities of the plants. In this my experiments have somewhat disappointed me; and the results of the above were the unexceptional abortion of every capsule. I next tried *O. graminifolium* with the pollen of *O. sphacelatum*, and succeeded in producing a capsule, which contained about one-fourth of embryonated seeds. I did not succeed *vice versa*. All the flowers thus treated dropped early. I may state that *O. graminifolium* does not appear from my experiments to be very susceptible of fertilisation with its own pollen.

"From four flowers of *O. ornithorynchum*, impregnated with the pollen of *O. sphacelatum*, I got one fine seed-capsule, though I had never before succeeded in fertilising this species with its own pollen; I have now a single capsule thus produced. On dissection of the crossed capsule, I was disappointed to find that it contained few seeds, and of these a great majority presented only a loose transparent testa, entirely destitute of the embryo. The capsule, on the other hand, fertilised with its own pollen, though not quite so large as the above, was quite filled with seeds, of which about three-fourths presented an embryo. I did not succeed in impregnating *O. sphacelatum* with pollen of *O. ornithorynchum*, though the capsules thus treated at first showed symptoms of swelling.

"The most successful experiments which I have made were with *O. divaricatum* var. *cupreum*, and *O. sphacelatum*. I impregnated six flowers upon the latter with pollen from the former; and I have now four fine capsules, nearly mature, as the result. I failed, however, in my attempts at crossing them reciprocally. I made also numerous attempts to impregnate *O. divaricatum* with its own pollen, and this as well upon those plants growing in baskets suspended in the hot-houses as those growing in pots. On one of the plants in the latter condition I have now, after many failures, succeeded in fertilising four flowers; the capsules, however, are very abnormal-looking productions. In one of them which I cut off for dissection, I did not find a single perfect seed. On the other hand, the results of experiments on plants growing in baskets, though the more natural mode of cultivating them, was, singularly enough, the abortion of every capsule thus impregnated. The only indications of pollinic influence were an earlier fading of the flowers, and the closing of their stigmatic orifices, which I may remark is in this case effected by a gradual depression of the clinandrium, instead of a simple incurvation of the wings of the orifice, as occurs in *O. sphacelatum*, mentioned above.

"Thus, in the above experiments, I have failed in crossing reciprocally any two species; nevertheless, I think I have satisfactorily shown an individual impotence of the sexual organs, in their mutual action, conjoined with a capability of normally performing their functions by the action of other species—and this in comparative disregard of recognised

systematic affinities. I have yet, however, another case to state, in which we are afforded an illustration of this reciprocal action of species. It occurred with the *Maillaria atro-rubens* and *M. squalens*. On the former of these I had made numerous attempts to fertilise flowers with their own pollen, yet in every case they proved abortive. I was thus induced to try pollen from another species. I accordingly took a few pollen-masses from flowers of *M. squalens*—certainly a very dissimilar species, but the only one in flower at the time—with which I fertilised eight flowers of *M. atro-rubens*. These have afforded me very successful results, as six out of the eight have produced well-formed capsules. I next tried pollen from *M. atro-rubens* upon a flower of *M. squalens*, and have now succeeded in fertilising a capsule on the latter also. I may remark, however, that I have found this species perfectly productive when fertilised with its own pollen.

III. *On some New British Lichens.* By the Rev. T. SALWEY, B.D.,
Vicar of Oswestry, and formerly Fellow of the Linnean Society.

The following is a list of the Lichens described :—

1. *Aspicilia aquatica*, Kőrber. 2. *Lecidia fusco-rubens*, Nyl. 3. *Lecidea lecanorina*, Nyl. (*Mischoblastia lecanorina*, Massalongo.) 4. *Lecidea lithophila*, Aeh.; Nyl. Lich. Scand., p. 226. 5. *Lecidea tessellata*, Flörke. 6. *Lecidea Phæops*, Nyl. 7. *Squamaria galactina*, Pers. 8. *Verrucaria rudorum*, DC.; Fl. Fr., p. 318. 9. *Verrucaria æthiobola*, Wahl. (*V. margacea*, var. 1, *æthiobola*, Nyl.) 10. *Verrucaria virens*, Nyl. in Bot. Notes, 1853, p. 180. 11. *Calicium trajectum*, Nyl., nov. sp.

IV. *Synopsis of the Canadian Species of Equisetum.* By GEORGE LAWSON, LL.D., Professor of Chemistry and Natural History in the Queen's University of Canada.

The author gave descriptions of the species and their localities under the following sections :—

SECTION I. Stems annual, the sterile ones bearing verticils of soft green herbaceous branches; the fertile stems either evanescent and naked, or permanent and ultimately clothed with similar verticillate branches.

1. *E. sylvaticum*. 2. *E. umbrosum*. 3. *E. arvense*, and β . *granulatum*. 4. *E. Telmateja*. 5. *E. limosum*.

SECTION II. Stems perennial, silicious, not verticillately branched.

6. *E. hyemale*. 7. *E. variegatum*. 8. *E. scirpoides*, and β . *minor*.

V. *Register of Plants in Flower in the Open Air at the Royal Botanic Garden (4th List).* By Mr JAMES M'NAB, Curator.

The following shrubs and trees were in flower on the 20th day of April 1863, and they have not been enumerated in any of the previous lists laid before the Society at the above date :—

Pyrus Malus	Syringa persica	Forsythia viridissima
Prunus Padus	Cratægus præcox	Medicago arborea
Syringa vulgaris	Sambucus racemosa	Kalmia glauca

<i>Fagus sylvatica</i>	<i>Acer rubrum</i>	<i>Pittosporum Tobira</i>
<i>Alnus glutinosa</i>	— <i>platanoides</i>	<i>Genista elongata</i> and
— <i>cordifolia</i>	<i>Azalea pontica</i>	<i>Wistaria sinensis</i> —
<i>Viscum album</i> , mas	<i>Daphne pontica</i>	(wall)
<i>Lonicera Xylosteum</i>	<i>Ribes aureum</i>	<i>Caragana frutescens</i>
<i>Cydonia japonica</i>	— <i>speciosum</i>	<i>Ulmus montana</i>
<i>Buxus sempervirens</i>	<i>Berberis Jamesoni</i>	<i>Fraxinus excelsior</i>
<i>Ilex Aquifolium</i>	<i>Viburnum vestitum</i>	Double flowering
<i>Euonymus europæus</i>	<i>Weigela rosea</i>	cherry
<i>Acer Pseudo-platanus</i>		

Thursday, 11th June 1863.—T. C. ARCHER, Esq., Vice-President,
in the Chair.

The following Communications were read :—

- I.—*On the Nature of the Perigynium or Utriculus of the Genus Carex.*
By A. WESMAEL of the School of Horticulture of Vilvorde in Belgium.

The author remarks that Saint Hilaire and Lindley compare the utricle of carices to the superior glumella or palea of grasses, and adopt the views of Brown, who considers the palea formed of two coherent bracts. Lemaout and Adrien Jussieu entertained the same opinion. The point to be determined is whether the axis bearing the secondary flower takes its origin laterally—and in that case either to the right or left, and opposite one of the two teeth which surmount the utricle, and which correspond to the two median ribs of the two perigonial divisions of Lindley; or whether that axis is inserted in front of the ovary. After careful examination, M. Wesmael finds that the latter view is the correct one—viz., that the secondary axis is produced in front of the ovary. This is the view taken also by Kunth, Gay, Roper, and Schlechtendal.

The following are the conclusions at which he has arrived :—

“1. The utricle which envelopes the ovary of the carices has been considered in two different ways.

2. According to the theory of Lindley, this utricle is formed by the cohesion of two bracts, whilst, according to Kunth, it is formed by a single bract.

3. Lindley believes in the presence of two bracts, from the existence of the two ribs in the utricle, which he thus regards as analogous to the two-keeled palea of grasses.

4. Lindley looks on the utricle as being developed on an axis, which gives origin both to it and the ovary.

5. This theory does not give a true view of the nature of the rachaeole.

6. The rachaeole is a prolongation of the secondary axis, which, in an abnormal inflorescence of *Carex acuta*, after having given origin to the normal utricle, is prolonged, and comes out in company with the stigma at the orifice of the utricle; it then gives origin to the bract, and afterwards to the secondary utricle; and on the axil of this is developed a very short axis, terminated by the ovary.

7. The rachaeole is not developed to the right or left of the ovary,—that is to say, in front of the one or the other of the two principal ribs of the utricle,—but is prolonged in front of the ovary, and consequently between it (the ovary) and the parent bract.

8. The mode of development of the racheole does not favour the view of Lindley ; according to which it ought to have for its point of origin the axil of one of the two bracts constituting the utriculus.

9. The utriculus is an organisation analogous to that of the vaginiform bract which we observe at the base of the axis of inflorescence.

10. All the observations I have made are in favour of the theory of Kunth, and contrary to that of Lindley."

II.—Photoglyphic Engraving of Ferns ; with Remarks.

By H. FOX TALBOT, Esq., LL.D. (Plate VI.)

Dr Talbot remarked—This plate, representing a fern leaf, is done by the process which I have called photoglyphic engraving, and which was published a few years ago in the various photographical journals. It consists in covering a plate of steel or copper with a film of gelatine mixed with bichromate of potash, placing the object, in this instance a fern leaf, upon the plate, and exposing it to sunlight for a minute, then removing the object, dusting the plate with finely powdered resin, to give a grain to the engraving, melting the resin, and when the plate is cold brushing it with a camel's-hair brush dipped in perchloride of iron, which speedily penetrates the parts upon which the light has not acted, and effects an etching, while the parts exposed to sunshine are thereby rendered impermeable to the etching liquid. Such a plate (steel), thus prepared, is capable of yielding at least five thousand impressions. It will be remarked that the leaf exhibits no venation. This arises from its being a dried specimen from the herbarium ; but recent specimens, especially of *Adiantum*, *Trichomanes*, *Hymenophyllum*, &c., yield engravings with a very beautiful venation. Grasses also succeed remarkably well, and one of the most curious objects when engraved is the feathery achenes of *Leontodon Taraxacum* (the common dandelion), which are so well given as sometimes to be almost deceptive, except that the achenes themselves are apt to come out too black to be natural. If printed in pale brown ink, I think they would have a strong resemblance to nature. I wished to have shown a plate of this description to the Botanical Society, but the occurrence of cloudy weather for several days in succession has prevented my doing so on the present occasion.

If this art had been invented a hundred years ago, it would have been very useful during the infancy of botany, when communications with distant countries were so difficult. It would have been easy for botanical travellers such as Thunberg, Aublet, Loureiro, &c., to have taken with them a small printing-press, and to have worked off a small edition of fifty copies of each engraving they made, and sent the copies home to Europe whenever an occasion presented itself. Some of the copies would doubtless have escaped shipwreck and come to the knowledge of European botanists. Supposing the engraving only represented a leaf and a flower of each plant described in the works, it would have greatly aided modern botanists in determining the plants intended by those authors, whose descriptions are frequently so incorrect that they are like so many enigmas, and have proved a hindrance and not an advantage to science.

III.—List and Localities of Rare Plants found in the vicinity of Perth.

By Mr JOHN SIM, A.B.S. Ed.



PHOTOPOLYMERIC ENGRAVING OF A FERN



IV.—*List of Plants found near the Rumbling Bridge.* By Professor
BALFOUR and Mr F. B. W. WHITE.

<i>Ranunculus aquatilis.</i>	<i>Listera ovata.</i>
— <i>auricomus.</i>	<i>Neottia Nidus-avis.</i>
<i>Trollius europæus.</i>	<i>Sparganium ramosum.</i>
<i>Aconitum Napellus.</i>	<i>Carex remota.</i>
<i>Cardamine amara.</i>	<i>Carex sylvatica.</i>
<i>Stellaria nemorum.</i>	<i>Milium effusum.</i>
<i>Dianthus barbatus.</i>	<i>Melica uniflora.</i>
<i>Geranium pratense.</i>	<i>Polypodium Phegopteris.</i>
— <i>lucidum.</i>	— <i>Dryopteris.</i>
— <i>sylvaticum.</i>	<i>Lastrea Oreopteris.</i>
<i>Trifolium medium.</i>	— <i>dilatata.</i>
<i>Vicia sylvatica.</i>	<i>Polystichum aculeatum.</i>
<i>Spiræa salicifolia.</i>	— <i>lobatum.</i>
<i>Rubus saxatilis.</i>	<i>Cystopteris fragilis.</i>
<i>Epilobium angustifolium.</i>	<i>Athyrium Filix-fœmina.</i>
<i>Circeæ alpina.</i>	<i>Asplenium Trichomanes.</i>
<i>Saxifraga hypnoides.</i>	<i>Blechnum boreale.</i>
— <i>umbrosa.</i>	<i>Equisetum umbrosum.</i>
<i>Sedum reflexum.</i>	<i>Weissia verticillata.</i>
<i>Archangelica officinalis.</i>	<i>Trichostomum flexicaule.</i>
<i>Viburnum Opulus.</i>	<i>Zygodon Mougeotii.</i>
<i>Lonicera Periclymenum.</i>	<i>Pogonatum urnigerum.</i>
<i>Hieracium prenanthoides.</i>	<i>Polytrichum juniperinum.</i>
<i>Eupatorium cannabinum.</i>	— <i>piliferum.</i>
<i>Petasites fragrans.</i>	<i>Mnium rostratum.</i>
<i>Matricaria Parthenium.</i>	— <i>punctatum.</i>
<i>Carduus heterophyllus.</i>	<i>Bartramia fontana.</i>
<i>Campanula latifolia.</i>	— <i>pomiformis.</i>
<i>Ligustrum vulgare.</i>	<i>Anomodon viticulosus.</i>
<i>Symphytum officinale.</i>	<i>Isoethecium myurum.</i>
<i>Veronica Anagallis.</i>	— <i>alopecurum.</i>
<i>Mentha sylvestris, β. velutina.</i>	<i>Hypnum striatum.</i>
<i>Calamintha Clinopodium.</i>	— <i>ruscifolium.</i>
<i>Rumex aquaticus.</i>	— <i>stellatum.</i>
— <i>sanguineus, β. viridis.</i>	— <i>cupressiforme, β. com-</i>
<i>Salix cinerea, β. aquatica.</i>	— <i>pressum.</i>
<i>Populus nigra.</i>	<i>Endocarpon miniatum.</i>
<i>Castanea vulgaris.</i>	<i>Peltidea scutata.</i>
<i>Paris quadrifolia.</i>	<i>Ramalina fastigiata.</i>

Professor Balfour exhibited a collection of dried plants sent by the Rev. W. C. Thomson, Old Calabar, and read a letter which he had received from him, dated 28th March 1863, in which he says:—By this mail I send you some fire-dried specimens of our Flora, which I trust will be acceptable and prove interesting. For this simple and easy method of preparing them, such as they are, I am entirely indebted to my friend Mr G. Mann, who has been spending some time on the coast collecting for Kew. He visited us lately, and spent a few days with us; but unfortunately for us he leaves the coast by this mail, so that our Flora is not likely to figure so largely as it may deserve in the forthcoming volume on the Botany of Western Africa. I trust the specimens being sent will enable Edinburgh friends to contribute towards filling up the gap.

In the case are two parcels of Ndam. It is the epidermis of the leaf of the

Membo palm. All are dyed by boiling,—the black with the bruised leaves of *Mucuna Balfouriana*, the red with the bruised wood of a tree called Ukpa, and the yellow with the bruised bark of a tree called Auniau.

Mr W. W. Evans recorded the occurrence of *Helminthia echinoides*, a rare Scotch plant, by the side of a field at Tynefield, near Tynningham, where it has probably been introduced.

Thursday, 9th July 1863.—Professor BALFOUR, Vice-President, in the Chair.

The following Communications were read :—

1. *Descriptions of New Genera and Species of Diatoms from the South Pacific* By R. K. GREVILLE, LL.D., F.R.S.E., &c. Part II.

(The paper appears in the present Number of this Journal, p. 181.)

- II. *Description of the Fruit and Seed of Clerodendron Thomsonæ*. By Professor BALFOUR.

(The paper appears in the present Number of this Journal, p. 258.)

- III. *Notice of a Botanical Excursion to Kielder and Deadwater Fell, Northumberland, on 4th July 1863*. By Professor BALFOUR.

Professor Balfour gave an account of a botanical trip to Kielder on Saturday, 4th July, with eighty pupils. The party went by the North British Railway to Riccarton, and thence by the Border Counties Railway to Kielder. They visited the woods near Kielder Castle, and proceeded by the Kielder Water to the mountains in the neighbourhood, ascending Deadwater Fell, which rises to the height of 1800 or 2000 feet. The numbers of species gathered were as follows :—

Flowering plants, ...	176
Ferns, Lycopods and Equisetums, ...	18
Mosses, ...	66
Jungermanniæ, ...	10
Lichens, ...	38

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Among the plants collected were the following :—

<i>Carduus heterophyllus</i> .	<i>Salix herbacea</i> .
<i>Aquilegia vulgaris</i> .	<i>Vaccinium Vitis-idaea</i> .
<i>Spiræa salicifolia</i> .	<i>Botrychium Lunaria</i> .
<i>Trollius europæus</i> .	<i>Scolopendrium vulgare</i> .
<i>Rubus Chamæmorus</i> .	<i>Hymenophyllum Wilsoni</i> .
<i>Listera cordata</i> .	<i>Cryptogramme crispa</i> .
<i>Habenaria viridis</i> .	<i>Polypodium Phegopteris</i> .
<i>Melampyrum pratense</i> var. <i>montanum</i> .	<i>Lastrea Oreopteris</i> .
<i>Veronica scutellata</i> .	<i>L. spinulosa</i> .
<i>Solidago Virgaurea</i> .	<i>L. dilatata</i> var. <i>alpina</i> .
<i>Corydalis claviculata</i> .	<i>Asplenium Ruta-muraria</i> .
<i>Sedum villosum</i> .	<i>Eridia auricula</i> .

Among the Musci, Hepaticæ, and Lichenes, which were determined by Mr John Sadler, were the following :—

Andræa Rothii.
Sphagnum cuspidatum var. *plumosum*.
Gymnostomum curvirostrum.
Campylopus longipilus.
Cinclidotus fontinelloides.
Racomitrium aciculare.
Pogonatum alpinum.
Bryum nutans.
Splachnum sphaericum.
Bartramia fontana.
Fegatella conica.
Jungermannia sphagni.

Baeomyces roseum.
Lecanora tartarea.
Sticta pulmonaria.
S. sylvatica.
S. scrobiculata.
Peltidea scutata.
Cetraria glauca.
Alectoria jubata.
Sphaerophoron compressum.
Stereocaulon paschale.
Cladonia uncialis.
Scyphophorus squamosus.
S. cocciferus.

IV. *Notice of Mosses found in Fifeshire.* By Mr CHARLES HOWIE.
 Communicated by Mr JOHN SADLER.

The author remarked that he had gathered 248 species and varieties of mosses in Fifeshire, some of them being very much restricted in their distribution.

Bankhead Moss, situated a few miles south of Ceres, is one of the few remaining portions of peat formation from which the country people cut peat for domestic purposes. It contains pools filled with water. In the larger and deeper pools we have *Sphagnum cuspidatum* var. *plumosum* in fruit, floating freely, bearing its fruit generally on lateral pedicels; *Sphagnum contortum* var. *subsecundum*, bearing fruit more sparingly. *Sphagnum cuspidatum* var. *recurvum* is generally associated in more dense masses in water of less depth. *Hypnum molluscum* is found by the sides of some of the deep ditches in fruit, and *Sphagnum cymbifolium* occurs with various tints of colour. *Sphagnum acutifolium*, *S. rubellum*, and *Hypnum fluitans* occur in fine fruit, in a pool of deep water, associated with *Sphagnum cuspidatum* var. *plumosum*. *Hypnum irriguum* grows in one of the small streams of the neighbourhood, and *Splachnum sphaericum*, on the naked peat soil. The Tents Muir is a station of peculiar interest in a Bryological point of view. *Bryum Marratii*, *B. calophyllum*, *Catoscopium nigrum* and *Distichium inclinatum* cover large patches of the Muir. *Amblyodon dealbatus*, with *Meesia uliginosa*, occurs densely covered with fruit. *Hypnum albicans*, *H. salebrosum*, *H. polygamum* and *H. lutescens* are also found in fruit. We also find *Bryum pseudotriquetrum*, *B. pallens*, and various other species more generally distributed, as well as *Climacium dendroides* and *Tortula vinealis* in fruit.

At Largo we come upon the basalt and trap which produce abundance of *Orthotrichum anomalum*, with a few examples of *O. saxatile*, *O. diaphanum*, and other species; *Grimmia leucophaea* and *G. Doniana* are also found. On Largo Law, *Grimmia apocarpa* is associated with *Pterogonium filiforme*, *Leucodon sciuroides*, and *Bryum crudum*. At Kilconquhar Loch specimens of *Mnium affine* var. *rugicum* occur in fruit; also *Hypnum cordifolium* in fruit. In the parts of the Links which are occasionally flooded by stream tides we meet with *Distichium inclinatum*, also *Hypnum abietinum* and *H. nitens*, *Encalypta rhabdocarpa* in considerable quantity and in dense fruit; further inland from the sea, *Tortula intermedia*, with *Orthotrichum anomalum* and *Hypnum lycopodioides*.

V. *Notice of some of the Woods used for Economical Purposes in New Zealand.* By Dr TUXE.

VI. *Notice of the Cinchona Cultivation on the Neilgherry Hills.*

By Dr Hugh Cleghorn.

The cultivation of Cinchona, or the Peruvian bark plant, is likely to supply a new produce for the trade of India. Mr Vincent, an enterprising planter, has secured 5000 Cinchona plants for the land he has purchased, and M. de Facien, an enterprising planter, has secured as many more. Altogether, the orders on record exceed the number of plants Mr M'Ivor (the superintendent of the Ootacamund garden) will be able to supply at the close of the current official year, which we understand will be 34,000. The plants available in the course of the next year, or from May 1863 to April 1864, are estimated at 100,000. This will be out of the Government nurseries, but the settings from their own stock, which planters in the meantime will have been able to rear, will be something considerable, for a single plant, some six feet high, in the public garden here, has given Mr M'Ivor no less than 900 cuttings, each the nucleus of a healthy sapling now, with the promise of a gigantic forest tree hereafter. The enormous source of wealth to which the Cinchona points, is actually derived from bricks. When a shoot is taken off a plant, it is immediately placed in a pot filled with brick dust. Hundreds, nay thousands of these pots may be seen in Mr M'Ivor's conservatories, covered with what look like nothing more than diminutive leaves thrust into them. Here the shoots are allowed to remain till they recover from the shock attending their severance from the parent stem. They are then transferred to pots charged with a mixture of decomposed felspar and garden mould, in which the process of rooting goes on. Several acres of shola land in the vicinity of the Government garden have been planted, and there can be no doubt that wherever the shade of a forest tree has fallen upon the interesting suckling below, it has pined and withered, or been stunted. The instances of this effect in a romantic glen to which Mr M'Ivor will cheerfully lead visitors are very remarkable. At a few yards from the umbrage of a group of trees, the plants look exceedingly flourishing, but their healthfulness, size, and vigour diminish in proportion to their approximation to shade. Some plants, which were put out in the Government garden in various positions a year ago, fully bear out this result. Those in the shade look sickly, and those subjected to drippings look worse. Mr M'Ivor does not consider the Ootacamund plantation a complete demonstration of his principles, that at Neddiwattum being more corroborative of his views; but any one knowing the difference between a healthy and a sickly plant, must at once acknowledge that shade is not the condition for the active development of the Cinchona. There are, of course, other important particulars connected with the ultimate success of the Cinchona in India which must be left to time and experiment to establish. We might mention the earliest period of growth at which the alkaloids begin to show themselves,—the efficacy of decoctions and infusions made from dry bark and leaves compared with their virtue when prepared with fresh specimens,—and the possibility of dispensing in a large measure, if not entirely, with the expensive manufactured article, quinine, in the event of bark and leaf possessing sufficient curative properties to be exhibited in most forms of fever disease. One or more of these points, accompanied with the requisite materials for arriving at correct results, will, we believe, be shortly submitted for decision in England. A very interesting feature in the habitude of the Cinchona plant is its power of reproducing its bark, when deprived of it, to a greater thickness than the original formation. This peculiarity, we learn, has been noticed by Mr M'Ivor, after covering the denuded parts with moss for a month

or two; but whether the larger volume of the bark thus produced will contain a proportionate excess of alkaloids, is a subject which at present rests in obscurity.

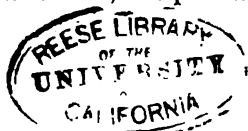
Professor Balfour read a letter which he had received from John Allan Brown, F.R.S., of the Trevandrum Observatory, Madras, in which he says:—"I used to botanise when in Scotland, and have always felt anxious to do something in that way among our unknown mountains, but my other pursuits have always prevented me. I tried to make a collection of a few plants, chiefly orchids, ferns, and mosses, but I had no good drying paper, only old newspapers, and it was very cloudy and damp, so that most of the flowering plants are spoiled. Could I have made out a collection worth anything, I would have sent you a set of the specimens. The finest palm of these mountains is the *Bentinckia Condapana*. I think it is the most graceful palm in Northern India. It grows chiefly in the clefts of precipices and among rocks not easily reached; but there are such forests of it on some slopes that we were kept from starving by cutting it down for the cabbage (young shoot), which is delicious. Raw, it is like the finest walnut; but we had it cooked as a vegetable and as a curry, when we had nothing else to eat. Its effect in the foreground in groups, and even in the distance of the landscape, is very fine. I have no book on palms, but I believe it is not found in the Neilgherries, nor do I know if it exists in Ceylon. It is found at a height of from 2500 to upwards of 5000 feet at the place where I have been, but I have no doubt it would grow at higher stations. I have brought down a few young plants to try to make it grow here. We have a museum here, of which I am honorary superintendent, and I tried to get up a botanic garden, but nothing has been done as yet. This climate is almost exactly that of the country of the *Victoria regia*, and I have a fine tank in which I could put it.

Mr F. Naylor presented specimens of *Briza maxima*, collected, apparently in a wild state, in Jersey.

SCIENTIFIC INTELLIGENCE.

BOTANY.

Report on the Bark and Leaves of Cinchona Succirubra grown in India. By J. E. HOWARD, Esq.—In order to make the best analysis of the small quantity of bark at my command, I commenced with 500 grains of that of the second year's growth, and was able to obtain therefrom a first and second crystallisation of white sulphate of quinine. By thus specifying the whiteness, I mean to imply that the bark had not the commercial disadvantage which frequently attends the "red bark" at a more mature stage of growth, resulting from the fact that the colouring matter has in these last become so much implicated with the alkaloids as to make the task of purification a difficult one. The crystallisations I obtained were mixed with some sulphate of cinchonidine, which is commercially (but not medicinally) a disadvantage, and one which always attends the products of "red bark." I also obtained cinchonine, and other usual products, of the process as from South American bark, viz., kinovic acid, kinate of lime, gum, cinchona red, &c. The product of alkaloid in a rough state was estimated at 4.30 per cent. A second trial of the same quantity enabled me to decide more accurately the per-centage product in purified alkaloids. I found the total contents 3.30 to 3.40 per cent., and of this (soluble in ether) quinine and some cinchonidine, 2.40 per cent.,



leaving 60 per cent. of cinchonine, which crystallised freely, and also 30 or 40 loss chiefly in water of the hydrated alkaloids. This result must be considered extremely favourable.

I have noticed the product of some fine quills of South American red bark as 3.60 per cent.,* the larger bark of the same parcel producing 3.91 of alkaloid. Dr Kiegel obtained from 1 oz. red bark, of best quality, 4.16 per cent. by Rabourdin's process, or 3.90 by that of Buckner. Of this 2.65 per cent., soluble in ether, was reckoned as quinine, and the rest was set down as cinchonine.† I have obtained a much higher per-centage of alkaloid from large and peculiarly fine "red bark," but I see no reason to doubt that even this higher per-centage would be attained in the East Indies, if time were allowed for the growth.

The exact period at which it would be advisable to cut the bark must be ascertained by experiment, but I think this should take place as soon as the bark attains to a thickness which would repay the cultivation. There would be positive disadvantage in allowing the bark to attain such an age as is indicated by many of the specimens from South America, if the object to be attained is the extraction of the alkaloids; since there is a continual process of deterioration‡ of these after a certain period of the history of the bark, which is connected with the oxidation of the red colouring matter, and the production, in very old trees, of those fine descriptions of bright red bark which command, indeed, a high price in the market (as much at the present time as eight shillings per pound), but which would not, in many cases, be more valuable for the production of quinine than bark of one year's growth.

I next examined the younger bark of one year's growth, taking care to select the most mature portion, and found that it yielded 2.59 of alkaloid, of which 2.55 (soluble in ether) appeared to be quinine and cinchonidine, and in part crystallised into tolerably white sulphate, which showed perhaps a rather larger proportion of cinchonidine than in the older bark. On the other hand, the proportion of cinchonine seemed notably less, viz., only 0.04 per cent., but it is possible that the separation was not exactly effected between the cinchonine and cinchonidine, which is not easily accomplished by ether in such small portions.

The above result induced me to pay further attention to the leaves, concerning which the absence of any carmine sublimate by heat led me at first to an unfavourable conclusion. The decoctions and infusions made by Mr M'Ivor, though in perfectly good condition, showed that the contents changed most rapidly under the influence of the oxygen of the atmosphere as soon as ammonia was added to the, at first, decidedly acid liquor. Fortunately, a good supply of several ounces of dried leaves had been sent over, and from these I succeeded in obtaining quinine,§ though in very small quantity, but presenting its usual characteristics, dissolving in acids and precipitated by alkalies as a whitish hydrate, soluble in ether, and left by this on evaporation as a resinous-looking body, having the usual bitter taste, also crystallising not only as a sulphate, but as an oxalate of quinine (the latter being the more critical test), but nevertheless, presenting a characteristic implication with resinous or extractive matter such as is usually met with in the very smallest quills or *canutillos* of

* "Illustrations of Nueva Quinologia," under head *C. succirubra*, p. 15.

† "Pharm. Central-blatt," for July 1852.

‡ Described in my "Illustrations of Nueva Quinologia," under head *C. succirubra*, p. 14.

§ [This had been previously done by Dr T. Anderson, Superintendent of the Botanic Garden, Calcutta.—*Ed. Phil. Jour.*]

South American bark, in analysing which it is frequently difficult to purify the quinine from this adhesion. I obtained first from these leaves to the extent of 0.11 of alkaloid, of which part was soluble in ether, the remainder in spirits of wine, and afterwards 0.19 of precipitate still more combined with astringent matter. From these data, it seems to follow that the leaves will not supply a material for the extraction of quinine, but that they will, nevertheless, be very useful when used fresh or in recently prepared decoction or infusion for the cure of the fevers of the country. To this end the abundance of kinovic acid they contain, equal (weighed in the rough state) to 4.20 per cent., may also conduce.

MISCELLANEOUS.

Coagulation of the Blood.—Professor Lister, from recent experiments, concludes that the ammonia theory, however plausible, and though supported by many ingenious arguments and experiments, must be discarded as entirely fallacious. The coagulation is in no degree connected with the evolution of ammonia, any more than with the influence of oxygen or of rest. The real cause of the coagulation of the blood when shed from the body, is the influence exerted upon it by ordinary matter, the contact of which, for a very brief period, effects a change in the blood, inducing a mutual reaction between its solid and fluid constituents, in which the corpuscles impart to the *liquor sanguinis* a disposition to coagulate. This reaction is probably simply chemical in its nature; yet its product, the fibrin, when mixed with blood-corpuscles in the form of an undisturbed coagulum, resembles healthy living tissues, in being incapable of that catalytic action upon the blood which is effected by all ordinary solids, and also by the tissues themselves when deprived of their vital properties. —*Croonian Lecture, Royal Society, 1863.*

Sir William Armstrong on the Consumption of Coal in Britain.—The statistics collected by Mr Hunt, of the Mining Record Office, show that at the end of 1861 the quantity of coal raised in the United Kingdom had reached the enormous total of 86 millions of tons, and that the average annual increase of the eight preceding years amounted to 2½ millions of tons. By combining the known thickness of the various workable seams of coal, and computing the area of the surface under which they lie, it is easy to arrive at an estimate of the total quantity comprised in our coal-bearing strata. Assuming 4000 feet as the greatest depth at which it will ever be possible to carry on mining operations, and rejecting all seams of less than 2 feet in thickness, the entire quantity of available coal existing in these islands has been calculated to amount to about 80,000 millions of tons, which, at the present rate of consumption, would be exhausted in 930 years, but, with a continued yearly increase of 2½ millions of tons, would only last 212 years. It is clear that long before complete exhaustion takes place, England will have ceased to be a coal-producing country on an extensive scale. Other nations, and especially the United States of America, which possess coal-fields thirty-seven times more extensive than ours, will then be working more accessible beds at a smaller cost, and will be able to displace the English coal from every market. The question is, not how long our coal will endure before absolute exhaustion is effected, but how long will those particular coal-seams last which yield coal of a quality and at a price to enable this country to maintain her present supremacy in manufacturing industry. So far as this particular district (Newcastle) is concerned, it is admitted that 200 years will be sufficient to exhaust the principal seams even at the present rate of working. If the production should continue to increase

as it is now doing, the duration of those seams will not reach half that period. How the case may stand in other coal-mining districts I have not the means of ascertaining; but as the best and most accessible coal will always be worked in preference to any other, I fear the same rapid exhaustion of our most valuable seams is everywhere taking place. Were we reaping the full advantage of all the coal we burnt, no objection could be made to the largeness of the quantity, but we are using it wastefully and extravagantly in all its applications. It is probable that fully one-fourth of the entire quantity of coal raised from our mines is used in the production of heat for motive power; but, much as we are in the habit of admiring the powers of the steam-engine, our present knowledge of the mechanical energy of heat shows that we realise in that engine only a small part of the thermic effect of the fuel. That a pound of coal should, in our best engines, produce an effect equal to raising a weight of a million pounds a foot high, is a result which bears the character of the marvellous, and seems to defy all further improvement. Yet the investigations of recent years have demonstrated the fact that the mechanical energy resident in a pound of coal, and liberated by its combustion, is capable of raising to the same height ten times that weight. But although the power of our most economical steam-engines has reached, or perhaps somewhat exceeded, the limit of a million pounds raised a foot high per lb. of coal, yet, if we take the average effect obtained from steam-engines of the various constructions now in use, we shall not be justified in assuming it at more than one-third of that amount. It follows, therefore, that the average quantity of coal which we expend in realising a given effect by means of the steam-engine is about thirty times greater than would be requisite with an absolutely perfect heat-engine.

Bones in Drift.—Sir Charles Lyell, in his last work, suggests that parts of the north of Europe had a climate of intense cold at the time of the supposed co-existence of several extinct quadrupeds with man (the maker of the much-discussed flint implements found in the valley of the Somme and elsewhere). Presuming that to have been the case, may not the same circumstances have prevailed as now occur in Siberia, where numerous extinct animals are perfectly preserved in the stratum of ice which covers parts of that country, and which probably, with its contents, is a remnant of our tertiary period? This ancient stratum of ice is in summer undergoing a constant process of destruction by thawing; and thus the animals imbedded in it are liberated, and their fleshy parts decaying, the bones remain on the surface of the land, or fall to the bottom of the river channels, and, with gravel and mud, must necessarily become so mixed up with the remains of man and of his works, as well as with the bones of other animals now living in that country, that it would, I believe, be impossible for the most skilful observer, after the disappearance of the ice, to declare, either from their state of preservation or from their relative position, that they were not the remains of man and quadrupeds which must have been contemporaneous inhabitants of the land, —leading to the same fallacies which may be observed in some of the conclusions which have of late been too hastily drawn regarding the human remains found in the drift in connection with those of animals, the species of which were probably extinct long before the appearance of man upon the scene.—*Athenæum*.

Illuminating power of the Electric Light.—The following is a short notice of Observations made by Professor W. B. Rogers on the Illuminating Power of one of the large batteries used by Mr Ritchie of Boston, United States, in the recent grand exhibitions of the Electric Light in that city:—"The battery in question, consisting of 250 Bunsen

pairs of the largest size commonly used, was arranged in the dome of the State House, and the carbon light and the photometric apparatus prepared for the purpose were placed in line across the same apartment, commanding a range of about 50 feet. In view of the immense power of the light as observed in the previous experiment, I substituted for the 20-candle gas-burner, used at that time as the standard of comparison, a unit ten times as great, formed by the flame of a Kerosene lamp placed in the focus of a small parabolic reflector, and throwing its concentrated light on a photometric screen of prepared paper fixed in front of it, at the distance of five feet. Before the observation the lamp and reflector were so adjusted as to make the light cast on the near side of the screen equivalent by measure to the action of 200 candles. A platform supporting the standard lamp and screen at the assigned distance was arranged to slide on a horizontal graduated bar, extending directly towards the carbon point, so that the screen should receive the rays from the electric light, and from the reflector perpendicularly on its opposite faces. In making the observations, the platform was moved to and fro until the illumination on the opposite sides of the screen was judged to be equal, and then the measured distances of the two antagonising lights from the screen gave by easy computation their relative illuminating power. By a series of such observations it was found that the carbon light had a force varying from 52 to 61 times that of the lamp with reflector, making it equivalent in illuminating power to the action of from 10,000 to 12,000 standard sperm candles pouring their light from the same distance upon the surface of the screen. This it will be remembered is the effect of the unaided carbon-light sending its rays equally in all directions from the luminous centre, and falls vastly short of the illuminating force of the cone of collected rays which was seen stretching like the tail of a comet from the surface of the great reflector. Judging from some recent experiments on the power of such a reflector to augment the intensity of the light emanating from its focus, there can be no doubt that along the axes of the cone when brought to its narrowest limits—the illuminating force of the carbon light as displayed on the State House could be rivalled only by that of several millions of candles shining unitedly along the same line.”

PUBLICATIONS RECEIVED.

1. Sur la Statistique Générale des Différents Pays. Par M. Ad. Quetelet.—*From the Author.*
2. Annuaire de l'Académie Royale des Sciences, des Lettres, et des Beaux Arts de Belgique, 1862–63.—*From the Academy.*
3. Bulletin de l'Académie Royale des Sciences, des Lettres, et des Beaux Arts de Belgique, 30^{me} Année, 2^{de} Serie, tom. XI. et XII., 1861 ; 32^{de} Année, 2^{de} Serie, tom. XV., Parts 1–6, et 32^{de} Année, 2^{de} Serie, tom. XVI., Part 7, 1863.—*From the same.*
4. Proceedings of the Literary and Philosophical Society of Manchester. Nos. 11 and 12, Session 1862–63.—*From the Society.*
5. Proceedings of the Academy of Natural Sciences of Philadelphia, Nos. 7–12, for 1862 ; Nos. 1 and 2, for 1863.—*From the Academy.*

6. *Journal of the Academy of Natural Sciences of Philadelphia.* New Series, Vol. V., Parts 2 and 3.—*From the same.*

7. *Journal of the Chemical Society.* 2d Series, Vol. I., Nos. 3-5, and No. 9, for 1863.—*From the Society.*

8. *Journal of the Asiatic Society of Bengal,* No. 5, for 1862, and No. 2, for 1863.—*From the Society.*

9. *Memoirs of the Geological Survey of India.* 2d Series, Part 3.—*From J. Oldham, Esq., Superintendent of the Survey.*

10. *American Journal of Science and Arts* for March, 1863.—*From the Editors.*

11. *Transactions of the Manchester Geological Society.* Vol. IV., No. 5, Session 1862-63.—*From the Society.*

12. *A New Theory of Muscular Action.* By the Rev. Samuel Haughton, M.D., F.R.S.—*From the Author.*

13. *Report of the Proceedings of an International Congress on the Subject of Cruelty to Animals, and especially Vivisection.*—*From the Society for the Prevention of Cruelty to Animals.*

14. *Holmes' Magneto-Electric Light as applicable to Lighthouses.*—*From the Proprietors.*

15. *Palæontologica Indica.* Parts 2-4 and 2-5.—*From Dr Oldham.*

16. *The Analogy of Thought and Nature.* By E. VANSITTART NEALE, —*From the Publishers.*

17. *Comptes Rendus,* from January 1859 to June 1862.—*From the Editors.*

18. *Report of Lieut.-Colonel Graham.* United States Topographical Engineer, on Mason and Dixon's Line.—*From the Author.*

19. *Transactions of the Academy of Science of St Louis.* Vol. II. No. 1.—*From the Academy.*

20. *Report of the Superintendent of the United States Coast Survey, for the years 1859 and 1860.* 2 Vols.—*From the Smithsonian Institution.*

21. *Catalogue of the Army Medical Museum,* Washington, D.C.—*From the same.*

22. *Annual Report of the Board of Regents of the Smithsonian Institution.*—*From the same.*

23. *Canadian Naturalist and Geologist,* Nos. 1-3 (February, April, June), for 1863.

24. *An Inquiry into the Nature of Heat, and into its Mode of Action in the Phenomena of Combustion, Vaporization, &c.* By ZERAH COLBURN.—*From the Publishers.*

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THE
EDINBURGH NEW
PHILOSOPHICAL JOURNAL,

EXHIBITING A VIEW OF THE
PROGRESSIVE DISCOVERIES AND IMPROVEMENTS
IN THE
SCIENCES AND THE ARTS.

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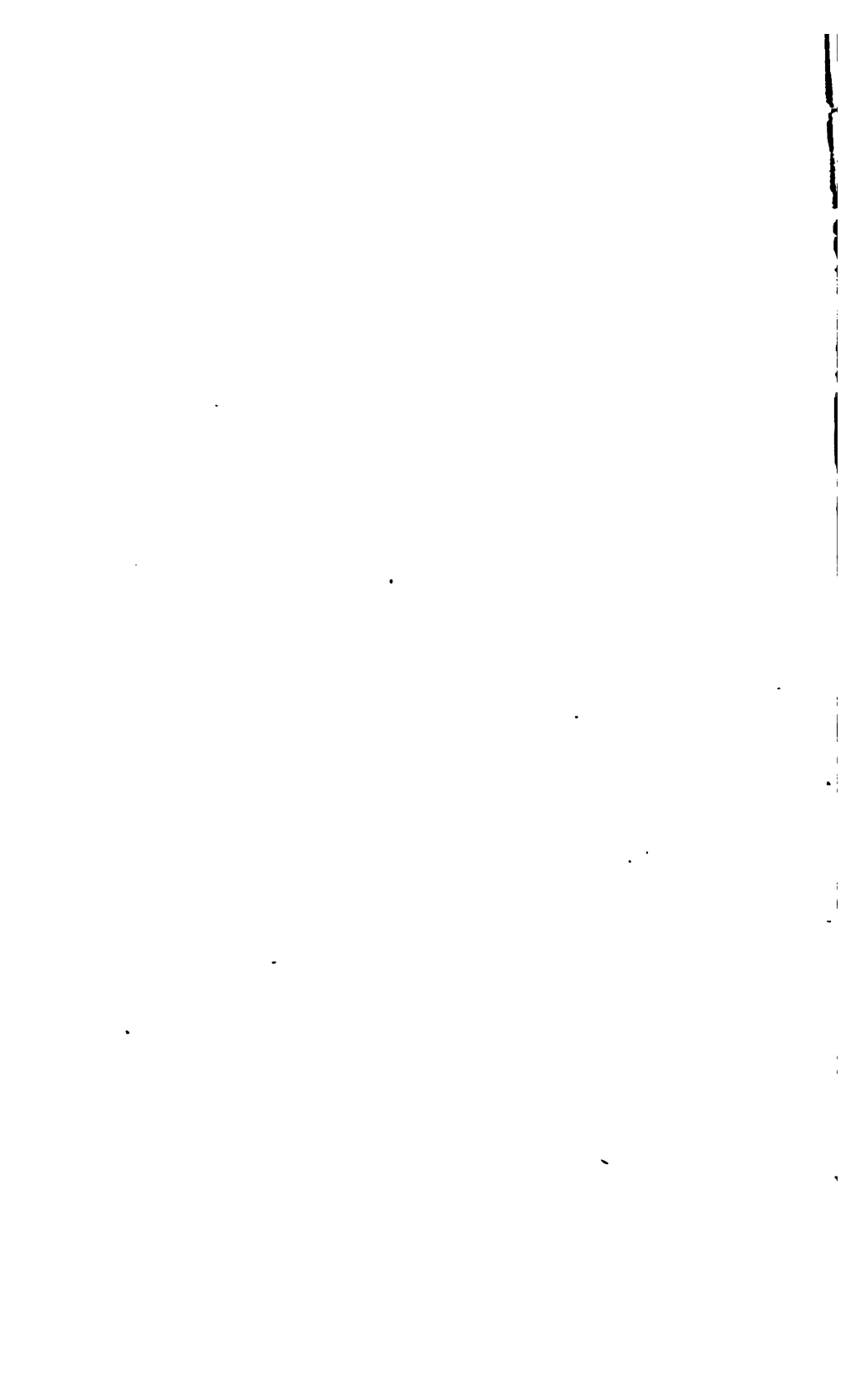
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THE
EDINBURGH NEW
PHILOSOPHICAL JOURNAL.

On some Anomalies in Zoological and Botanical Geography.
By ALFRED R. WALLACE.

THE subject of Geographical Distribution is now generally allowed to be one of the most interesting branches of Natural History, and owing to the accumulation of much trustworthy material within the last few years, we are at length enabled to generalise many of the most important facts, and to form a tolerably accurate idea of the import and bearing of the whole inquiry.

In the admirable chapters on this topic in the "Origin of Species," Mr Darwin has given us a theory as simple as it is comprehensive, and has besides gone into many of the details so fully as to render it needless to say another word here on those parts of the question which he has treated. As an explanation of the main facts, and of many of the special difficulties, of geographical distribution, those chapters are in every respect satisfactory; and I therefore propose now to consider only the anomalies and discrepancies which so frequently occur between the distribution of one class or order and another, and to discuss the possibility of arriving at a division of the earth into Regions, which shall represent accurately the main facts of distribution in every department of nature.

In doing this I shall consider in detail a few cases of special difficulty only, and endeavour to establish certain

principles, which, if accepted, will enable us to deal with such cases for the future, and avoid the confusion into which the whole question must necessarily fall, if (as has hitherto been the case) every naturalist proposes a distinct set of geographical regions for the group to which he pays most attention.

The entire subject naturally comes under the two heads of *terrestrial* and *marine* distribution, which may be treated of independently, but upon similar principles. I now confine myself entirely to the terrestrial division. The chief fault of the Zoological and Botanical regions that have been hitherto proposed is, that they have generally been too numerous, and have been more or less artificially bounded by lines of latitude and longitude. Those established by Meyen for plants, and by Woodward for shells, have this fault, and were, besides, never intended to apply to the whole organic world. Swainson's division (*Geog. and Class. of Animals in Lardner's Cab. Cyc.*), was much more natural, and was, I believe, the first that took into consideration all classes of animals, and can lay any claim to rank as a general system. But by carrying out even here his favourite quinary theory, and by following too closely the supposed typical races of man, he was led into many important errors,—such as including the northern and southern continents of America in one region, and placing Northern Asia with India rather than with Europe.

In June 1857, a paper was read before the Linnean Society by Dr Sclater, entitled, “On the general Geographical Distribution of the Members of the Class Aves,” which marks an era in this branch of natural history. The subject was now for the first time treated in a philosophical manner by a naturalist well acquainted with the whole class with which he proposed to deal, and who, by looking chiefly to groups,—to genera and families rather than to species—and by taking account of broad contrasts rather than local peculiarities, has succeeded in marking out upon the globe those divisions, which not only represent accurately the great facts presented by the distribution of birds, but seem also well adapted to become the foundation for a general system of Ontological regions.

The following six Regions are those established by Dr Sclater :—

1st, The Neotropical, comprising South America, Mexico, and the West Indies ; 2d, The Nearctic, including the rest of America ; 3d, The Palæarctic, composed of Europe, Northern Asia to Japan, and Africa north of the Desert ; 4th, The Ethiopian, which contains the rest of Africa and Madagascar ; 5th, The Indian, containing Southern Asia and the western half of the Malay Archipelago ; and 6th, The Australian, which comprises the eastern half of the Malay Islands, Australia, and most of the Pacific Islands. Each of these regions is characterised by a number of peculiar genera, and even families of birds, which, while found everywhere within the region, do not pass over its boundaries ; and by other genera which, though found sparingly in several regions, have their metropolis in one. This scheme of Ornithological distribution has been founded on such an extensive basis of facts, and after having been five years before the world has met with such general acceptance, that it may fairly be taken as established, subject only to modifications of the dividing lines between those regions which gradually merge into each other.

It remains to be shown whether this is not only a true Ornithological, but also a true Zoological and Botanical division of the earth ; and if not, to show how it is that what is true for one part of nature should not be equally true for all.

In a paper on the " Geographical Distribution of Reptiles" (*Proc. Zool. Soc.* 1858, p. 373), Dr Gunther has shown that for snakes and batrachians the same divisions will almost exactly apply ; the only important discrepancy being that Japan, judging from its snakes, would belong to the Indian region, while its batrachians are decidedly related to those of the Palæarctic region.

In Mammalia the same geographical divisions are very strongly marked, but here again one important discrepancy has been pointed out, namely, that the quadrupeds of North Africa are of Ethiopian, while the birds and reptiles are of European forms. (*Ibis*, vol. i. pp. 93, 157.

In the immense class of Insects, very little has been done

to work out the details of Geographical distribution. There is no doubt but that the six regions marked out by Dr Selater are generally characterised by distinct forms of insects. There is one case, however, which has come under my own observation, in which the entomological would not correspond with the ornithological regions. The Moluccas and New Guinea in their birds and mammals are most decidedly Australian, while the insects show a general correspondence with the Indian type. It has also been pointed out that the insects of Chili and of south temperate South America have little affinity with Neotropical forms.

Land shells, I am informed by the Rev. H. B. Tristram, generally agree very well with the ornithological regions. The subdivisions or provinces are, however, often very strongly marked.

In Plants, I am informed by Dr Hooker, the regions will in many cases not at all correspond.

In order to arrive at the cause and meaning of these singular differences in the Geographical distribution of the various classes, we must inquire how Zoological and Botanical regions are formed, or why organic existences come to be grouped geographically at all.

It appears to me that this can be explained by a few simple principles—

1st, The tendency of all species to diffuse themselves over a wide area, some one or more in each group being actually found to have so spread, and to have become, as Mr Darwin terms them, dominant species.

2d, The existence of barriers checking, or absolutely forbidding that diffusion.

3d, The progressive change or replacement of species, by allied forms, which has been continually going on in the organic world.

4th, A corresponding change in the surface, which has led to the destruction of old and the formation of new barriers.

5th, Changes of climate and physical conditions, which will often favour the diffusion and increase of one group, and lead to the extinction or decrease of another.

By means of these principles we will endeavour to ex-

plain the discrepancies already mentioned. And, first, how is it that the snakes in Japan are Indian and the batrachians Palæarctic? Dr Gunther informs us, in the paper already alluded to, that snakes are a pre-eminently tropical group, decreasing rapidly in the temperate regions, and absolutely ceasing at 62° N. Lat. Batrachians, on the other hand, are almost as fully developed in northern as in tropical regions. They can support intense cold, and are, moreover, more diffusible, geographically, than snakes. These facts furnish a clue to the peculiarities of the Japanese reptile fauna. For let us suppose that Japan once formed a part of northern Asia (with which it is even now almost connected by two chains of islands), it would then have received its birds, mammals, and batrachians from the Palæarctic region; but there could have been few or no snakes, owing to the much lower curve of the isothermal lines in Eastern Asia than Western Europe, giving to Mandtchouria a climate as rigorous as that of Sweden. Now, at a subsequent period, Japan must have been connected with Southern Asia through the line of the Loo-choo and Madjicosima islands, and would then acquire its population of Indian forms of snakes, which would easily establish themselves in an unoccupied region,—whereas the batrachians, as well as the birds and mammals of Southern Asia, would find a firmly established Palæarctic population ready to resist the invasion of intruders, and it is therefore not to be wondered at that but few, if any, Indian forms of these groups should have been able to maintain themselves. Again, the insects of Japan are decidedly Palæarctic in character, except in the case of a few tropical forms of diurnal lepidoptera, which would have been able to establish themselves, like the snakes, on account of the extreme poverty of that group in high latitudes. It would thus appear that the tropical character of the snakes is quite exceptional, depending upon the fact of the whole group being pre-eminently tropical, and can therefore not be held to throw any doubt on the position of Japan in the Palæarctic zoological region.

We have next to consider the supposed discrepancy in the mammals of Algeria compared with the birds, reptiles, insects, and plants—all of which are decidedly of Palæarctic

forms. It will, I think, be found that the facts have here been somewhat hastily assumed, and that the mammalia do not differ very much in this respect from other classes, and in no degree invalidate the position that North Africa belongs to the Palæarctic region. Leaving out domesticated animals, I have drawn up a list of the genera of Algerian mammals (from Captain Loche's Catalogue), and have divided the species, so as to show how far they correspond with those of the Palæarctic or other regions.

From an examination of this table, it will be seen that thirty-three of the Algerian mammals are absolutely identical with European or West Asian species, fourteen more are representative species of European genera, and ten belong to West Asian and Siberian (and therefore Palæarctic) genera, giving a total of fifty-seven species and about twenty-eight genera, as the measure of Palæarctic affinity. Now, to balance this, what have we to indicate an Ethiopian fauna? The most important, and what have probably been most relied on as giving an extra-European character to the country, are the four large felines,—the lion, the leopard, the serval, and the hunting-leopard,—but as these all range the whole of Africa, from the Cape to the Mediterranean, and may very probably have crossed the desert in the tracts of caravans, they cannot be held to have much weight on the present question. Then there is the solitary monkey; but as that actually inhabits Europe, we need hardly have included it among the representatives of Ethiopian groups, except to give all the facts that can be fairly claimed on that side. The antelope is a desert-haunting species, and therefore may be looked upon as a straggler on the northern side of the Sahara; and, besides these, we have representatives of two really African genera (*Macroscelides* and *Zorilla*), giving a total of only eight species as the measure of Ethiopian affinity. The remaining species, seven in number, are true desert-haunters, roaming over North Africa, Egypt, and Arabia, into the Indian deserts, and have scarcely any more right to be considered as belonging to one region than another, since they inhabit the district which forms the boundary and debateable land of the Ethiopian, Indian, and Palæarctic regions.

in Zoological and Botanical Geography.

Gènera of Algerian Mammals.		Palearctic Species.	Species of Palearctic Gènera.	Ethiopian Species and Gènera.	Asiatic or Desert Species and Gènera.
Carnivora.	Macacus innuus	1	..	1	...
	Ursus	1	...	...	...
	Canis	1	3	...	...
	Fenecus	...	...	...	1
	Hyena	...	1	...	...
	Meles	1	...	...	...
	Mangusta	...	...	...	1
	Genetta	1	1	...	...
	Felis	1	...	4	1
	Putorius	3	...	...	...
Ruminantia.	Zorilla	...	...	1	...
	Lutra	1	...	...	...
	Sus	1	...	...	...
	Cervus	1	...	...	...
	Dama	1	...	...	...
	Antelope	...	...	1	...
Insectivora.	Gazella	...	...	...	2
	Musimon	...	1	...	...
	Vespertilionidæ	8	...	...	...
	Sorex	2	...	...	...
	Pachyura	...	1	...	...
	Crocidura	1	...	...	...
Rodentia.	Crossopus	1	...	...	...
	Macroscelides	...	...	1	...
	Erinaceus	...	1	...	...
	Hystrix	1	...	...	...
	Myoxus	...	1	...	...
	Dipus	1	2	...	...
	Alactaga (Siberian Genus)	...	1	...	...
	Lepus	1	1	...	...
	Gerbillus (N. and W. Asian Genus)	...	7	...	...
	Ctenodactylus (Asiatic Group)	...	...	...	1
	Sciurus	...	1	...	...
	Mus	5	3	...	...
		33	24	8	6

It would seem, therefore, that the supposed discrepancy of the Mammalia, in determining the southern limit of the Palæarctic province, is altogether imaginary. The number of species absolutely identical is not so great as in the birds; but Europe is not the whole Palæarctic province, and if we take *genera* instead of *species*, we shall find the correspondence as complete as possible,—twenty-eight genera being truly Palæarctic, only three Ethiopian, while five are Asiatic, or desert-dwellers. In this case, therefore, the whole of the vertebrata combine with the insects, the land shells, and the plants, to place North Africa in the Palæarctic region.

The case of the insects in the Australian portion of the Malay Archipelago is one of much greater difficulty. Australia itself contains a remarkable assemblage of insects, among which its Lamellicornes, Buprestidæ, and Curculionidæ offer a number of striking forms and genera quite peculiar to it. In New Guinea and the Moluccas, on the other hand, Lamellicornes are comparatively scarce, and with the Buprestidæ and Curculionidæ are of Indian rather than Australian genera; while the great family of the Anthribidæ, which is almost entirely absent in Australia, is here everywhere abundant in genera, species and individuals, though less so than in the Western or Indian region.

To account for this remarkable discrepancy, we must consider,—*1st*, That insects are much more immediately dependent on the character of the vegetation, and therefore on climate, than are vertebrated animals; and, *2dly*, That water-barriers are much less effective in preventing their dispersion. A narrow strait is an effectual bar to the migration of mammals and of many reptiles and birds, while insects may be transported in the egg and larva state by floating timber, and from their small size and great powers of flight, may be easily carried by the winds from one island to another. Now, the characteristic insects of Australia seem specially adapted to a dry climate and a shrubby flower-bearing vegetation, and could hardly exist in the excessively moist atmosphere and amid the dense flowerless forests of the equatorial islands. If, therefore, we suppose Australia itself to be the most ancient portion of this

region (which its great richness in peculiar generic forms seems to indicate), we can easily understand how, when the islands of the Moluccas and New Guinea first rose above the waters and became clothed with dense forests nurtured by tropical heat and perpetual moisture, though the birds and mammals readily adapted themselves to the new conditions, the insects could not do so, but gave way before the immigrants from the islands to the west of them, which having been developed under similar climatal conditions, and thus become specially adapted to them, were enabled, by the enormous powers of multiplication and dispersion possessed by insects, at once to establish themselves in the newly-formed lands, and develop an insect population in many respects at variance with other classes of animals.

There are, however, several instances of groups of insects almost as strictly confined to one-half of the Archipelago as is so remarkably the case with the vertebrata; and when the extensive collections made by myself in most of the islands come to be accurately worked out, no doubt more such instances will be found. Among Coleoptera I may mention the *Tmesisterninae*, a remarkable sub-family of Longicornes, as being strictly confined to the Australian region, over the whole of which it extends, and has its western limit in Celebes along with the Marsupials and the Trichoglossi. Again, Mr Baly, so well known for his acquaintance with the Phytophagous Coleoptera, finds that one of the principal sub-families of that tribe (Adoxinæ), which he has recently classified, though spread over Europe and the whole of Asia, is only found in the Archipelago in those islands which belong to the Indian region of zoology. This proves that there is an ancient insect-population in the Austro-Malayan Islands, which accords in its distribution with the other classes of animals, but which has been overwhelmed, and in some cases perhaps exterminated, by immigrants from the adjacent countries. The result is a mixture of races, in which the foreign element is in excess; but naturalists need not be bound by the same rule as politicians, and may be permitted to recognise the just claims of the more ancient inhabitants, and to raise up fallen nationalities. The aborigines, and not the invaders, must

be looked upon as the rightful owners of the soil, and should determine the position of their country in our system of Zoological geography.

My friend, Mr Bates, has kindly furnished me with some facts as to the entomology of Chili and south temperate America, which would show that the insects of this region have very little connection with those of tropical America.

Out of ten genera of butterflies found in Chili, not one is characteristic of tropical America. Four (*Colias*, *Argynnis*, *Erebia*, and *Satyrus*) are northern forms, only one of which occurs at all in tropical America, and that high up in the Andes; three others are peculiar to Chili, but have decided north temperate or Arctic affinities; and three more (*Anthocharis*, *Lycæna*, and *Polyommatus*) are cosmopolitan, but far more abundant in temperate than tropical regions. Judging, therefore, from butterflies only, we should decidedly have to place south temperate America in the Ne-arctic region, or form it into a region by itself.

Two important families of Coleoptera, the Geodephaga and the Lamellicornes, furnish different but equally remarkable results. There are 77 genera of these families found in Chili, of which 46 are peculiar to south temperate America, being $\frac{3}{4}$ ths of the whole; 17 are cosmopolitan, 2 are north temperate, 10 tropical American, and 1 is African.

But of the 46 peculiar genera, no less than 10 are closely allied to Australian forms, and 3 to South African,—so that the affinities of these groups of coleoptera are almost as strong to Australia as to tropical America; next comes South Africa, and, lastly, the north temperate zone; though as the two genera *Carabus* and *Geotrupes* are very extensive and important, and are totally absent from the tropics, but appear again in Chili, the real amount of affinity to northern regions may be taken as somewhat larger.

Here, then, as only 10 genera out of 77 are common to south temperate and tropical America, and as the remainder have wide-spread affinities—to the northern hemisphere, to Australia, and to South Africa—it would seem impossible, from a consideration of these families of Coleoptera alone, not to separate the south temperate zone of South America as a distinct primary region.

Other orders of insects and other families of Coleoptera may very probably give somewhat different results. From Boheman's work on the Cassididæ, I find that the genera of tropical America send representatives into Chili, and even into Patagonia, and that none of the south temperate forms have a direct affinity with those of Australia. But this family is almost exclusively tropical, very few and obscure species inhabiting the colder regions of the earth, while there are no generic forms peculiar to the Australian region.

In many of the preceding facts we have a most interesting correspondence with those furnished by the distribution of plants. Dr Hooker has shown the large amount of resemblance between the flora of southern South America and Australia, especially Tasmania and New Zealand,—one-eighth of the whole New Zealand flora being identical with South American species. Again, the occurrence of northern genera of coleoptera in Chili, and the whole of the butterflies having northern affinities, agrees with the number of northern genera and species of plants in Patagonia and Fuegia, and is an additional proof of the intensity and long continuance of the glacial epoch which sufficed to allow so many generic forms to pass the equator from north to south.

We have here another illustration how much easier of diffusion, and how much more dependent on local conditions are insects than the higher animals. A great part of the southern portion of America is of more recent date than the central tropical mass, and must have had at one time a closer communication than at present with the antarctic lands and Australia, the insects and plants of which, finding a congenial climate, established themselves in the new country, being only feebly opposed by the few northern forms which had already, or soon after, migrated there. And the fact that Tasmania and New Zealand are the poorest countries in the world in butterflies, will enable us to understand how it is that all those found in Chili are northern forms, while the coleoptera of the same countries (Tasmania and New Zealand) being tolerably abundant and varied, and having a shorter journey to perform than the north temperate immigrants, were enabled to get the upper hand in colonizing the new country.

The marsupial Opossums are the most remarkable case of vertebrata in America having Australian affinities. It is very doubtful whether these could have been introduced in the same manner as the plants and insects already alluded to, because the latter have to a considerable extent an antarctic character, and do not appear in such numbers as to indicate an actual continuity of land, which would have been almost indispensable for the passage of mammalia, and would at the same time have undoubtedly admitted Australian forms of land birds, which do not exist in South America. It seems more reasonable, therefore, to suppose that these marsupials have inhabited America since the Eocene period, when the same genus existed in Europe, and the marsupial order had probably a universal distribution.

With this one exception, the birds, the mammalia, and the reptiles* of south temperate America have little or no affinity either with north temperate or Australian forms, but are modifications of the true denizens of the Neotropical regions. They appear to have been enabled rapidly to seize hold of the country, and to adapt themselves to its modified climate and physical features—a remarkable instance of which is mentioned by Mr Darwin in the woodpecker of the Pampas, which never climbs a tree. The tropical insects, on the other hand, having become gradually specialized during long periods for a life amid continual verdure and unvarying summer, were totally unfitted for the new conditions presented to them, and only in a very few cases were able to struggle against forms already adapted to a more barren country and a more rigorous climate.

This difference in the adaptive capacity of groups, combined with an unequal power of diffusion, will cause the various kinds of barriers to be sometimes more and sometimes less effective. For example, when a mountain range has attained only a moderate elevation, it will already completely bar the passage of many insects, while mammalia, birds, and reptiles, more capable of sustaining different conditions, will readily pass over it. On the otherhand, an arm of the sea, or even a wide river, will completely

* Except the batrachians, which show some affinities between Australia and South America, a case analogous to that of Japan.

isolate most mammals and many reptiles, while insects have still various means of passing it.

Another consideration which must help to determine the amount of specific peculiarity in a given region, is the average rate at which specific forms have changed. Palæontologists have determined that mammalia have changed much more rapidly than mollusca, from the phenomena of the comparatively recent extinction of so many species of mammals, whose remains are found along with existing species of shells. From the evidence of the distribution of existing species, birds would appear to have changed at least as quickly as mammals, and insects, in some cases, perhaps more so; owing, no doubt, to their very small diffusibility, and the readiness with which they are affected by local conditions.

Taking the various facts and arguments now brought forward into consideration, it appears evident that no regions (be they few or many in number) can be marked out, which will accurately represent the phenomena of the geographical distribution of all animals and plants. The distribution of the several Classes, Orders, and even Families, will differ, because they differ in their diffusibility, their variability, and their mode of acting and reacting on each other, and on the external world. At the same time, though the details of the distribution of the different groups may differ, there will always be more or less general agreement in this respect, because the great physical features of the earth—those which have longest maintained themselves unchanged—wide oceans, lofty mountains, extensive deserts—will have forbidden the intermingling or migration of all groups alike, during long periods of time. The great primary divisions of the earth for purposes of natural history should, therefore, correspond with the great permanent features of the earth's surface—those that have undergone least change in recent geological periods. Later and less important changes will have led to discrepancies in the actual distribution of the different groups; but these very discrepancies will enable us to interpret those changes, of which they are the direct effects, and very often the only evidence.

From this examination of the anomalies that occur in

the distribution of different groups, and of the probable causes of such anomalies, it appears that the six regions of Dr Sclater do approximately represent the best primary divisions of the earth for natural history purposes. They agree well with the present distribution of mammalia, birds, reptiles, land shells, and very generally of insects also. The cases in which they do not seem correct are those of isolated groups in restricted localities. The greatest discrepancies occur in groups which have at once great capacities for diffusion, and little adaptability to change of conditions; and, in the case of plants, have probably been much increased by what may be called the adventitious aid of the glacial period and of floating ice.

Of botanical distribution I have said little, from want of knowledge of that branch of the subject, and I can find no detailed information bearing directly upon the questions here discussed, but what I have already mentioned. It is much to be desired that some competent botanist would point out how far these regions agree with, and how far they contradict, the main facts of the distribution of plants. It seems evident that the various modes of glacial action have produced much more effect on the migrations of plants than on those of animals, and also that plants have, on the whole, more varied and more effectual means of dispersal. Still, if the views here advocated are true, the flora of each region should exhibit a characteristic substratum of indigenous forms, though often much modified, and sometimes nearly overwhelmed by successive streams of foreign invasion.

My object in calling attention to the subject by this very partial review of it, is to induce those naturalists, who are working at particular groups, to give more special attention to geographical distribution than has hitherto been done. By carefully working out the distribution of allied genera and closely connected groups of species, they could give the amount of agreement or discrepancy with other groups whose geography is best known, and furnish us with such information on the habits of the species, as might help to explain the anomalies which were found to occur. We should thus soon accumulate a sufficiency of detailed facts to enable us to determine whether these are the best pri-

mary divisions of the earth into terrestrial Zoological and Botanical regions, or whether such general divisions are altogether impracticable. Some such simple classification of regions is wanted to enable us readily to exhibit broad results, and to show at a glance the external relations of local faunas and floras. And if we go more into detail, and adopt a larger number of primary divisions, we shall not only lose many of these advantages, but shall probably find insuperable difficulties in harmonising the conflicting distribution of the different groups of organised beings.

On the Slaking of Quicklime. By JOHN DAVY, M.D., F.R.S.,
Lond. and Edin.*

IN some experiments which I have made on the slaking of quicklime, as its conversion into a hydrate is commonly called, I have noticed certain results new to me, and as I cannot find them noticed in any chemical work I have referred to, I propose to give a brief account of them on the possibility that they may be new to others.

It is well known that as soon as water is added to and absorbed by well-burnt lime fresh from the kiln, an immediate union takes place, the mass becoming broken up and falling into powder, with the production of much heat and steam.† But if the lime has been kept exposed to the air for two or three days, during which time it absorbs a small quantity of water,‡ without at all disintegrating, the same rapid union is not witnessed on the addition of water sufficient to form a hydrate; on the contrary, some minutes will elapse before the combination takes place, and I find there is a similar retardation of action from other causes as shown by the results of the following experiments:—

1. To a piece of lime taken from a mass, such as that

* Read at the Meeting of the British Association for the Advancement of Science held at Newcastle. (1868.)

† Gunpowder and sulphur have been ignited by it. See *Annales de Chem. et de Phys.* xxiii. p. 217. About six pounds were slaked.

‡ A little carbonic acid is absorbed at the same time, but this latter is not essential, inasmuch as the lime exhibits the same peculiarity if kept in damp air, excluding carbonic acid.

adverted to, ten grains of water were added. The whole of the water was absorbed. As I held it, a very slight sensation of warmth was perceived by the fingers in contact with it—a sensation that occurred immediately, and for about eight minutes it did not distinctly increase. About that time small cracks began to appear at the surface of the piece; the temperature instantly sensibly rose, and in another minute the heat became too great to be bearable. Now, put down, in a few seconds it became rent, and the hydrate was formed.

2. In a piece of lime of two or three pounds, a hole was bored an inch and half in depth, sufficiently large to admit the bulb of a thermometer. Water, no more than the lime could absorb, was next poured on the mass. The thermometer, from 55° , immediately rose to 70° . During about eight minutes little change of temperature was observed; then, in less than a minute, the thermometer rose to 280° , accompanied with the production of steam and the falling to pieces of the mass.

3. Into a small receiver, $2\frac{1}{4}$ inches high, $1\frac{1}{4}$ inch in diameter, quicklime in fine powder (just pounded) was put in sufficient quantity to fill it to about two-thirds. A thermometer then introduced stood about 60° . Next added water, more and more, till the whole of the lime appeared to be moistened. This done, in about half a minute, the thermometer stood at 80° ; in about five minutes it had fallen to 78° ; then it began to rise; in one minute it had risen to 100° , in less than half a minute more to 120° ; then, after a few seconds, an explosion took place, the thermometer was thrown out and the lime was scattered, some even beyond twelve feet.

4. Into the same receiver about the same quantity of pounded lime was introduced with excess of water, and the mixture was immediately stirred. The lime subsided on rest; there was about one-tenth of an inch of superincumbent water. After about twenty minutes the temperature had become a little higher; in about five minutes more steam was produced, but there was no explosion.

5. To a piece of quicklime, weighing 88.3 grs., 9.2 grs. of water were added. A slight increase of temperature was

produced. After about a quarter of an hour it became warm, but quite bearable; portions fell off one after another, but no high temperature was produced. When cool, it was again weighed; its weight was now 93·8 grs.; so it had gained only 5·5 grs. of water. The portions which had fallen off, put into water, were some minutes before they became hot; then they fell to powder, and became a thick paste.

6. A piece of quicklime, weighing 310 grains, was kept an hour and a half in a close vessel with some damp paper. During this time it had increased in weight only half a grain. Now added gradually, in about half a minute, 19·5 grains of water. The water, in minute quantity, was applied successively to different parts of the mass. There was no sensible increase of temperature; 13·5 grains more of water were added; still no increase of temperature. After twenty-four minutes 6 grains more were added, without perceptible effect; and an hour later 6 more, making a total of 45 grains. Shortly after some action had taken place, and a portion had fallen to powder with evolution of heat; next morning the whole mass was found broken up and reduced to the state of powder.

7. To 76 grains of quicklime reduced to fine powder, 11 grains of water were added in two portions, triturating the powder on each addition. I am not aware of any heat having been produced, nor did I see indications of any action. The dry powder was put into a tube and tightly corked. On the day following there was no perceptible change. Now added water in great excess, so as to form a thick paste. After a few minutes there was a slight increase of temperature; after twelve, it had become moderately warm, and it continued so some time, showing the slow formation of the hydrate.

Do not these results warrant the conclusion, that lime is capable of uniting feebly with less water than is required to form the hydrate, that consisting of one proportion of each, the weaker compound containing probably two proportions of lime. In the last-mentioned experiments the quantity of water was nearly in accordance with this composition.

I shall now relate one or two other results which are rather favourable to this conclusion, and at the same time show how unstable is the union (if it be admitted) with the smaller proportion of water.

8. To 39·5 grs. of quicklime added gradually 4 grs. of water. A little heat was produced, but a bearable one. Now plunged the mass into cold water, moving it rapidly for about a minute; it remained cool. Transferred it now to the balance; it had gained 8 grs. In a few seconds action took place, and the hydrate was formed. This experiment has been more than once repeated with the same result.

9. Instead of mere water a mixture of about equal parts of water and sulphuric ether was poured on a piece of quicklime. The immediate effect was the cooling of the little mass by the evaporation of the ether. For many minutes there was a retardation of action, and when it began it went on slowly, the evaporation probably interfering, and preventing rapidity of combination.

10. If, instead of cooling the quicklime, its temperature be at all raised, so much the more rapidly is the hydrate formed on the addition of water. Thus, on pouring a few drops of water on a small piece of quicklime fresh from the fire, allowed to cool, so that its warmth was hardly to be felt, no sooner did the water touch it than the union took place with explosive violence, driving the little fragments to the distance of several feet.

There are other results which I have obtained of like significance.

11. If aqua ammoniæ, or a strong solution of common salt, or of chloride of calcium—compounds having an affinity for water, and not readily parting with it—be added to quicklime, the formation of the hydrate is more or less retarded, but is nowise prevented, and when it takes place it is sudden, with the usual phenomena as to evolution of heat, &c.

12. A similar retardation is witnessed when quicklime in mass is put into a solution of the carbonate or sesquicarbonate of ammonia, or of the carbonate or bicarbonate of potash; but when action commences it is rapid, the hydrate of lime being formed, with the usual production of heat.

But if the lime be reduced to the state of a very fine powder, and mixed (using trituration) with a strong solution of either, a carbonate of lime is formed with a very slight elevation of temperature—that small degree referrible, I believe, to the portion of hydrate at the same time produced.

On the supposition that two of lime can unite with one proportion of water, the instability of the compound is no more than might be expected. There are many analogous examples, such as the sesquicarbonate of ammonia, the neutral carbonate of this alkali, and the bicarbonate of potash and soda. *A priori*, we cannot predicate the chemical relation of one body to another; it may be conjectured, but it can only be determined by experiment. It might be supposed, that because carbonic acid is expelled from lime by a bright red heat, that it would not combine with this acid at a dull red heat. Yet this I find is the case.

Considering the high temperature produced in the act of union of water and lime, and the quantity of steam that may be generated, the idea could hardly fail to occur, that the formation of the hydrate may be applied to some useful purpose, such as the blasting of rocks; and if successful, might be especially useful in collieries as a substitute for gunpowder, which has so often occasioned, by the igniting of gas, terrible accidents with loss of life.

The few trials I have instituted, with a view to this application, have not answered my expectations. I shall mention one or two of the latest I have made. Recently I had a boring made in a block of sandstone, about 15 inches deep and 2 inches in diameter. It was filled with small pieces of quicklime; water was poured in, which, it was inferred, found its way to the bottom in sufficient quantity, and the hole was then firmly closed by a plug of wood. No rending of the rock was produced; yet the hydrate was formed. It must be concluded that the elastic expansive force exerted was not superior to the resistance, and that all the steam was condensed. A second experiment was made, substituting for the boring in rock a strong earthenware jar, capable of holding about a quart. It was similarly charged and tightly corked; the cork bound down

firmly by a cord. After about fifteen minutes an explosion took place. The report was like that of a pistol. The jar was broken into several pieces, and some of them were projected many yards from the spot.

Now, as coal is not nearly so resisting as sandstone, and as its boring is easily effected, I venture to express the hope that the experiment may be repeated in a colliery. It is easily made, at a cost not worth mentioning, is attended with no serious danger; and should it be successful, it may conduce to the saving of many valuable lives.

Some Observations on the Blood, chiefly in relation to the question, Is Ammonia in its volatile state one of its Normal Constituents? By JOHN DAVY, M.D., F.R.S. Lond. and Edin., &c.

Of the many questions which have been propounded respecting the blood, there are two in particular which of late years have excited some interest and have given rise to much discussion; one, whether it contains any ammonia in a volatile state;—the other, whether the escape of volatile ammonia is the cause of that characteristic quality of healthy blood, its coagulation?

The latter question has been answered, as is well known, in the affirmative by Dr Benjamin Richardson in a work of much ability, a successful prize essay, which was published in 1858. Shortly after, viz., in the following year, I endeavoured to show that this is not the case. The experiments I made were mostly on the blood of the common fowl, which I selected chiefly on account of the rapid manner in which the blood of birds coagulates, and its high temperature during the time the phenomenon is in progress. The results were all negative. The coagulation took place without obvious difference of time whether the blood was allowed to coagulate in a closed vessel, or exposed to the air, as, for instance, when received into a vial, the blood completely filling it, and instantly closed by a glass stopper,—or into a vial of the same kind and the stopper left out. Further, I found that when ammonia in a notable quantity

was added to the blood, its coagulation was not prevented. These results, all of them well marked, seemed to warrant the conclusion that the volatile alkali is no wise concerned in the coagulation of the blood. I have made many experiments since, and they have all been of a confirmatory kind.*

The other question, whether the volatile alkali is a normal constituent of the blood, is not so easily answered, and there is a difference of opinion on the subject among physiologists: thus, Frederichs, who has the reputation of being an accurate observer, thinks that it forms no part of the vital fluid; whilst Dr Hammond takes the opposite view, and believes with Dr Richardson that it is an integrant part of that fluid, and that he has detected it in no less than fourteen experiments, even in the blood of the common fowl, employing Dr Richardson's test—that is, a slip of glass moistened with hydrochloric acid, and exposed to the vapour rising from the blood.†

He does not state the particulars of the trials he made. This, I cannot but hold to be an omission, considering the nature of the fluid, how readily it changes, how apt it is to undergo decomposition,—that of the putrid kind,—and in the act to give rise to the production of ammonia in the form of the volatile carbonate. I believe there is no exaggeration in stating that the instant the blood is taken from the living body a change of this kind commences; hardly perceptible indeed at first, but with advance of time, especially at a temperature above 60 Fahr., rapidly increasing. In illustration, I shall give the details of an experiment which I have made after the reading of Dr Hammond's statement.

When the thermometer in the open air was 62°, a pullet was killed by dividing the great vessels in the neck. The blood, which was very florid, was collected in three small cups, and each was covered with a plate of glass moistened with hydrochloric acid. In each the blood coagulated in less than two minutes. After an exposure of five minutes the glass from one of them was removed, and the acid

* See Trans. Roy. Soc. of Edin. for 1859; and my *Physiological Researches*, London, 1863.

† *Physiological Memoirs*, Philadelphia, 1863. I quote this author, being one of the latest and ablest inquirers I can refer to.

evaporated at a low temperature ; now on examination, whilst still warm, using a $\frac{1}{8}$ -inch power, not a trace of muriate of ammonia was visible. After ten minutes' exposure, another glass was removed ; the result was similar. After fifteen minutes, the third was examined ; now, there were traces of the salt in unmistakable crystals. A fresh plate with acid was now put on the first cup, and left on five minutes ; on evaporation, a distinct formation of the salt was found on the glass.

In further illustration of the rapid manner in which ammonia is formed and evolved in connection with the change which takes place in animal matter after deprivation of life, I shall mention another experiment, made when the temperature of the air was 64° . A portion of muscle, with a little fat (together equal in weight to 90.7 grs.), was taken from the leg of a lamb in less than ten minutes after it had been killed by the butcher ; the flesh was still warm. After fifteen minutes (the time taken in bringing it from the slaughter-house) it was put into a small low cup and covered, without being in contact, with a plate of glass moistened with dilute acid. After an hour, the glass was taken off and the acid evaporated ; a distinct trace of muriate of ammonia was left. The experiment was repeated ; the glass was left on for two hours and twenty minutes : now, the result, as shown by the crystals formed on evaporation, was still more strongly marked. It was again repeated without delay, and the glass was left on from 6.4 P.M., to 12 P.M. The formation of crystals now obtained on evaporation was copious ; they were, as seen with the $\frac{1}{8}$ -inch power, large and characteristic, and yet the meat was not apparently the least tainted ; it had undergone during the time—altogether about nine hours—no change of colour, and had not acquired the slightest unpleasant smell.

Need I point out the bearing of the results of these experiments on those of Dr Hammond ? If he delayed examining the blood for two or three hours, and more especially if his experiments were made during the summer, his finding ammonia in the vapour of fowl's blood is no more than might be expected.

Still, it may be said, it is open to question whether

ammonia, and in its volatile state, does not exist in the blood of other animals, and even in excessively minute quantity, in the blood of birds.

Reasoning from certain facts this seems probable. It is known in the instance of man that the air expired commonly contains a minute quantity of this alkali, leading to the inference that it is exhaled from the lungs, and previously existed in the blood. Dr Richardson, in the work already quoted, gives an account of many experiments seemingly conclusive on this point. I may relate a few which I have made, employing the same method which he used. The subjects of the trials were persons of different ages, including infants, all in health. The expiration directed on the moistened glass was made through one nostril, the other and the mouth closed. Forty deep inspirations in my own case were commonly sufficient to afford distinct, though slight, traces of muriate of ammonia, as seen with the high power after the evaporation of the acid, and whilst the glass was still warm. Rarely, in any instance, have no traces of the salt been obtained. The indications, however, have been variable, sometimes stronger, sometimes feebler, as if depending on states of the system at different times of the day and under different conditions,—a variability which Dr Richardson also observed.*

* In the experiments described above, the precaution was taken of breathing through the nostril to avoid the risk of error which might occur were the respiration through the mouth: in the latter case, the result might be vitiated, and an erroneous inference made, were there a tooth undergoing decay, or a bit of meat adhering, from which, if in a state of incipient putrefaction, ammonia could not fail to be exhaled. When I have compared the breath expired from the lungs through the nostrils and through the mouth, I have found stronger indications in most instances of ammonia from the latter than from the former. I need hardly remark, it is so obvious to reason, that this test of ammonia may have a useful application in medical practice—*e.g.*, in diagnosing the earlier and later stages of pulmonary consumption.

It may be worthy of mention, that the transparent limpid fluid which so often drops from the nose in cold weather (as it were by distillation) has had, as often as I have examined it, an alkaline reaction, and has afforded, after the addition of hydrochloric acid, on evaporation, distinct crystals of muriate of ammonia, mixed with which have been a few of common salt. The healthy saliva, as is well known, has the same reaction, and, similarly treated, affords crystals of the same kind as the preceding—tending to prove that ammonia in each instance is eliminated, and is derived from the blood.

I have obtained also traces of the volatile alkali from the breath of other animals. In the instances of the horse and duck they were unmistakeable. The trials were made in the same manner as that described—the mouth and one nostril closed. The time occupied was about five minutes. In the instance of the common fowl, no trace was detected; but as its nostril is very much smaller than that of the duck, it is not so easy to direct the current of expired air on the spot moistened with the acid. The crystals obtained from the breath of the horse were so large as to be distinctly seen with a glass of a quarter-inch power, yet were hardly appreciable by weight. These trials were made in the open air.

Besides an exhalation of ammonia from the lungs, it is also well ascertained that it is excreted by the skin. It has been found in sweat by Berzelius in the form of the muriate, and has been detected by other inquirers. Like Dr Richardson, I have found it evolved even in insensible perspiration. Here is an instance:—When the thermometer in my room was 70° , a slip of glass moistened with the dilute acid was kept under the palm of the warm hand for ten minutes, carefully avoiding contact; now, on examination, after evaporation of the acid, a trace of muriate of ammonia was obtained in minute crystals, sufficiently distinct, and more than I could have expected. This experiment I have repeated on myself and others with like result. The warmer the weather, and the higher the temperature of the surface, the larger commonly has been the proportion of the salt formed. It is noteworthy, that when the glass, without the addition of the acid, had been exposed to the insensible perspiration during the same length of time, no trace of salt was detected on it; leading to the inference that the ammonia exhaled is in the volatile form, and probably in union with carbonic acid.

Reflecting on these facts, it seemed probable that if ammonia in the volatile state exists in the blood, it is likely to be in a larger proportion in venous than in arterial blood, on the supposition that it is exhaled from the lungs with carbonic acid in the act of expiration. To test this I made trial of the two kinds of blood, one from the jugular

vein, the other from the carotid artery of a lamb about to be slaughtered. The weather at the time was warm, the thermometer in the shade 75° . The quantity of blood collected of each kind was about the same, the recipient vessels of like size, capable of holding about three ounces. The instant they were filled they were covered with a plate of glass partially moistened with the acid. After fifteen minutes an examination was made in the manner before described. On the glass exposed to the arterial blood no muriate of ammonia could be detected with the high power of the microscope; but on that exposed to the venous a trace of the salt was observed.

I shall now mention some other instances in which, using the same method, I have examined the blood in special quest of ammonia. The animals from which the blood was obtained were the common fowl, the duck, horse, sheep, heifer, calf, sea-trout (*Salmo trutta*), and toad. In all the experiments, in those already described and those which follow, due precautions were taken to avoid as much as possible error, and this both in relation to the acid used and the place where the trials were made; in many instances, for greater security, a comparative experiment was made under the same circumstances as to time and locality, with the acid alone and the blood alone. The results of these were negative.*

1. *Of the Common Fowl*.—When the temperature of the open air was 47° , a hen of about three years old was killed by the division of the great cervical vessels. The blood that first flowed was received into a wine-glass. It coagulated almost instantly, certainly in less than half a minute. A plate of glass moistened with acid was immediately placed over it. After about five minutes it was taken off, and the acid evaporated; no trace of ammonia could be detected on it, on inspecting it with the high power. The blood which flowed last, which also rapidly coagulated, and was nearly

* In my earlier experiments, those made in 1859, the test employed for the detection of ammonia was a glass rod, moistened with hydrochloric acid of reduced strength, which, as has been well pointed out by Dr Richardson, is less delicate, less to be relied on than that of the crystalline formation of muriate of ammonia, as seen with the microscope.

equal in quantity, was subjected to the same trial, and for about the same time ; it afforded a tolerably distinct trace of muriate of ammonia. Fresh glasses were applied, and it may be generally remarked, that the indications of the evolution of the volatile alkali increased pretty regularly from hour to hour.

2. *Of the Duck.*—When the open air was 40° , a duck of about three months old was killed in the same manner as the fowl. The blood, as it flowed, was received into two wine-glasses, and into a smaller glass, in each of which it was subjected to the same treatment as the preceding. In the first glass, it coagulated in about two minutes ; in the second, there was a slight retardation ; in the third, the retardation reached about ten minutes. The blood which flowed first was brightest. The plates of glass, moistened with acid, were examined after about ten minutes' exposure. On that from the first, not a trace of the salt could be detected under the high power ; on that from the second and third, a slight trace was visible ; on repetition, and four hours' exposure (this in a room the temperature of which was 50° , the first was in the open air), a trace of salt was obtained from each portion, strong from the second, and in proportion to quantity of blood ; also from the third, but very slight from the first. In this, as in the preceding example, the saline formation obtained by repetition of the trials increased, and the more rapidly as putridity advanced.

3. *Of the Horse.*—When the open air was 65° , three ounces of blood were taken from the jugular vein of a carriage-horse. A prepared plate of glass was instantly placed over it. After half an hour, when the blood had coagulated, and when the buffy coat that had formed was equal nearly in thickness to the crassamentum which had subsided, an examination for ammonia was made ; barely a trace of it was discernible, no crystals could be detected, merely numerous minute granules. On a fresh glass, after thirty minutes, a distinct trace was obtained, and in a crystalline form. The trial was repeated as many as four different times in the twenty-four hours. The saline formation was found to increase in quantity with the lapse of time. In the last, the crystals of muriate of ammonia, as

seen with the high power, were numerous and large, and yet the glass had increased in weight only $\cdot 01$ grain.

4. *Of the Sheep.*—When the temperature of the air was about 70° , two portions of blood were obtained from a sheep of about three years old, one portion just after the great cervical vessels had been divided—this chiefly arterial; another when the flow of blood had become languid—this of a darker hue, and, it may be inferred, chiefly venous. The quantity of each was about three ounces; they were tested in the same manner as the preceding. The first examination was made after an hour. From the arterial blood a very few and minute crystals of muriate of ammonia were obtained; from the venous, more. The difference was well marked. After two hours, and again after sixteen hours, the trial was repeated. Each time there was an increase of the salt, and in a somewhat larger proportion from the venous than from the arterial.

5. *Of the Calf.*—The temperature of the air was about the same as the last mentioned, as was also the quantity of blood, which was the first that flowed. Examined after an hour, a distinct but very minute formation of muriate of ammonia was detected. Examined a second and a third time, after the same intervals as the sheep's blood, the results were very similar. The increase of weight after the last and longest interval did not exceed $0\cdot 01$ grain.

6. *Of the Bullock.*—The air was about 65° . The quantity of blood collected was five ounces; it had the character of venous blood, having been obtained when the flow had nearly ceased. Examined after eight minutes, distinct crystals of muriate of ammonia were seen on the glass more than in any of the preceding trials. Again, examined at intervals during the twenty-four hours, the results were much the same as the preceding, the quantity of saline matter increasing with the length of time. On the last glass its proportion was greater than in any of the foregoing; the crystals were distinguishable by the naked eye.*

* Of these specimens of blood, the sheep's bore distinct marks of putridity, as indicated by smell and discoloration (reddening of the serum) somewhat earlier than the bullock's; the bullock's than the calf's and horse's. On the

7. *Of the Sea-Trout*.—When in Lewes of the Orkney islands, in the beginning of August, I availed myself of the capture of some fish of this kind in the sea with the net to examine the blood. About half an ounce was collected from three fish by cutting the gills the instant they were taken out of the water. After fifteen minutes' exposure, on evaporating the acid, a distinct trace of muriate of ammonia was observed, and rather more on a second trial after four hours' exposure. The coagulum was soft and dark.

8. *Of the Toad*.—This trial was made in July. The toad was of ordinary size, and vigorous, as it commonly is in this month. Though the quantity of blood was small, it afforded a distinct trace of ammonia after an hour's exposure. As the blood was obtained by decapitation, it was a mixture of venous and arterial.*

9. *Of the Fluid of the Allantoid of the Egg of the Common Fowl*.—In this fluid, when the foetal chick had nearly reached its full time, I have detected ammonia. The fluid was of sp. grav. 1016, of alkaline reaction; on evaporation after the addition of hydrochloric acid, it yielded some minute crystals of muriate of ammonia. In this stage of existence the chick may be considered as differing but little from the batrachian, the respiratory function being of no greater activity than suffices apparently for the organic changes essential to development, and nowise sufficient to preserve the temperature essential to the life of the foetus.†

What are the conclusions to be drawn from these results? 1. Do they not all tend to confirm the inference that the conglutination of the blood is not owing to the escape

third day, the serum of the calf's blood was still colourless; that of the horse's only very slightly coloured.

* When stooping over the toad, a nauseous smell was perceived, and an acrid taste in the pharynx, followed by slight headache and malaise, which at the time I fancied might be owing to vapour from the body of the reptile. The cutaneous glandular structure was in an active state, and distended with its peculiar acrid fluid.

† It is very remarkable how rapidly the temperature of the young bird rises when its active respiration is established. The following is an example:—The temperature of a gosling, in process of hatching, just after the end of the egg had been partially broken, was 94°; two hours later it had risen to 104°; after other two hours to 109°; then its head was out of the shell, and the young bird was making muscular efforts to extricate the body.

of the volatile alkali? 2. Are they not favourable to the idea that the blood generally contains a very small proportion of ammonia? 3. Do they not also favour the idea that the proportion of ammonia is larger in venous than in arterial blood? 4. And do they not render it probable that in those animals in which the blood is least thoroughly aerated, such as the Batrachians and other allied genera, the proportion of this alkali is greater than in those animals, such as birds and mammalia, of higher temperature and more complete pulmonary respiration?

Should these conclusions be admitted, it is not difficult to imagine the source of the volatile alkali, inasmuch as, irrespectively of the metamorphic changes which are in constant progress in the body at large, were we to confine the attention to the stomach and intestines, we might in them, in the ingesta, in the one during the formation of chyme, and in the other in the further changes going on in them, find the production of ammonia. Whenever I have examined either the contents of the stomach of any of the mammalia killed in health,* or the contents of the intestines, I have always detected it; and that it should pass into the blood, may be easily credited, as the urine is seldom free from it, and often abounds in it.

On the Relative Effects of Acid and Alkaline Solutions on Muscular Action through the Nerve. By H. F. BAXTER, Esq., Cambridge.

The present inquiry originated during an investigation on the subject of Muscular Contraction; and as some experimental conclusions have been based upon the so-called anelectro-tonic state of the nerve, it became of importance to ascertain whether this state—electro-tonic—depended upon any peculiar electrical condition, or whether the effects which indicate its existence might not be referable to changes, electrolytical, which take place in the nerve; and the facts which I propose to consider are the

* See *Physiological Researches*, by the author, p. 339.

following. If a current of electricity be made to traverse a portion of a nerve connected with the muscles, a difference will be observed in the effects, as manifested by the muscular contractions, when the current is suspended. The muscles of the limb whose nerve has been traversed by an *inverse* current will contract *tetanically*; whilst the muscles, if the current be *direct*, will contract but once—the tetanic contractions being very rarely produced. These effects vary according to the strength of the current and the time of its passage. Matteucci,* in his papers on the “Physiological Action of the Electric Current,” came to the conclusion that the passage of the electric current through a mixed nerve produces a variation in the excitability of the nerve, differing essentially in degree, according to the direction of the current through the nerve. This excitability being weakened and destroyed, and that more or less rapidly, according to the intensity of the current, when it circulates through the nerve from the centre to the periphery (direct current). The excitability, on the contrary, being preserved and increased by the passage of the same current in a contrary direction, that is to say, from the periphery towards the centre (inverse current). But in a subsequent paper † on the “Secondary Electro-Motor Power of Nerves,” he refers the effects to a secondary electro-motor power, and says, “the secondary current, the existence of which is demonstrated by the galvanometer, and which is *direct* for the nerve that has been traversed by the *inverse* current, and which is also demonstrated by the contractions of the galvanoscopic frog [placed upon the nerve], explains, according to the known laws of electro-physiology, the effects produced by it on the opening of the circuit.”

“If any part of a nerve,” says Du Bois Reymond, ‡ “be submitted to the action of a permanent current, the nerve, in its whole extent, suddenly undergoes a material change in its internal constitution, which disappears on breaking the circuit as suddenly as it came on. This change, which is called the Electro-tonic state, is evidenced by a new electro-motive power, which every point of the whole length of the

* Phil. Trans., 1846, 1847.

† Ibid., 1861.

‡ On Animal Electricity. Edited by H. Bence Jones, M.D., p 218.

nerve acquires during the passage of the current, so as to produce, in addition to the nerve-current, a current in the direction of the extrinsic current. As regards this new mode of action, the nerve may be compared to a voltaic pile, and the transverse section loses its essential import. Hence the electric effects of the nerve, when in the electro-tonic state, may also be observed in nerves without previously dividing them."

On a former occasion* I endeavoured to ascertain whether the electric condition of the nerve might not, under these circumstances, be increased; the results of my experiments, however, failed to give any evidence in support of that conclusion; but, on the contrary, the nerve-current was destroyed, and I was led to suppose that the electric current occasioned a disorganisation of the nerve. The later researches of Matteucci clearly point out the cause of my failure, and confirm in a great measure the results I then attained.

In an elaborate article, published in the "British and Foreign Medico-Chirurgical Review" for July 1862, on general Nerve Physiology (German), it would seem that amongst the German physiologists this state of the nerve is still considered as being in a peculiar state (electro-tonic—electrotonus). Pflüger, who has worked at the subject to some extent, speaks of the portion of nerve connected with the cathode as being brought into the state of catelectro-tone, and that the irritability of this portion is increased and rendered favourable for conducting; whilst that portion of the nerve connected with the anode is brought into a state possessing the opposite properties, and is spoken of as being brought into a state of anelectro-tone. Reasoning from the facts established by Matteucci, it appears to me that the two portions of the nerve called anelectro-tone and catelectro-tone, by Pflüger, correspond to the portions where the acid and alkaline compounds are developed by the electric current; if so, the effects are evidently referable to the secondary electro-motor of the nerve as considered by Matteucci. To ascertain how far this supposition

* Edinburgh New Philosophical Journal, New Series, April 1868. Essay on Organic Polarity, chap. ix.

may be correct, it becomes necessary to obtain further evidence than that afforded by the use of the electric current, and for this purpose I employed chemical reagents in the following manner:—

It is reasonable to suppose, that by placing an acid and an alkaline solution on different portions of a nerve connected with the muscles, that some difference might be obtained in the contractions, depending upon the relative position of these solutions in regard to the muscles. In my first experiments the two limbs remained connected with the spinal cord, but the contractions excited in one limb, in consequence of reflex actions, rendered the results doubtful. The following plan was consequently adopted:—The two lower limbs being separated, a long portion of the sciatic nerve was dissected out and placed on two portions of bibulous paper, resting on distinct pieces of glass, and the leg rested on another distinct piece of glass. The bibulous paper, when well soaked, prevented the solutions from running over the surface of the glass: the distances between the place where the solutions were applied and the muscles could be varied at pleasure; but the piece of glass upon which the limb rested was higher than the others, so as to keep the solutions from running down on to the muscles. To prevent the nerve from being moved during the contractions of the muscles, it was necessary to fasten the limb down, either by a ligature or by placing a weight on the thigh bone.

The limb in which the acid solution was nearest the muscle will be designated by the letter *a*; that in which the alkaline solution was nearest to the muscle by the letter *b*. I need scarcely add that the nerves should be placed on distinct papers and glasses, otherwise a circuit would be formed between the two nerves, if resting on the same papers.

The acids comprised the sulphuric, nitric, muriatic, and the acetic acid. Three solutions of each were prepared, varying in strength, and consisting—No 1, of 1 part of acid to 10 of water; No. 2, of equal parts of acid and water; No. 3, of concentrated acid. The alkaline solutions, comprising those of potash, ammonia, and soda, were formed as

follows:—The potash solution, No. 1, consisted of 1 part liq. potassæ to 10 of water; No. 2, of equal parts of liq. potassæ and water; No. 3, of pure liq. potassæ. The ammonia solution—No. 1, of 1 part liq. ammoniæ to 10 of water; No. 2, of equal parts of liq. ammoniæ and water; No. 3, of concentrated liq. ammoniæ. The soda solution—No. 1, of a scruple of bicarbonate of soda to one ounce of water; No. 2, of two drachms of the bicarbonate to one ounce of water. The acids and alkalies were those prepared according to the London Pharmacopœia.

Some care is requisite in applying the solutions. The best plan was to use a glass rod instead of a piece of glass tubing, or a glass pen, which could be easily cleaned; and as it was necessary to apply the solutions simultaneously, two rods were required. Or, instead of dropping the solutions *upon* the nerve, the solutions were placed on the papers and the nerve then dropped upon the solutions.

Having arranged the nerve of *a* and of *b* upon the papers, which were one-eighth of an inch in width, and the distance between the papers a quarter of an inch, whilst that of the muscle from the paper was half an inch, the limbs being firmly secured, the first experiments were for the purpose of ascertaining the effect of placing the solutions, not simultaneously upon the nerve, but one after the other. The nitric acid and potash solutions were employed. Upon placing the alkaline solution, No. 1, upon the nerves, a slight contraction in *a*, but none in *b*; No. 1 acid solution was now placed upon the nerves—no effect. The acid solution, No. 1, was placed upon the nerves—no effect. On placing the alkaline solution on the nerves, a slight contraction in *a*, but none in *b*. The No. 2 solutions were now employed in the same manner: with the alkaline solutions, contractions in both limbs, consisting of two strong contractions in *a* and four in *b*; upon placing the acid solutions on the nerves, three contractions were excited in *a*, but none in *b*. When the acid solutions were first placed upon the nerves, two contractions in *a*, none in *b*; but upon the addition of the alkaline solutions, three strong contractions in *b*, slight fibrillar contractions in *a*. With the solutions No. 3, with the alkaline, three strong contractions in *a*,

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four in *b*; on the addition of the acid, none in *b*, three strong ones in *a*. With the acid solutions, two in *a*, one in *b*; upon the addition of the alkaline, none in *a*, four strong contractions in *b*.

From the foregoing experiments we see—*First*, that the alkaline solutions excite stronger muscular contractions than the acid solutions; and *Secondly*, that the strong solutions, especially the acids, prevent the transmission of nervous impressions excited at the distal extremities of the nerve, which is no doubt due to the disorganisation of the nervous structure.

In the following experiments, the solutions were placed simultaneously upon the nerves, beginning with the weaker solutions.

EXPERIMENT I.—*With Nitric Acid and Potash.*

With No. 1 solutions.—A slight contraction in *a*, none in *b*.

With No. 2 solutions.—Two powerful contractions in *b*; slight contractions and more continued in *a*.

With No. 3 solutions.—Four powerful contractions in *b*, but only one in *a*.

EXPERIMENT II.—*With Nitric Acid and Ammonia.*

With No. 1 solutions.—A slight contraction in *b*, none in *a*.

With No. 2 solutions.—Slight fibrillar contractions in *b*, two powerful ones in *a*, and after a short time, slight fibrillar contractions.

With No. 3 solutions.—Three powerful contractions in *b*, and two in *a*.

I was much surprised at these results with the ammonia solutions, as Kühne* has stated that ammonia is a strong muscular excitant, and that the vapour of ammonia is sufficient to cause muscular contractions. On applying the strong solution to the surface of the muscle, slight contractions occurred, but with the weak solutions, I could not obtain any effect. I believe that a great many of the results which appear as contradictory arise from neglecting to state the season of the year in which the experiments are performed.

* Report on Muscular Contraction. By A. B. Duffin, M.D., in Beale's Archives of Medicine. Nos. 10 and 11. Lond., April 1862.

My present investigation was undertaken during the months of September and October; the weather at the time being cold, and the frogs were getting apparently into a torpid state, and certainly not so excitable as during the summer months. It is well known that during the spring months, at the time of spawning, the irritability of the frogs is so great, that it is difficult to perform any experiments upon them. The mere division of the spinal cord will throw the animal into a state of tetanus; and I have even found that removing them from the water, and handling them, will be quite sufficient to produce the same state.

EXPERIMENT III.—*With Nitric Acid and Soda.*

With No. 1 solutions.—No contractions either in *a* or *b*.

With No. 2 solutions.—Two contractions in *a*, one in *b*.

With No. 3 acid and No. 2 soda.—Two powerful contractions in *a*, and then slight fibrillar contractions; three powerful contractions in *b*.

EXPERIMENT IV.—*With Sulphuric Acid and Potash.*

With No. 1 solutions.—Slight contraction in *a*, none in *b*.

With No. 2 solutions.—One strong contraction in *a*, and then slight fibrillar contractions; two strong contractions in *b*, and slight fibrillar contractions, but they did not last so long as those in *a*.

With No. 3 solutions.—Two strong contractions in *b*, three strong contractions in *a*.

EXPERIMENT V.—*With Sulphuric Acid and Ammonia.*

With No. 1 solutions.—Slight fibrillar contractions in *b*, none in *a*.

With No. 2 solutions.—Two strong contractions in *b*, three strong contractions in *a*, and slight fibrillar contractions.

With No. 3 solutions.—Two strong contractions in *a*, three strong contractions in *b*, and then fibrillar contractions.

EXPERIMENT VI.—*With Sulphuric Acid and Soda.*

With No. 1 solutions.—A slight contraction in *a*, none in *b*.

With No. 2 solutions.—Two strong contractions in *a*, and then slight fibrillar contractions; one strong contraction in *b*.

With No. 3 acid and No. 2 soda.—Two strong contractions in *a*, three strong contractions in *b*.

EXPERIMENT VII.—*With Muriatic Acid and Potash.*

With No. 1 solutions.—A slight contraction in *a* and in *b*.

With No. 2 solutions.—One strong contraction in *a*, two contractions in *b*, and then fibrillar contractions.

With No. 3 solutions.—Two strong contractions in *a*, and then fibrillar contractions; three strong contractions in *b*.

EXPERIMENT VIII.—*With Muriatic Acid and Ammonia.*

With No. 1 solutions.—Slight fibrillar contractions both in *a* and *b*.

With No. 2 solutions.—One strong contraction in *a*, and then fibrillar; three strong contractions in *b*.

With No. 3 solutions.—One strong contraction in *a*, four in *b*.

EXPERIMENT IX.—*With Muriatic Acid and Soda.*

With No. 1 solutions.—No contraction either in *a* or *b*.

With No. 2 solutions.—Slight contraction in *a*, none in *b*.

With No. 3 acid, No. 2 soda.—One strong contraction in *a*, and then fibrillar; two in *b*.

EXPERIMENT X.—*With Acetic Acid and Potash.*

With No. 1 solutions.—No effect.

With No. 2 solutions.—Two strong contractions in *a*, one in *b*.

With No. 3 solutions.—One strong contraction in *a*, and then fibrillar; three strong contractions in *b*.

EXPERIMENT XI.—*With Acetic Acid and Ammonia.*

With No. 1 solutions.—A slight contraction in *a*, none in *b*.

With No. 2 solutions.—Two slight contractions in *b*, three strong contractions in *a*.

With No. 3 solutions.—Two strong contractions in *b*, one in *a*.

EXPERIMENT XII.—*With Acetic Acid and Soda.*

With No. 1 solutions.—No effect.

With No. 2 solutions.—Slight contraction in *a*, none in *b*.

With No. 3 acid, No. 2 soda.—Two slight contractions in *a*, one powerful contraction in *b*.

The first conclusions to be deduced from the results of these experiments, are,—1st, That a difference in the position of the solutions on the nerve produces a difference of effect; 2d, That the contractions which result do not bear any relation, beyond a certain point, to the strength of the solutions,

they do not increase with their increased strength ; and, 3d, That a difference is obtained with different solutions, the alkaline solutions producing a greater or more constant effect than those of an acid nature.

The next question which naturally arises for consideration is that respecting the mode of action of chemical reagents upon the nervous tissue, in causing muscular contractions. Do they act by reacting upon the electricity of the nervous tissue, or by reacting upon the compound forming the tissue, and thus indirectly, upon the molecular forces associated with it ; or in other words, upon nerve-force ?

We have no reason for supposing that chemical reagents would act upon the nervous tissue in a different manner to that observed during ordinary chemical reactions, much less reason have we for believing that, under these circumstances, the vital property of the tissue, nerve-force, would be increased ; we must therefore consider what are the effects that take place during ordinary chemical actions, and I shall limit myself now to the electrical effects. In ordinary chemical actions, it is well known that during chemical combination the compound which performs the part of an acid takes positive electricity, and that of an alkali negative electricity, and the current of electricity which results therefrom goes from the alkali to the acid. When we apply our acid and alkaline solutions upon a nerve, the same effects are produced, which can be readily proved by the galvanometer. Now, the *direction* of the current, as is well-known, has a most important influence in causing contractions, the *direct* being far more influential than the *inverse*. I will first consider the action of a single solution upon a nerve, and believe it to be as follows :—We have first the chemical changes taking place between the chemical reagent and the nerve, during which changes contractions are excited in the muscle, and an electric current developed at the seat of chemical action ; if an acid be employed, the current goes from the nervous tissue to the acid, if an alkali, from the alkali to the nervous tissue. The strength of the current would depend upon that of the solutions. Now, I do not think that when one solution alone is used, that the muscular contractions excited depend upon the electric current

that is developed, but rather upon the changes which take place during the chemical reactions; when the two solutions are placed upon the nerve, then the actions become more complex; in addition to those acting upon the nerve, we have those arising from the combination between the solutions. If the solutions are weak, there may not be sufficient disorganisation of the nerve produced to prevent it from being able to conduct either nervous impressions, or much less a current of electricity, through the part where the solution has been applied; but if concentrated, then disorganisation takes place, and the nerve becomes incapable of conveying its own impressions, and even perhaps the electric current, so far as to excite nervous action; so that the conducting power of a nerve under these circumstances becomes a point of some consideration.

Let us consider for a moment the effects produced by using one solution only, as in my first experiments, and then by adding another. If the weak acid solution be placed near the muscle—no contraction; let the alkaline solution be now placed on the distal side of the nerve—if of sufficient strength, it will cause contractions, in consequence of the acid not destroying the conducting power of the nerve to its own impressions; but when the combination of the acid with the alkali takes place, we may then have contractions produced, in consequence of the current thus developed, which being *direct*, going from the alkali to the acid, is favourable for causing contractions. If the acid be too strong, the transmission of the nervous impression is prevented, and very likely the influence of the current also. Let the alkali be placed near the muscle, contractions occur; now, place the acid upon the distal extremity,—no effect; the current is now in the reverse direction—unfavourable for producing contractions. Let the two solutions be placed on the nerve, simultaneously, as in Experiment 1, the same reasoning will apply, and it is interesting to observe, that when the No. 2 solutions were applied, a difference in the nature of the contractions were observed, being slightly tetanic in the limb α . In other experiments also, when the acid was near the muscle, the fibrillar or slight tetanic contractions, were more frequently produced than in the other limb. It may be said

that the acid being a stronger muscular excitant, the effects were due to the acid getting upon the muscle ; to avoid this a small quantity of oil was smeared over the surface of the muscle previous to applying the acid, so as to prevent its action, but the same effects were obtained.

The results of this inquiry go far to confirm the conclusion of Matteucci, that when an electric current has traversed a nerve and is then suspended, the tetanic contractions produced are due to a secondary electro-motor power established in the nerves. If the current be sufficiently strong, there is an electrolyzation of the nervous structure, and consequently, an acid developed at the anode and an alkali at the cathode ; as these tetanic contractions are principally observed in the limb in which the current has been *inverse*, it corresponds to the acid being developed near the muscle, and the alkali at the distal extremity ; and upon the suspension of the current, the current arising from the combination of the compound formed during the electrolyzation of the nervous tissue is then directed towards the muscle, as shown by Matteucci. That the tetanic contractions should be more prolonged after the passage of the electric current, than when the solutions are applied, as in these experiments, is what one would naturally expect, considering that the changes were, in the one case in the substance of the nervous tissue itself, and in the other, merely between the solutions. We have therefore no evidence for believing that the so-called electro-tonic* state of the nerve (electrotonus) is anything more than the secondary electro-motor power of the nerves induced by the electric current. The contractions of the muscles becoming, in fact, a galvanoscopic test of the chemical and electrical changes which take place in its own nerve.

* It is interesting to observe whence the origin of the term *electro-tonic* arose. Faraday employed it in his first series of papers (*Experimental Researches*, vol. i. p. 16) to indicate the peculiar state in which a wire was supposed to be brought, when subject to volta-electric or magneto-electric induction ; but he subsequently found that the supposed effects could be fully explained without admitting the electro-tonic state.

*On the Antiquity of Man ; a Review of "Lyell" and "Wilson."** By J. W. DAWSON, LL.D., F.R.S., F.G.S., Principal of M'Gill College and University, Montreal. Communicated by the Author.†

Questions of human origins have always been popular, and have been agitated in all sorts of forms. Next to the dread question of the unknown future, the long-buried past is one of the most attractive subjects of inquiry ; and while the faith of the Christian rests for both on the statements of Holy Scripture, the imagination of the poetical or the superstitious, and the reason of the philosopher or the sceptic, have found ample scope for exercise. In our day, geological investigation on the one hand, and antiquarian and philological research on the other, have given an exact and scientific character to such researches, which, without detracting from their interest, has fitted them to attract a more sustained and systematic attention ; hence the appearance of such works as those above named. One of these works is the summing up of the geological evidence in relation to the origin of man, by one of our greatest masters of inductive reasoning. The other is the effort of a skilful antiquarian and ethnologist to apply to the explanation of the primitive conditions of the old world the facts derived from the study of the more recent primitive state of the western hemisphere. Both books are very valuable. Their methods are quite different, and their results as well ; and it may be truly said that the geologist might have profited by the labours of the western antiquarian, had he known of them in time ; and that the antiquarian might have found some new problems to solve, and difficulties to remove, had he read the work of the geologist. For this, among other

* *The Geological Evidences of the Antiquity of Man ; with Remarks on Theories of the Origin of Species by Variation.* By Sir Charles Lyell, F.R.S. 8vo, pp. 520, illustrated. London, John Murray ; Montreal, Dawson Bros.

Pre-historic Man—Researches into the Origin of Civilisation in the Old and New World. By Daniel Wilson, LL.D., Professor of History in University College, Toronto. 2 vols. 8vo, pp. 488-499, illustrated. London, MacMillan & Co. ; Montreal, Dawson Bros.

† From the "Canadian Naturalist," 1868.

reasons, it may be well to consider them together. It will be necessary for us in doing this to summarise the numerous and varied facts adduced, and the reasonings therefrom, and we shall follow the order employed by Sir C. Lyell, bringing in Dr Wilson's antiquarian lore to our aid as we proceed.

The great question to be noticed in this review is that of the connection of human with geological history. How far back in that almost boundless antiquity disclosed by the geologist has man extended? At what precise point of the geological scale was he introduced on the mundane stage; and what his surroundings and condition in his earlier stages? In answer to these questions, negative geological evidence, and some positive considerations, testify, without a dissenting voice, that man is very modern. All the evidences of his existence have, until the last few years, belonged exclusively to the recent or latest period of the geological chronology. Certain late observations would, however, indicate that man may have existed in the latter part of the Post-pliocene period, and may have been contemporary with some animals now extinct. Still the evidences of this, as well as its true significance, are involved in much doubt; partly because many of the facts relied on are open to objection, partly because of the constant accession of new items of information, and partly because the age of the animals whose remains are found with those of man, and the time required by the physical changes involved, are not certain.

To these questions Sir Charles addresses himself, with all his vast knowledge of facts relating to tertiary geology, and his great power of generalisation; and he has, for the first time, enabled those not in the centre of the discussions which have for a few years been carried on upon this subject, to form a definite judgment on the geological evidence of the antiquity of our species.

As a necessary preliminary, Sir Charles inquires as to the *recent* remains of man, including those which are prehistoric in the sense of antedating secular history, but which do not go back to the period of the extinct mammalia. He refers, in the first place, to the detailed researches of the

Danish antiquaries, respecting certain remains in heaps of oyster-shells found on the Danish coast (which appear to be precisely similar to those heaps accumulated by the American Indians on our coasts from Prince Edward Island to Georgia); and respecting similar remains found in peat bogs in that country. These remains show three distinct stages of unrecorded human history in Denmark:—1st, A *stone period*, when the inhabitants were small-sized men, brachykephalous or short-headed, like the modern Lapps, using stone implements, and subsisting by hunting. Then the country, or a considerable part of it, was covered by forests of Scotch fir (*Pinus sylvestris*). 2d, A *bronze period*, in which implements of bronze as well as of stone were used, and the skulls of the people were larger and longer than in the previous period; while the country seems to have been covered with forests of oak (*Quercus Robur*). 3d, An *iron period*, which lasted to the historic times, and in which beech forests replaced those of oak. All of these remains are geologically recent; and except the changes in the forests, and of some indigenous animals in consequence, and probably a slight elevation of some parts of Denmark, no material changes in organic or inorganic nature have occurred.

The Danish antiquaries have attempted to calculate the age of the oldest of these deposits, by considerations based on the growth of peat and the succession of trees, but these calculations are obviously unreliable. The first forest of pines would, when it attained maturity, naturally be destroyed, as usually happens in America, by forest conflagrations. It might perish in this way in a single summer. The second growth which succeeded would in America be birch, poplar, and similar trees, which would form a new and tall forest in half a century; and in two or three centuries would probably be succeeded by a second permanent forest, which, in the present case, seems to have been of oak.* This would be of longer continuance, and would, independently of human agency, only be replaced by beech,

* The details of this process, as it occurs in America, will be found noticed in a paper by the writer in the "Edinburgh Philosophical Journal" for 1847. Such changes are constantly in progress in the American forests.

if, in the course of ages, the latter tree proved itself more suitable to the soil, climate, and other conditions. Both oak and beech are of slow extension, their seeds not being carried by the winds, and only to a limited degree by birds. On the other hand, the changes of forests cannot have been absolute or universal. There must have been oak and beech groves even in the pine woods; and the growing and increasing beech woods would be contemporary with the older and decaying oak forest, as this last would probably perish not by fire, but by decay, and by the competition of the beeches. In like manner, the growth of peat is very variable even in the same locality. It goes on very rapidly when moisture and other conditions are favourable, and especially when it is aided by wind-falls, drift-wood, or beaver-dams, impeding drainage and contributing to the accumulation of vegetable matter. It is retarded and finally terminated by the rise of the surface above the drainage level, by the clearing of the country, or by the establishment of natural or artificial drainage. On the one hand, all the changes observed in Denmark may have taken place within a minimum time of two thousand years. On the other hand, no one can affirm that either of the three successive forests may not have flourished for that length of time. A chronology measured by years, and based on such data, is evidently worthless.

Possibly a more accurate measurement of time might be deduced from the introduction of bronze and iron. If the former was, as many antiquarians suppose, a local discovery, and not introduced from abroad, it can give no measurement of time whatever; since, as the facts so clearly detailed by Dr Wilson show, while a bronze age existed in Peru, it was the copper age in the Mississippi valley, and the stone age elsewhere; these conditions might have co-existed for any length of time, and could give no indication of relative dates. On the other hand, the iron introduced by European commerce spread at once over the continent, and came into use in the most remote tribes, and its introduction into America clearly marks an historical epoch. With regard to bronze in Europe, we must bear in mind that tin was to be procured only in England and Spain, and in

the latter in very small quantity: the mines of Saxony do not seem to have been known till the middle ages. We must further consider that tin ore is a substance not metallic in appearance, and little likely to attract the attention of savages; and that, as we gather from a hint of Pliny, it was probably first observed, in the west at least, as stream tin, in the Spanish gold washings. Lastly, when we place in connection with these considerations, the fact that in the earliest times of which we have certain knowledge, the tin trade of Spain and England was monopolised by the Phœnicians, there seems to be a strong probability that the extension of the trade of this nation to the western Mediterranean, really inaugurated the bronze period. The only valid argument against this, is the fact that moulds and other indications of native bronze casting have been found in Switzerland, Denmark, and elsewhere; but these show nothing more than that the natives could recast bronze articles, just as the American Indians can forge fish-hooks and knives out of nails and iron hoops. Other considerations might be adduced in proof of this view, but the limits of our article will not permit us to refer to them. The important questions still remain: when was this trade commenced, and how rapidly did it extend itself from the sea-coast across Europe? The British tin trade must have been in existence in the time of Herodotus, though his notion of the locality was not more definite than that it was in the extremity of the earth. The Phœnician settlements in the western Mediterranean must have existed as early as the time of Solomon, when "ships of Tarshish" was the general designation of sea-going ships for long voyages. How long previously these colonies existed we do not know; but considering the great scarcity and value of tin in those very ancient times, we may infer that perhaps only the Spanish, and not the British, deposits were known thus early; or that the Phœnicians had only indirect access to the latter. Perhaps we may fix the time when these traders were able to supply the nations of Europe with abundance of bronze in exchange for their products, at, say 1000 to 1200 B.C., as the earliest probable period; and probably from one to two centuries would be a sufficient allowance for the complete

penetration of the trade throughout Europe; but of course wars or migrations might retard or accelerate the process; and there may have been isolated spots in which a partial stone period extended up to those comparatively modern times, when first the Greek trade, and afterwards the entire overthrow of the Carthaginian power by the Romans, terminated for ever the age of bronze, and substituted the age of iron. This would leave, according to our ordinary chronologies, at least ten or fifteen centuries for the post-diluvian stone period; a time quite sufficient, in our view, for all that part of it represented by such remains as those of the Danish coast, and the still more remarkable platform habitations, whose remains have been found in the Swiss lakes, and which belong properly to the *recent* period of geology. In connection with this, we would advise the reader to study the many converging lines of evidence derived from history, from monuments, and from language, which Dr Wilson shows, in his concluding chapter, to point to the comparatively recent origin of at least post-diluvian man. Let it be observed, also, that the attempts of Bunsen and others to deduce an extraordinarily long chronology from Egyptian monuments, and from the diversity of languages, have signally failed; and that the observations made by Mr Horner in the Nile alluvium are admitted to be open to too many doubts to be relied on.*

Before leaving the recent period, it is deserving of note that Sir C. Lyell shows on the best evidence, that in Scotland, since the building of the wall of Antoninus, an elevation of from twenty-five to twenty-seven feet has occurred both on the eastern and western coast, and consequently that the raised sea-bottoms containing canoes, &c., in the valley of the Clyde, supposed by some to be of extremely ancient date, were actually under water in the time of the Romans; a fact of which, but for their occupation of the country, we should have been ignorant.

From the recent period we pass, under the guidance of

* The chronology deduced from the Delta of the Tinière, which would give to the stone period an antiquity of 5000 to 7000 years, appears to us to be similarly defective; and the data assigned to human remains in the valleys of the Mississippi and Ohio, and the old reefs of Florida, still more so.

Sir Charles, to the Post-pliocene, geologically distinguished from the Recent by the fact that its deposits contain the bones of many great extinct quadrupeds ; as for instance the mammoth, *Elephas primigenius*, the woolly rhinoceros, *R. tichorhinus*, and others, heretofore (but it would seem on insufficient evidence) supposed to have disappeared before the advent of man. The evidence now adduced that primeval man was really contemporary with these creatures is manifold and apparently conclusive, and in the work before us is carefully sifted and weighed in all its bearings, much being rejected as inapplicable or uncertain. The evidences relied on are chiefly the following :—

1. Human remains found with those of extinct animals in caves in Belgium, in England, and elsewhere, in circumstances which preclude the probability of their mixture by interments or other modern causes.

2. The finding of flint implements associated with bones of extinct animals in the valley of the Somme, and elsewhere.

3. A supposed sepulchral cave of this period discovered in the south of France. In addition to these there are many minor facts tending to the same conclusion, but with less distinctness.

It is impossible to give extracts which will convey any adequate idea of the facts adduced from the above sources, but the following paragraphs may serve as examples of some of them. They relate to evidence that man was contemporary with extinct animals, afforded by caverns near Liège, explored by Dr Schmerling, and to the similar evidence obtained in the cave of Brixham in England.

“The rock in which the Liège caverns occur belongs generally to the Carboniferous or Mountain Limestone, in some few cases only to the older Devonian formation. Whenever the work of destruction has not gone too far, magnificent sections, sometimes 200 and 300 feet in height, are exposed to view. They confirm Schmerling's doctrine, that most of the materials, organic and inorganic, now filling the caverns, have been washed into them through narrow vertical or oblique fissures, the upper extremities of which are choked up with soil and gravel, and would scarcely ever be discoverable at the surface, especially in so wooded

a country. Among the sections obtained by quarrying, one of the finest which I saw was in the beautiful valley of Fond du Forêt, above Chaudefontaine, not far from the village of Magnée; where one of the rents communicating with the surface has been filled up to the brim with rounded and half-rounded stones, angular pieces of limestone and shale, besides sand and mud, together with bones, chiefly of the cave bear. Connected with this main duct, which is from one to two feet in width, are several minor ones, each from one to three inches wide, also extending to the upper country or table-land, and choked up with similar materials. They are inclined at angles of 30° and 40° , their walls being generally coated with stalactite, pieces of which have here and there been broken off and mingled with the contents of the rents, thus helping to explain why we so often meet with detached pieces of that substance in the mud and breccia of the Belgian caves. It is not easy to conceive that a solid horizontal floor of hard stalagmite should, after its formation, be broken up by running water; but when the walls of steep and tortuous rents, serving as feeders to the principal fissures, and to inferior vaults and galleries, are encrusted with stalagmite, some of the incrustation may readily be torn up when heavy fragments of rock are hurried by a flood through passages inclined at angles of 30° or 40° .

"The decay and decomposition of the fossil bones seem to have been arrested in most of the caves by a constant supply of water charged with carbonate of lime, which dripped from the roofs while the caves were becoming gradually filled up. By similar agency the mud, sand, and pebbles were usually consolidated.

"The following explanation of this phenomenon has been suggested by the eminent chemist Liebig. On the surface of Franconia, where the limestone abounds in caverns, is a fertile soil in which vegetable matter is continually decaying. This mould or humus, being acted on by moisture and air, evolves carbonic acid, which is dissolved by rain. The rain-water, thus impregnated, permeates the porous limestone, dissolves a portion of it; and afterwards, when the excess of carbonic acid evaporates in the caverns, parts with the calcareous matter and forms stalactite. So long as

water flows, even occasionally, through a suite of caverns, no layer of pure stalagmite can be produced ; hence the formation of such a layer is generally an event posterior in date to the cessation of the old system of drainage—an event which might be brought about by an earthquake causing new fissures, or by the river wearing its way down to a lower level, and thenceforth running in a new channel.

“In all the subterranean cavities, more than forty in number, explored by Schmerling, he only observed one cave, namely, that of Chokier, where there were two regular layers of stalagmite, divided by fossiliferous cave-mud. In this instance, we may suppose that the stream, after flowing for a long period at one level, cut its way down to an inferior suite of caverns, and, flowing through them for centuries, choked them up with debris ; after which it rose once more to its original higher level : just as in the Mountain Limestone district of Yorkshire some rivers, habitually absorbed by a ‘swallow hole,’ are occasionally unable to discharge all their water through it ; in which case they rise and rush through a higher subterranean passage, which was at some former period in the regular line of drainage, as is often attested by the fluvatile gravel still contained in it.

“There are now in the basin of the Meuse, not far from Liège, several examples of engulfed brooks and rivers : some of them like that of St. Hadelin, east of Chaudfontaine, which reappears after an underground course of a mile or two ; others, like the Vesdre, which is lost near Goffontaine, and after a time re-emerges ; some, again, like the torrent near Magnée, which, after entering a cave, never again comes to the day. In the season of floods such streams are turbid at their entrance, but clear as a mountain-spring where they issue again ; so that they must be slowly filling up cavities in the interior with mud, sand, pebbles, snail-shells, and the bones of animals which may be carried away during floods.

“The manner in which some of the large thigh and shank bones of the rhinoceros and other pachyderms are rounded, while some of the smaller bones of the same creatures, and of the hyæna, bear, and horse, are reduced to pebbles, shows that they were often transported for some distance in the channels of torrents, before they found a resting-place.

"When we desire to reason or speculate on the probable antiquity of human bones found fossil in such situations as the caverns near Liège, there are two classes of evidence to which we may appeal for our guidance. First, considerations of the time required to allow of many species of carnivorous and herbivorous animals, which flourished in the cave period, becoming first scarce, and then so entirely extinct as we have seen that they had become before the era of the Danish peat and Swiss lake dwellings: secondly, the great number of centuries necessary for the conversion of the physical geography of the Liège district from its ancient to its present configuration; so many old underground channels, through which brooks and rivers flowed in the cave period, being now laid dry and choked up.

"The great alterations which have taken place in the shape of the valley of the Meuse and some of its tributaries, are often demonstrated by the abrupt manner in which the mouths of fossiliferous caverns open in the face of perpendicular precipices, 200 feet or more in height above the present streams. There appears also, in many cases, to be such a correspondence in the openings of caverns on opposite sides of some of the valleys, both large and small, as to incline one to suspect that they originally belonged to a series of tunnels and galleries, which were continuous before the present system of drainage came into play, or before the existing valleys were scooped out. Other signs of subsequent fluctuations are afforded by gravel containing elephants' bones at slight elevations above the Meuse and several of its tributaries. The loess also, in the suburbs and neighbourhood of Liège, occurring at various heights in patches lying at between 20 and 200 feet above the river, cannot be explained without supposing the filling up and re-excavation of the valleys at a period posterior to the washing in of the animal remains into most of the old caverns. It may be objected that, according to the present rate of change, no lapse of ages would suffice to bring about such revolutions in physical geography as we are here contemplating. This may be true. It is more than probable that the rate of change was once far more active than it is now. Some of the nearest volcanoes, namely, those of the Lower Eifel about sixty miles

to the eastward, seem to have been in eruption in Post-pliocene times, and may perhaps have been connected and coeval with repeated risings or sinkings of the land in the basin of the Meuse. It might be said, with equal truth, that according to the present course of events, no series of ages would suffice to reproduce such an assemblage of cones and craters as those of the Eifel (near Andernach for example) ; and yet some of them may be of sufficiently modern date to belong to the era when man was contemporary with the mammoth and rhinoceros in the basin of the Meuse.

“ But although we may be unable to estimate the minimum of time required for the changes in physical geography above alluded to, we cannot fail to perceive that the duration of the period must have been very protracted, and that other ages of comparative inaction may have followed, separating the Post-pliocene from the historical periods, and constituting an interval no less indefinite in its duration.”

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“ As the osseous and other contents of Kent's Hole had, by repeated diggings, been thrown into much confusion, it was thought desirable, in 1858, when the entrance of a new and intact bone-cave was discovered at Brixham, three or four miles west of Torquay, to have a thorough and systematic examination made of it. The Royal Society made two grants towards defraying the expenses,* and a committee of geologists was charged with the investigations, among whom Mr Prestwich and Dr Falconer took an active part, visiting Torquay while the excavations were in progress under the superintendence of Mr Pengelly. The last-mentioned geologist had the kindness to conduct me through the subterranean galleries after they had been cleared out in 1859 ; and I saw, in company with Dr Falconer, the numerous fossils which had been taken from the subterranean fissures and tunnels, all labelled and numbered, with references to a journal kept during the progress of the work, and in which the geological position of every specimen was recorded with scrupulous care.

“ The discovery of the existence of this suite of caverns

* When these grants failed, Miss Burdett Coutts, then residing at Torquay, liberally supplied the funds for completing the work.

near the sea at Brixham was made accidentally, by the roof of one of them falling in. None of the five external openings now exposed to view in steep cliffs or the sloping side of a valley, were visible before the breccia and earthy matter which blocked them up were removed during the late exploration. According to a ground-plan drawn up by Professor Ramsay, it appears that some of the passages which run nearly north and south are fissures connected with the vertical dislocation of the rocks, while another set, running nearly east and west, are tunnels, which have the appearance of having been to a great extent hollowed out by the action of running water. The central or main entrance, leading to what is called the Reindeer gallery, because a perfect antler of that animal was found sticking in the stalagmitic floor, is 95 feet above the level of the sea, being also about 60 above the bottom of the adjoining valley. The united length of the five galleries which were cleared out amounted to several hundred feet. Their width never exceeded 8 feet. They were sometimes filled up to the roof with gravel, bones, and mud; but occasionally there was a considerable space between the roof and floor. The latter, in the case of the fissure-caves, was covered with stalagmite, but in the tunnels it was usually free from any such incrustation. The following was the general succession of the deposits forming the contents of the underground passages and channels:—

“1st, At the top, a layer of stalagmite, varying in thickness from 1 to 15 inches, which sometimes contained bones, such as the reindeer's horn, already mentioned, and an entire humerus of the cave-bear.

“2dly, Next below, loam or bone-earth, of an ochreous-red colour, from 1 foot to 15 feet in thickness.

“3dly, At the bottom of all, gravel with many rounded pebbles in it, probed in some places to the depth of 20 feet without being pierced through, and, as it was barren of fossils, left for the most part unremoved.

“The mammalia obtained from the bone-earth consisted of *Elephas primigenius*, or mammoth; *Rhinoceros tichorhinus*; *Ursus spelæus*; *Hyæna spelæa*; *Felis spelæa*, or the cave-lion; *Cervus tarandus*, or the reindeer; a species

of horse, ox, and several rodents, and others not yet determined.

"No human bones were obtained anywhere during these excavations, but many flint knives, chiefly from the lowest part of the bone-earth; and one of the most perfect lay at the depth of 13 feet from the surface, and was covered with bone-earth of that thickness. From a similar position was taken one of those siliceous nuclei, or cores, from which flint flakes had been struck off on every side. Neglecting the less perfect specimens, some of which were met with even in the lowest gravel, about fifteen knives, recognised by the most experienced antiquaries as artificially formed, were taken from the bone-earth, and usually from near the bottom. Such knives, considered apart from the associated mammalia, afford in themselves no safe criterion of antiquity, as they might belong to any part of the age of stone, similar tools being sometimes met with in tumuli posterior in date to the era of the introduction of bronze. But the anteriority of those at Brixham to the extinct animals is demonstrated not only by the occurrence at one point, in overlying stalagmite, of the bone of a cave-bear, but also by the discovery at the same level in the bone-earth, and in close proximity to a very perfect flint tool, of the entire left hind-leg of a cave-bear. This specimen, which was shown me by Dr Falconer and Mr Pengelly, was exhumed from the earthy deposit in the reindeer gallery, near its junction with the flint-knife gallery, at the distance of about 65 feet from the main entrance. The mass of earth containing it was removed entire, and the matrix cleared away carefully by Dr Falconer, in the presence of Mr Pengelly. Every bone was in its natural place, the femur, tibia, fibula, ankle-bone, or astragalus, all in juxtaposition. Even the patella or detached bone of the knee-pan was searched for, and not in vain. Here, therefore, we have evidence of an entire limb not having been washed in a fossil state out of an older alluvium, and then swept afterwards into a cave, so as to be mingled with flint implements, but having been introduced when clothed with its flesh, or at least when it had the separate bones bound together by their natural ligaments, and in that state buried in mud.

"If they were not all of contemporary date, it is clear from this case, and from the humerus of the *Ursus spelæus*, before cited as found in a floor of stalagmite, that the bear lived after the flint tools were manufactured, or in other words, that man in this district preceded the cave-bear."

Multitudes of questions arise out of these observations, and many of them will probably long remain unanswered; but we may, in the remainder of this article, profitably restrict ourselves to three of them.

1. What style of men were these contemporaries of the mammoth, as compared with those who now walk the earth?

2. How great is their antiquity?

3. What bearing have the conclusions which we must form on these points, on the facts known to us on other evidence than that of geology, as to the origin and early history of man?

The writer of these pages, on a former occasion, ventured to predict that if any osseous remains of antediluvian man should be discovered, they would probably present characters so different from those of modern races that they might be regarded as belonging to a distinct species.* With perhaps one exception, this anticipation has not yet been realized. The skull from the cave of Engis, in Belgium, supposed to be the oldest known, is in the judgment of Professor Huxley, not by any means abnormal, but on the contrary, not unlike some European skulls. Another skull, that of Neanderthal, not found with remains of extinct animals, and therefore of uncertain geological antiquity, has, however, excited more attention than the Engis skull. Its prehistoric antiquity has been assumed by many writers, and its low forehead, prominent superciliary ridges, and general flatness, giving a more ape-like air than that of the heads of any modern tribes, together with the great stoutness and strong muscular impressions of the bones found with it, have been regarded as confirmatory evidence of this supposition. It is quite certain, however, that the characters for which this skeleton is eminent, are found, though perhaps in a less degree, in the rude tribes of

* *Archæia*, p. 237.

America and Australia. It is also doubtful whether this skeleton really indicates a race at all. It may have belonged to one of those wild men, half crazed, half idiotic, cruel and strong, who are always more or less to be found living on the outskirts of barbarous tribes, and who now and then appear in civilized communities, to be consigned perhaps to the penitentiary or to the gallows, when their murderous propensities manifest themselves. Still, as we shall show under our third head, this Neanderthal man is nearer in some respects to our historical idea of antediluvian man than any other of these very ancient examples; though, as Lyell properly suggests, there is no absolutely valid reason for assuming that he may not even have belonged to the same nation with the Engis man; since nearly as great differences are found in the skulls of individual members of some unmixed savage races.

One remarkable conclusion, however, deducible from the answer to this our first question, must not be omitted. Of all the criteria for the distinction of races of men, the skull is probably the most certain, and, as any one may perceive who reads Dr Wilson's book, it affords, in really reliable hands, the best possible evidence of distinctness or of unity, except where great mixtures have occurred. Now man is one of the most variable animals; and yet it would seem that, since the Post-pliocene period, he has changed so little that the skulls of these Post-pliocene men fall within the limits of modern varieties; and this, while so great changes have occurred that multitudes of mammals, once his contemporaries, have utterly perished. Now, if these men are so ancient as many geologists would assume, nay, if they are even 6000 years old, surely the human race is very permanent, and Professor Huxley may well say that "the comparatively large cranial capacity of the Neanderthal skull, overlaid though it may be with pithecoïd bony walls, and the completely human proportions of the accompanying limb-bones, together with the very fair development of the Engis skull, clearly indicate that the first traces of the primordial stock whence man proceeded need no longer be sought, by those who entertain any form of the doctrine of development, in the newest tertiaries, but that they may

be looked for in an epoch more distant from the age of the *Elephas primigenius* than that is from us." They may, in short, spare themselves the trouble of looking for any such transition from apes to men in any period ; for this great lapse of time renders the species practically permanent ; more especially when we bear in mind that of the numerous species whose remains are found with those of these ancient men, some have continued unchanged up to our time, and the rest have become extinct, while not one can be proved to have been transmuted into another species.

Sir Charles devotes no less than five concluding chapters to this doctrine of transmutation, as held by Darwin and others. He does not commit himself to it, but wishes to give it due consideration, as a possible hypothesis, which may at least lead to great truths. We are not disposed to give it quite so high a position. Mr Darwin's book impressed us with the conviction that his hypothesis really explains nothing not otherwise explicable, and requires many assumptions difficult of belief ; while the whole argument in its favour is essentially of the nature of reasoning in a circle. The point to be proved is, that variations arising from external influences and "natural selection" may produce specific diversity. Now, in order to begin our proof of this, we require at least one species, with all its powers and properties, to commence with. This being granted, we proceed to show that it may vary into several races, and that these races, if isolated, may be kept distinct and perpetuated. We further proceed to show that these races differ so much, that if wild, and not tampered with, we might suppose them originally distinct. So far all goes well with our demonstration ; but we find that many of the differences of these races are of the nature of mere monstrosities, like the six fingers of some men, which, as far as they go, would exclude the individuals having them, not only from their species, but from their order or class. Further, we find that the differences which do resemble those of species, have not, when tried by the severe test of crossing, that fixity which appertains to true specific differences ; so that with due care all our races can be proved to belong to but one species. Thus our whole argument falls

to the ground ; unless we are content quietly to assume the thing to be proved, and to say, that after showing that some species are very variable, we have established a certain probability that they may overpass the specific limits ; though the fact that with all this variability, no species has been known practically to overpass these limits, should logically bring us to the opposite conclusion, viz., that the laborious investigations of Mr Darwin have more than ever established the fixity of species, though they have shown reason to believe that many so-called species are mere varieties.

Applying this to man, and even admitting, what Sir Charles Lyell very properly declines to admit, that the differences between men and apes are in all respects only differences of degree, and further admitting with Professor Huxley, that the difference between the size of the brain in the highest and lowest races of men is greater than the differences between the latter and the highest apes, nothing would be proved towards the doctrine of transmutation ; for all these variations might occur without the ape ever overleaping the dividing line between it and the man ; and the one fact to be proved is that this overleap is possible.

Perhaps this question as to man and apes, which some recent transmutationists have started, is one of the most damaging aspects of the doctrine, since it shows better than other cases the essential absurdity of supposing the higher nature to be evolved out of the lower ; and thus startles the common sense of ordinary readers, who might detect little that is unreasonable in the transmutation of an oyster into a cockle, or even of a pigeon into a partridge ; more especially if the reader or auditor is enabled to perceive the resemblance of type between these creatures, without receiving the further culture necessary to appreciate specific and generic difference, and thus is made ready to believe that similarity of type means something more than similar plan of construction. It is very curious too to observe, that while these theorists seize on occasional instances of degraded individuals in man as evidence of *atavism* reverting to a simian ancestry, they are blind to the similar ex-

planation which those who hold an opposite view may give to the cases of superior minds appearing in low races, in which the transmutationists can see nothing but spontaneous elevation. It is also deserving of a passing remark, that while, as Dr Gray shows, the doctrine of transmutation is not subversive of all natural theology—that is, so long as transmutationists admit the presiding agency of a spiritual Supreme Being—the application of such views to the human species attacks leading doctrines of that biblical Christianity which is practically of so much higher importance to man than mere natural theology.

Still some of our modern naturalists follow with as much pertinacity these transmutation hypotheses, as did the old alchemists their attempts to transmute chemical species into each other. Perhaps the comparison is hardly fair to the older school of speculators, for chemical species or elements tend by their combination to form new substances, which animal and vegetable species do not; and by so much the balance of antecedent probability was on the side of the alchemists, as compared with the transmutationists, though their methods and doctrines were very similar. We may, however, at least hope that, like the researches of the alchemists, those of their successors may develop new and important truths. Leaving then this much vexed topic, let us proceed to our second inquiry, as to the actual antiquity of these primitive men.

This antiquity is of course to be measured by the geological scale of time, whose periods are marked not by years or centuries, but by the extinction of successive faunas and floras and the progress of physical changes. With respect to the first of these marks of time, we confess that we have not regarded the observations of Boucher de Perthes and others as free from the suspicion that accidental mixtures of human and fossil bones, or other causes not taken into the account, may have vitiated their conclusions; and this suspicion still applies to some of the cases cited by Sir C. Lyell, as more or less certain proofs. After reading the statements of the present volume, we think the Belgian and Brixham caves may be taken as good evidence of the probable contemporaneousness of man with the *Elephas primi-*

genius, *Rhinoceros tichorhinus*, *Ursus spelæus*, and their contemporaries; or rather as evidence that man was beginning to appear in Western Europe before those animals had finally disappeared. In consequence of some flaws in the evidence, as it appears to a reader at a distance, we cannot as yet so implicitly receive the evidence of the Somme flint weapons. The cave of Aurignac described by M. Lartet, and in which seventeen human skeletons were found buried, apparently in a sitting posture, cannot be relied on, owing to the late period at which it was explored. We are sorry to doubt this unique instance of antediluvian sepulchral rites; but all the appearances *actually seen** by M. Lartet are better explicable on the supposition that a cave, once tenanted by the cave-bear and hyæna, had been partially emptied of its contents by some primitive tribe, who had broken up the bones of the extinct animals, not for their marrow, but to make tools and ornaments of them, and had subsequently used the cave as a place of burial, and the ground in front of it for "feasts for the dead." If the bones are still so perfect as M. Lartet asserts, they must have been quite sound when first disturbed at the early historical time in which the cave may have been ransacked. Further, the skeleton of *Ursus spelæus* found in the interior was below the place of deposit of the human skeletons; and we can suppose it to have been contemporaneous only by the unlikely theory that the earth containing this skeleton was placed in the cave by the aboriginal people.†

We give the above leading cases as examples of the rest which are cited, and all of which may in like manner be divided into those which afford probable, though not absolutely certain evidence of Post-pliocene man, and those which are liable to too grave suspicion to be accepted as evidence. It may be said that we should be more ready to believe, and less critical, but it is not the wont of geologists to be so,

* We refer to M. Lartet's account of his discovery in the "Natural History Review," as well as the more concise statement given in the book before us.

† It is certainly very curious that the objects and arrangements of these caves, and other ancient European depositories, are so thoroughly American, even to the round stone-hammers, whose use is so oddly misinterpreted by the Danish antiquaries.

when new facts and conclusions are promulgated ; and the present case involves too important consequences, both in relation to history, and to the credibility of geological proof in general, to escape the most searching criticism. Geologists must beware lest their science, at the point where it comes into contact with other lines of investigation, and where its own peculiar methods are most liable to err, should be found wanting, and its reliability fall into discredit.

But when did the fossil mammals named above really become extinct ? As a preliminary to our answer to this question, we may state that in Western Europe, in the Post-pliocene period of geologists, these animals were contemporary with many still extant, some of them in Europe, others elsewhere. Pictet even maintains that all, or nearly all of our modern European mammals co-existed with these animals in the Post-pliocene period, and that consequently there has since that time been a progressive diminution of species down to the present day. The mammoth, *Elephas primigenius*, existed, or perhaps began to exist, at a still more ancient period,—the newer Pliocene ; when it was contemporary with *Elephas meridionalis*, and other animals of an older fauna. It continued to survive until the introduction of the modern mammals, and then became extinct along with *Rhinoceros tichorhinus* and several other species, which, however, may have been of younger date than itself. With these species lived the *Megaceros Hibernicus*, or great Irish stag, which lasted longer, but perished before the dawn of history. With them also lived the *Bos primigenius*, or gigantic wild-ox, the aurochs, the musk-ox, and the rein-deer. The first of these existed wild until the time of Cæsar ; the second is still preserved in a forest in Lithuania ; the third exists now in Arctic America ; and the fourth still remains in Lapland. With them also co-existed the wolf, the fox, the hare, the stag, and other creatures still living in Western Europe.

That these creatures have been disappearing at different times seems certain ; some may have been exterminated by man, but the greater part must have perished from other causes. They may have gone *seriatim*, or in considerable numbers at or near the same time ; and there seems some

reason to believe in a considerable and rapid decadence at the end of the Post-pliocene and beginning of the recent period. One cause which may be assigned is change of climate. The climate of Europe in the time of the mammoth was very cold, as indicated by the evidence of glaciers, and other forms of ice action, and by the presence of the musk ox. No doubt the extinction of this creature, and of the mammoth and tichorhine rhinoceros as well, would follow from the amelioration in this respect as the recent period approached. This change of climate depended on geographical changes, modifying the distribution of land and water, and the direction of ocean currents. A subsidence in Central America or in Florida might restore the climate of the mammoth by altering the course of the Gulf-stream; and an elevation of land in these regions may have introduced the climate of the recent period. There is abundant evidence that much subsidence and elevation did occur while these changes in organic life were in progress; and these may, more directly, by the submergence or elevation of large areas in Europe itself, have tended to extinguish species, or introduce them from other regions. All these points being granted, and abundant evidence of them will be found given by Sir C. Lyell, it remains to ask, can we convert the period required for these changes into solar years? There is but one way of doing this in consistency with the principles of modern geology, and this is to ascertain how long a time would be occupied by agencies now in operation in effecting the changes of elevation, subsidence, erosion, and deposit, observed. Reasoning on this principle, it is plain that a vast lapse of time will be required, and that we may place the earliest men and the latest mammoths at an almost incredible distance before the oldest historical monuments of the human race.

But can we assume any given rate for such changes? Not certainly till all the causes which may have influenced them can be ascertained and weighed. We have only recently learned that Scotland has risen 25 feet in 1700 years; but we do not know that this elevation has been uniform and continuous. There is another older sea-level at 44 feet above the present coast; and there is a still higher

sea-level 524 feet above the sea, which certainly goes back to the time of the mammoth. Now we may calculate that if an elevation of 25 feet requires 1700 years, an elevation of 500 feet will require twenty times that length of time ; but if we should find, on further investigation, that 10 of the 25 feet were raised in the first century of the seventeen, and that the rate had gradually decreased, our calculation must be quite different, and even then might be altogether incorrect, since there may have been periods of rest or of subsidence ; so that " such estimates must be considered in the present state of science as tentative and conjectural."

Again, at the rate in which the Somme, the St. Lawrence, and the Mississippi now cut their channels and deposit alluvium, we can calculate that several tens of thousands of years must have elapsed since the mammoth roamed on their banks ; and we have been accustomed to rest on these calculations as close approximations to the truth : but Sir Charles Lyell has, in his present work, introduced a new and disturbing element, in the strong probability which he establishes that the cold of the glacial period extended to a later time than we have hitherto supposed. If, when the gravels of the Somme were deposited, the climate was of a sub-arctic character, we have to add to modern eroding causes the influence of frost, greater volume of water, spring freshets, and ice-jams, and the whole calculation of time must be revised. So if it can be proved that when the St. Lawrence began to cut the ravine of Niagara, in the Post-pliocene or New Pliocene period, there were great glaciers in the basin of Lake Superior, all our calculations of time would be completely set at naught.

Such are the difficulties which beset the attempt to turn the monumental chronology of geology into years of solar time. The monuments are of undeniable authenticity, and their teachings are most valuable, but they are inscribed with no record of human years ; and we think geologists may wisely leave this matter where the Duke of Argyll, in his address to the Royal Society, lately placed it, as a doubtful point, in so far as geological evidence is concerned, whether the mammoth lived later than we have hitherto supposed, or man lived earlier. Still, as we have already

stated, those geologists who hold that we must reason inflexibly on rates of change indicated by modern causes, will necessarily, on the evidence as it now stands, maintain that the human race, though recent geologically, is of very great antiquity, historically.

We must now shortly consider our third question, as to the bearing of these facts and doctrines on our received views of human chronology, derived from the Holy Scriptures and the concurrent testimony of ancient monumental and traditional history. It is certain that many good and well-meaning people will, in this respect, view these late revelations of geology with alarm; while those self-complacent neophytes in biblical learning who array themselves in the cast-off garments of defeated sceptics, and, when treated with the contempt which they deserve, bemoan themselves as the persecuted representatives of free thought, will rejoice over the powerful allies they have acquired. Both parties may however find themselves mistaken. The truth will in the end vindicate itself; and it will be found that the results of such careful scrutiny of nature as that to which naturalists now devote themselves, are not destined to rob our race either of its high and noble descent, or its glorious prospects. In the mean time, those who are the true friends of revealed truth will rejoice to give free scope to legitimate scientific investigation, trusting that every new difficulty will disappear with increasing light.

The biblical chronology, though it allows an unlimited time for the prehuman periods of the earth's history, fixes the human period within narrow limits, though it does this not by absolute statement of figures, but rather by inference from chronological lists, with respect to the computation of which there may be and has been some difference, especially in the antediluvian period. Allowing large latitude for these differences, we have, say 2000 to 3000 years for the human antediluvian period, corresponding, it is to be supposed, to the later Post-pliocene of geologists. In this period men may have extended themselves over most of the old continent; and it has been calculated that they may have been nearly as numerous as at present, but this is probably an exaggeration. They had, locally at least,

domesticated animals ; they had discovered the use of the metals, and invented many useful arts, though there must have been a vast, scattered, barbarous population. They had split into two distinct races, some portions of which at least had sunk to a state presumably lower than that of any modern tribe, since these latter are all amenable to the influences of civilisation and Christianity, while the former seem to have been hopelessly depraved and degenerate. At the same time they had much energy for aggression and violence ; and it would seem that these giants of the olden time were in process of extinguishing all of the civilisation of the period when they were overwhelmed with the deluge. This is described in terms which may indicate a great subsidence, of which the Noachian deluge was the culminating point, in so far as Western Asia was concerned. The subsidence, unless wholly miraculous, may have commenced at least at the beginning of the 120 years of Noah's public life, and possibly much earlier, and the re-elevation may have occupied many centuries, and may not have left the distribution of land and water, and consequently climate, in the same state as before. At a very early period of this subsidence, if there were men in Europe, they would be perfectly isolated from the original seats of population in Asia, and so would the land animals, their contemporaries. There is, further, nothing in the Mosaic account to prevent us from supposing that the existence of many species was terminated by this great catastrophe.* These are some of the conditions of the biblical deluge, which we might much further illustrate, were this a proper place for doing so, but those stated will suffice to show precisely in what points the new doctrines of geologists in regard to the antiquity of man appear to conflict with this old narrative.

When we carefully consider the geological facts, in so far as they have been ascertained, it seems to us that the discrepancy may be stated thus. Reasoning on the geological doctrine that all things are to be explained by modern causes, and insisting on a rigid application of that doctrine, we must infer that the date of the introduction of

* See "Archaia," pp. 216 *et seq.*, and pp. 288 *et seq.* ; King's "Geology and Religion," "Deluge."

man was "many ten thousands of years" ago. Adopting the biblical theory, so to speak, that a great subsidence, of which modern history affords no example, has occurred within the human period, we might adopt a very much shorter chronology. It would seem at present that the facts can be explained on either view, and that the possibility of reconciling these views must depend on the greater or less evidence which geologists may find of more rapid changes than they have heretofore supposed within the human period. Our own impression, derived from a careful study of all the facts so well stated by Sir C. Lyell, is, that the tendency will be in this direction, that the apparent antiquity of the comparatively insignificant deposits containing remains of man and his works will be reduced, and that a more complete harmony than heretofore between the earliest literary monuments of the human race and geological chronology will result. At present the whole inquiry is making rapid progress, and the time may perhaps be not far distant when its difficulties will receive some such solution. In the meantime, both of the writers whose works are noticed in this article deserve careful study, and will be found to contribute much toward the solution of these great questions.

The Observed Motions of the Companion of Sirius considered with Reference to the Disturbing Body indicated by Theory. By T. H. SAFFORD, Assistant at the Observatory of Harvard College. Communicated by the Author.*

It is well known to astronomers that the motions of the bright star Sirius indicated the presence of a disturbing body, before the discovery of a companion by Mr Alvan Clark. It was shown by Bessel,† that there were irregularities in the motion of this star in right ascension which were only to be explained by the presence of an unseen

* From the Proceedings of the American Academy of Arts and Sciences, vol. vi.

† *Astronomische Nachrichten*, Nos. 514-516.

companion, unless, indeed, we might permit ourselves to doubt the universality of the law of gravitation. C. A. F. Peters,* some years later, computed such of the elements of the motion of Sirius around the centre of gravity of the system as could be deduced from the motions in right ascension; and Schubert† pointed out that there was some reason to believe that the motion in declination also was irregular, though he seems to have fallen into the error of supposing that the motions in right ascension and declination were not completed in the same period.

Afterwards M. Laugier,‡ of the French Institute, represented the observations of Sirius in declination from 1690 to 1852 by a formula of interpolation which I fear we must consider erroneous. Laugier gives a certain weight to Flamsteed's position from the "*Historia Cœlestis Britannica*," which is known to have been reduced (and probably from a single observation), without regard to aberration or nutation; so that it cannot be depended upon within 15", while the real irregularities of Sirius's motion in declination are less than 2".

Calandrelli,§ Director of the Pontifical Observatory|| at Rome, has in several places insisted that the Greenwich Twelve-Year Catalogue was in error by about 3" for the date 1845. This, however, was shown by Main¶ to be contradicted by the several years' work, and I presume most astronomers would agree in considering Calandrelli's argument as irrelevant.

In No. 28 of Professor Brunnnow's valuable "*Astronomical Notices*," I have shown that, in spite of the misapprehensions to which I have just alluded, the observed motion of Sirius in declination is in fact represented by a formula depending on the previous investigation of Peters, but with four new unknown quantities inserted. The ad-

* *Astronomische Nachrichten*, Nos. 746-747.

† *Astronomical Journal*, vol. i. p. 154.

‡ *Astronomische Nachrichten*, No. 1142.

§ *Atti dell' Accademia Pontificia de' Nuovi Lincei*, 5 Aprile, 1853, p. 316, and elsewhere.

|| This is not to be confounded with the observatory of the Collegio Romano.

¶ *Monthly Notices of the Royal Astronomical Society* vol. xx. p. 202.

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dition of these four quantities, which I have determined by least squares, enables us to state with a certain degree of accuracy the angle of position of the centre of gravity with respect to the visible mass, and thus the angle of position of the supposed invisible companion.

Closely following the actual publication * of this memoir, came the discovery † of the companion by Mr Clark. The question at once arose, whether this were the disturbing body; the evidence bearing upon this appeared very noteworthy. In the first place, the angle of position agreed (within the uncertainty of observation) with that computed for the disturbing body, assuming my investigation ‡ as the basis. The following table shows the relation for 1862 between computation and observation. To my own computation I have added the similar one of Auwers, published afterwards.§

Computed by Auwers,	1862·1	97°·3	{ (yearly diminution 1°·4)
„ Safford,	1862·1	83·8	
Observed by Bond,	1862·2	84·6	
„ Chacornac,¶	1862·2	84·6	
„ Lassell,**	1862·3	83·8	
„ Rutherford,††	1862·2	85·0	

The difference between Dr Auwers's theoretical investigation and my own is perhaps not larger than the uncertainty of all the series of observations on Sirius would explain; as I have before stated, the amount of deviation from which the angle of position was computed is very small.

But that the companion of Sirius may produce the disturbances, it—the faint object barely visible in the largest class of telescopes—must have a mass nearly *two-thirds* that

* The number bears date Dec. 20, 1861; my own communication, Sept. 20th.

† Jan. 31, 1862. First announced by Professor Bond, in No. 1353 of the *Astronomische Nachrichten*.

‡ This fact was stated by Professor Bond (*American Journal of Science* for March 1862, p. 287).

§ *Astronomische Nachrichten*, No. 1371. It is proper for me here to express my sense of the courtesy with which Dr Auwers admitted my priority in the matter.

|| *Astr. Nachr.*, No. 1374.

¶ *Ibid.* No. 1355.

** *Ibid.* No. 1360.

†† *American Journal of Science*, May 1863, p. 407.

of Sirius itself. It is difficult to believe this; but, as the evidence of this year (1863) shows, we may be compelled to do so.

There are three hypotheses logically possible with respect to the new star. It may be either unconnected with the system of Sirius; or, secondly, a satellite, but not the disturbing body; or, thirdly, the disturbing body itself. On the first hypothesis, the proper motion of Sirius itself would put it in the following position, assuming the angle of position $84^{\circ}5$, for 1862.2, and distance $10''\cdot19$ for the same date, the latter being the mean of these results (excluding Lassell's $4''\cdot92$, which is quite wrong).

10 ^h 09	Rutherford.*
10 ^h 07	Bond.†
10 ^h 41	Chacornac,‡

Position and Distance by Hypothesis I.; assuming the little star to be fixed.

1863.0	79 ^h 1	10 ^h 80
1864.0	73.3	11.69

The second hypothesis gives no ground for calculation, and it will be considered further on.

The third hypothesis would give (correcting my own investigation, so as to agree in 1862.2 with observation, by $+0^{\circ}9$).

1863.0	83 ^h 5
1864.0	82.1

Observation gives, compared with these hypotheses,

1863.3	Bond,§	82 ^h 8	Hyp. I.	77 ^h 4	Hyp. III.	83 ^h 1
1863.2	Rutherford,	81.2		77.9		83.2

Computed — Observed.

	I.	III.
Bond,	-5.4	+0.3
Rutherford,	-3.3	+2.0

* American Journal of Science for May 1863, p. 407.

† Astronomische Nachrichten, No. 1874.

‡ Ibid. No. 1855.

§ MS. furnished by Professor Bond.

|| As before, American Journal of Science for May 1863, p. 407.

To which must be added, that the first hypothesis requires an increase of distance between 1862.2 and 1863.2 of $0''.8$; the third, a very slight diminution; but observation indicates a diminution of about $0''.55$, a quantity, to use Mr Rutherford's expression,* so small that its existence cannot be asserted with confidence." It is hardly conceivable that the long and careful series of observations of Mr Rutherford should be in error $3''.3$; and also inconceivable that Professor Bond's measures, agreeing as they do within $2^\circ 20'$ among themselves, should be in the mean $5^\circ.4$ erroneous.

We have therefore nothing to oppose to the hypothesis that the new companion is the disturbing body, but the very improbable supposition that the small star partakes very nearly in the great proper motion of Sirius without physical connection; or the second hypothesis, that the new star is in the system, but with small mass. If this is the case, the disturbing body must, in lieu of the small light of the companion, have still less, or even be absolutely invisible. *It is consequently highly probable that the disturbing body has been actually found; that what was predicted by theory has been confirmed by sight.* The importance of continued observations on Sirius cannot be too highly felt. The companion must be measured the coming year, and for several years; while Sirius itself should be re-observed with meridian instruments. So far as the right ascension element is concerned, a series of observations is now in progress at Cambridge; while Captain Gilliss has most obligingly consented to make a series of declination-observations at Washington; and the standard observatories at Greenwich and Paris will doubtless continue their series of fundamental star observations, including, of course, Sirius.

I am much obliged to Mr Rutherford for the communication of the details of his observations in 1863, and hope he will publish them, together with similar details of those of 1862, and others to be made hereafter. The subject is one where the co-operation of several observers is desirable. Full certainty here can only be obtained after several years' observations.

* As before, American Journal of Science for May 1863, p. 407.

Notes on the Fertilisation of Orchids. By WILLIAM RUTHERFORD, M.D., President of the Royal Medical Society, Resident Physician Royal Infirmary. (Being a portion of a thesis, for which a gold medal was awarded by the Medical Faculty of the University of Edinburgh at the Graduation in 1863.)*

Mr Darwin, in the introduction to his admirable work on "The Fertilisation of Orchids," states, that his chief reason for writing the work was, "to show that the contrivances by which orchids are fertilised, have for their main object the fertilisation of each flower by the pollen of another flower;" and to show that, in his "Origin of Species," he had good grounds for expressing his belief in what he regards as an apparently universal law—viz., "That no hermaphrodite fertilises itself for a perpetuity of generations, an occasional cross with another individual being required." He, moreover, expresses the hope, that his researches may stimulate others to inquire into the habits of our native species.

During the past summer (1862), I spent some time in the examination of a considerable number of orchids, with a view to ascertain whether or not Mr Darwin's observations were accurate, and the conclusions at which he had arrived correct. The points which I especially wished to test, were, 1st, Is insect agency essential for their fertilisation? 2d, Is a flower fertilised by its own pollinia, or by those of other flowers? As regards the first of these, Mr Darwin says, that in every orchis, with the exception of the bee orchis and *Cephalanthera grandiflora*, insects are required to remove the pollinia, and apply them to the stigma; and with regard to the second point, he says,—that although in some cases the pollinia may be applied to the stigma of the flower from which they are taken, yet in all they may be—and most generally they are—applied to the stigmas of other flowers; farther, in some flowers—the marsh *Epipactis*, for

* Read before the Botanical Society November 12, 1863.

example—the pollinia are removed only when the insect retires from the flower.

Sprengel, in 1795, and Robert Brown, in 1833, though the latter was not without his doubts on the subject, both expressed their belief in the necessity for insect agency; and many others have concurred with the opinion; but Darwin was the first to show that the necessity for insects, which was previously considered to be confined to a few, is almost universal. My observations, so far as they have extended, have most thoroughly convinced me of the truth of Mr Darwin's statement. But I must here mention, to prevent any misunderstanding, that I have examined four species only,—for the district in which I resided contained only these four species, although they were severally represented by large numbers of individuals, so that I was able to make a pretty thorough examination of each species. I was staying in a part of Kent where *Orchis maculata* and *Cephalanthera grandiflora* were especially abundant; and *Gymnadenia conopsea*, and *Orchis pyramidalis*, to a lesser degree. I examined 1175 flowers of *Cephalanthera*, 1000 of *Orchis maculata*, 244 of *Gymnadenia conopsea*, and 60 of *Orchis pyramidalis*, in all 2479 flowers. This number may seem very large; but it must be remembered, that the flowers grew abundantly in the locality; and I had but little difficulty in procuring them. All the plants grew near, or in, woods, so that they were most favourably situated for visitation by insects. Mr Darwin says, that on one occasion only has he seen an insect capable of carrying away the pollinia visit an Orchis. I have been more fortunate; for I have repeatedly observed, especially on warm, cloudy days, lepidopterous insects paying their visits; and on one occasion I actually saw an insect remove the pollinia. Although Mr Darwin thinks that an insect does not confine its visitations to one particular species, but embraces several,—an opinion which he has shown to be true in the case of some one or two insects,—I must say that *Orchis maculata* and *Cephalanthera grandiflora*, although growing together, were visited by totally distinct insects, and either species was only visited by one kind of insect.

This fact is certain regarding the fertilisation of three

out of these four species,—*self-fertilisation is impossible*,—the pollinia must be removed from the flower and applied to the stigma of either the same or another flower. In by far the greater majority of the flowers, the pollinia, where these were single, were both removed, and in only a few of these were the ovaries non-fertilised. Sometimes I found the heads of pollinia sticking to the stigmas: this was rare, however; more frequently I found bundles broken off from the pollinia adhering to the stigma, and in some of these instances the pollinia remained in the same flower untouched, showing conclusively, that these flowers had been fertilised by the pollinia of other flowers. The flowers I examined were generally *old*, with the viscid discs and stigmas quite dry, so that no farther change could take place in the fertilisation of such flowers. Out of 1304 flowers, 953 had both pollinia removed, of which 895 were fertile and 58 were non-fertile. From this it appears, that although the pollinia may have been removed from the flowers, these were sometimes non-fertile. This is, because the insect has carried away the pollinia without pushing them against the stigma, and because the flowers have never been visited by insects having pollinia on their probosces. If such flowers could ever have become fertilised (most were old), it must have been by the pollinia of other flowers.

In 212, both pollinia were still remaining, although the flowers were mostly dry and shrivelled. Of these 119 were fertile, and 96 were non-fertile, so that although these flowers are incapable of self-fertilisation, the flowers are oftener fertilised than not. Insects with pollinia attached to their probosces visited the flowers and fertilised them, although they did not remove the pollinia. Had the flowers grown in a less wooded district, where insects are more scarce, many more of them would have had both pollinia remaining, and fewer of these would have been fertilised. Observe (*see the Table at the end*) how different is the case of *Cephalanthera grandiflora*, which is capable of self-fertilisation, although to a small degree: only 39 out of 1175 flowers had both pollinia remaining, and these, nevertheless, were *all* fertile; while of the 1128 which had both pollinia removed, only 8 were non-fertile. In the two other species which had the pollinia

separate, that is, unattached at the base to one another, the right pollinium was removed rather oftener than the left, a fact which would be difficult to explain. Of the 166 flowers which had only one pollinium removed, 142 were fertile and 24 non-fertile, showing that where only one pollinium is removed, the flower is not so certainly fertilised; in short, the insects have not visited them so frequently.

It is unnecessary for me to comment further upon the following Table, but I may shortly state, that it fully bears out Mr Darwin's conclusions; it establishes nothing new, but simply places beyond doubt very important opinions advanced by Darwin, among which the following are the most important:—1st, Insect agency is necessary for fertilisation; 2d, Crossing of the individuals of a species is not only permitted, but all the arrangements seem especially adapted to bring about such a result.

One would suppose that hybrids ought to be very common if Mr Darwin's opinion were correct,—that one insect visits several species of orchids,—while it is well known that orchidaceous hybrids are extremely rare. From all that I have observed, I believe it to be the rule that each species has its special visitor, and that the same insect visits several species, to be the exception. I dare not, however, speak too positively on this point, for my observations have not been extensive.

Finally, it may seem superfluous for me to draw attention to the beautiful and laborious investigations contained in Mr Darwin's work on orchids; but only those who have carried on such researches are able to estimate the severe and prolonged labour which they entail.

NAME.	Number of Flowers examined.	Both Pollinia remaining.		Both Pollinia removed.		One Pollinium removed.			
						The Right.		The Left.	
		Fertile.	Non-fertile.	Fertile.	Non-fertile.	Fertile.	Non-fertile.	Fertile.	Non-fertile.
<i>Orchis maculata</i> }	1000	50 2 of these young.	28 12 of these young.	765	47 15 of these young.	53	3	44	10
<i>Gymnadenia conopsea</i> }	244	54	29	96	9	30	7	15	4
<i>Orchis pyramidalis</i> }	60	15	39	34	2	Pollinia attached to one another by their bases.			
TOTALS, {	1304	119	96 215	895	58 953	83	10	59	14 73
<i>Cephalanthera grandiflora</i> }	1175	39	None.	1128	8	Pollinia attached to one another by their bases.			

Remains of Birds' Eggs found at Fisherton, near Salisbury.

By H. P. BLACKMORE, M.D., Salisbury. Communicated by Sir WILLIAM JARDINE, Bart.

In "The Geologist" for October last, Mr Blackmore of Salisbury, while giving a list of the fossil mammalia and flint instruments obtained in the Pleistocene districts of Fisherton, near Salisbury, states—"Although you ask no information with regard to birds, it may be interesting to some of your readers to know that fragments of such fragile things as birds' eggs have been obtained from the same deposits; one in point of size and thickness of shell would correspond, if entire, to that of a goose, the other to that of a moor hen." This being the first record, we believe, of the remains of eggs having been found with those of the lower animals and others of like time, attracted our attention, and in reply to our inquiry Mr Blackmore has kindly sent the following particulars to Mrs Strickland:—

"Both fragments of egg-shells are in my possession; the larger of the two, which I think is probably part of the egg of *Grey-Lag Wild Goose*, was found in March 1861, by workmen digging brick earth in Mr Baker's pit. The clay, sand, and gravel in this pit are slightly stratified, and at one point are nearly 30 feet in thickness. The shell was found about 14 feet below the surface, the soil above having evidently never been disturbed. Within a few feet of the spot where the shell was discovered the men found two small bones, one the coracoid, and the other about the upper three-fourths of the femur of a species of *Anser*, corresponding in size with similar bones of the Grey-lag. The shell itself is stained of a pale fawn colour, and both upon the in and outside has many small superficially raised incrustations; hence I infer that the shell must have been already broken when embedded in the clay.

"Since writing to 'The Geologist,' I have joined the fragments of the smaller egg more perfectly together, and find, from the small size of the fragments, I formed an erroneous opinion of the size of the egg. The restored fragments I have carefully compared with a collection of recent British

eggs, and find them both in texture and size to correspond closely with the eggs of the *Common Wild Duck*. In colour the shell is rather darker than the larger one, but is in parts similarly incrustated both inside and out. This was found in Mr Harding's pit in November 1862, about 20 feet below the surface, in undisturbed clay. No bones accompanied this specimen."

I. On Parallel Relations of the Classes of Vertebrates, and on some Characteristics of the Reptilian Birds.

II. The Classification of Animals based on the Principle of Cephalization. No. I. By JAMES D. DANA. Communicated by the Author.*

I. On certain parallel relations between the classes of Vertebrates, and on the bearing of these relations on the question of the distinctive features of the Reptilian Birds.

At the close of an article by Professor Hitchcock, a portion of a letter of the writer is quoted, in which a parallelism is drawn between the Oötocoid or semi-oviparous Mammals (*Marsupials* and *Monotremes*), the Ichthyoid Reptiles (*Amphibians* of De Blainville, *Batrachians* of many authors), and the Reptilian Birds. The general fact of this parallelism throws light on (1.) the classification of Mammals, (2.) the distinctive features of the Reptilian birds, and (3.) the geological progress of life.

1. *Classification.*—The Amphibians are made by many zoologists an independent class of Vertebrates, on the ground of the fish-like characteristics of their young. The same systematists, however, leave the Marsupials in the class of Mammals, notwithstanding their divergencies from that type. The number of classes of Vertebrates, usually regarded as four, thus becomes *five*, namely, Mammals, Birds, Reptiles, Amphibians and Fishes. There are some indications that this number will soon be further increased by some zoologists, through the making of another class out of the *Reptilian Birds*.†

The discovery of the Reptilian Birds has brought the general law to view, that, among the four classes of Vertebrates, ordinarily

* From the American Journal of Science and Arts, vol. xxxvi., Nov. 1868.

† Professor Agassiz, in vol. i. of his "Contributions to the Natural History of the United States," page 187, subdivides Fishes into four *classes*, namely, 1. Myzonts; 2. Fishes proper, or Teliosts (Ctenoids and Cycloids); 3. Ganoids; 4. Selachians; which would make the total number of classes of Vertebrates *nine*.

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received, each, excepting the lowest, consists of, *first* a grand *typical* division, embracing the majority of its species, and *secondly*, an inferior or *hemitypic* division, intermediate between the typical and the class or classes below.

Before proceeding with our illustrations of this point, a word may be added in behalf of these four classes. In order to appreciate their true value, it is necessary to have in view the *type-idea* which is the basis of the fundamental characteristics of each, and which is connected with the existence of *three* distinct habitats for life—the water, the air, and the land: that in Fishes, this idea is that of *swimming aquatic* life; in Reptiles, that of *creeping terrestrial* life; in Birds, that of *flying aerial* life; in Mammals, that of *terrestrial* life, again, but in connection with a higher grade of structure, the Mammalian. The type-idea is expressed in the adults both of the typical and hemitypic groups; and any attempt to elevate the hemitypic into a separate class tends to obscure these ideal relations of the groups in the natural system of Vertebrates.

The following are the illustrations of the law above mentioned.

(1.) In the classification of Vertebrates, Mammals, the first class, are followed by birds, as the second; and while the former are viviparous, the latter are, without exception, *oviparous*. The species of the inferior or hemitypic group of Mammals, partake, therefore, in some degree, of an *oviparous* nature, as the term *semi-oviparous* or *Oötocoid* implies.

In fact, all Vertebrates, excepting Mammals, are typically *oviparous*, although some cases of viviparous birth occur among both Reptiles and Fishes. In the viviparous Mammals, the embryo, during its development, derives nutriment directly from the body of the parent until birth, and also for a time after birth; while in the viviparous Fish, the Selachians excepted, there is simply a development of the egg internally, in the same manner essentially as when it takes place externally. Applying, then, the term *oviparous* to all cases in which the embryo is shut off from any kind of placental nutrition, Reptiles and Fishes, with the exception mentioned, are as essentially *oviparous* as Birds. Hence, the *Oötocoids* or non-typical Mammals are actually intermediate in this respect, and in others also, between the typical Mammals, on one side, and the inferior *oviparous* Vertebrates collectively, on the other.

(2.) Again, the class next below Birds is that of Reptiles. And, correspondingly, the inferior or hemitypic group of Birds is *Reptilian* in some points of structure.

(3.) Again, the class next below Reptiles is that of Fishes; and therefore the inferior or hemitypic group of Reptiles is the intermediate or *Ichthyoid* one of Amphibians—the young of frogs

and salamanders and other included species having gills like fishes, besides some additional fish-like peculiarities.

The parallelism between the three classes, Mammals, Birds, and Reptiles, is thus complete.

(4.) Fishes have no class of Vertebrates below them, so that an *inferior* hemitypic division is not to be looked for. It might be suspected that the intermediate group in this case would be one between Fishes and the lower sub-kingdoms either of Mollusks or of Articulates; but none such exists. The lowest fish, an Amphioxus, is as distinctly a Vertebrate as the highest, and no Mollusk or Articulate exhibits any transition towards a vertebrate structure.

There are, however, *hemitypic* Fishes; but their place is towards the top of the class instead of at its bottom. Ganoids constitute one group of this kind, between Fishes and Reptiles, as long since pointed out by Agassiz. Again, Selachians (or Sharks and Rays) constitute another, between Fishes and the higher classes of Vertebrates. This last idea also has, we believe, been suggested by Agassiz (although we cannot refer to the place where published), this author regarding the species as intermediate in character between Fishes and the allantoidian Vertebrates. Moreover, Müller long ago observed the relation of the Sharks to the Mammals in having a vitelline placenta, by which the embryo draws nutriment from the parent, as does the mammalian foetus by means of its allantoidian placenta.

Ganoids and Selachians are thus two *hemitypic* groups in the class of Fishes.

The scheme of grand divisions is then as follows:—*

- | | |
|--|--|
| I. | |
| A. Typical Mammals. | |
| B. Hemitypic Mammals,
or OÖTOCOIDS. | |
| II. | III. |
| A. Typical Birds. | A. Typical or true Reptiles. |
| B. Hemitypic Birds,
or ERPETOIDS. | B. Hemitypic Reptiles,
or AMPHIBIANS. |
| IV. | |
| A. Hemitypic Fishes,
or SELACHIANS. | B. Hemitypic Fishes.
or GANOIDS. |
| C. Typical Fishes,
or Teliosts. | |

One of the groups of hemitypic Fishes looks directly towards

* It is here seen that the term *Oölocoid*, applied to Marsupials and Monotremes has great significance; and so likewise, *Erpetoids* and *Amphibians*. *Oölocoid* is simply the Greek form of the term *semi-oviparous*.



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Reptiles, and the other towards the three higher classes of Vertebrates collectively, but especially Mammals and Birds.

It is plain from the preceding that the sub-kingdom of Vertebrates, instead of tailing off into the Invertebrates, has well pronounced limits below, and is complete within itself.

2. *Distinctive Features of the Reptilian Division of Birds.*—The skeleton of the fossil Bird, discovered at Solenhofen, has some decided Reptilian peculiarities, as pointed out by Wagner, Owen, and others. But even if perfect, it could not indicate all the Reptilian features present in the *living* animal. It is therefore a question of interest, whether the relations of the hemitypic to the typical species in the two classes Mammals and Reptiles—one superior to that of Birds, and the other inferior—afford any basis for conclusions with regard to characteristics of the hemitypic Birds undiscoverable by direct observation. The following considerations, suggested by analogies from the classes just mentioned, may be regarded as leading to unsatisfactory results; and yet they deserve attention.

A. *Mammals.*—(1.) It is a fact to be observed that the hemitypic Mammals are as truly and thoroughly *Mammalian*, as regards the fundamental characteristic of the type—the suckling of their young—as the typical species.

(2.) The departure from the typical Mammals is small in the *adult* individuals, especially the adult males. But it is profoundly marked in their *young*, they thus approximating in period of birth and some other respects to oviparous Vertebrates.

B. *Reptiles.*—(1.) The *adult* Amphibians, or hemitypic Reptiles, depart but little from the typical Reptiles, either in structure or habits.

But (2.) the *young*, in their successive stages, from the egg upward, partake strikingly of characters of the inferior class of Fishes.

The law seems, then, to be, that the species of the hemitypic group have their principal or most fundamental resemblance to those of the class or classes below in the *young* state. We should hence conclude that the *young* of the Reptilian Birds or Erpetoids possessed more decided Reptilian peculiarities than the adults.—What these unknown peculiarities, if real, were, we can infer only doubtingly from the analogies of the known cases already considered.

The characteristic of the intermediate type, on which the intermediate character depends, is, in the case of both Mammals and Reptiles, that particular one which is the special distinction of the inferior type. The types inferior to Mammals are *oviparous*, and hence the hemitypic Mammals are semi-oviparous. The type

inferior to Reptiles, or that of Fishes, is distinctively *aquatic* and breathes consequently by means of *gills* instead of lungs, and hence the hemitypic Reptiles have gills in the young state.

What then are the characteristics of Reptiles that may have been presented by the inferior or hemitypic Birds? The more prominent distinctions of Reptiles are the following:—

(1.) A covering of scales, or else a naked skin, instead of a covering of feathers.

(2.) A terrestrial creeping mode of life instead of an aerial or flying mode.

(3.) Incomplete circulation, and hence, to some degree, cold-blooded, instead of complete and warm-blooded.

Now, as to the young of the Reptilian Birds, it may be inferred that—

(1.) They were unquestionably unfledged. For this is universal among birds, for a while after leaving the egg. It is quite probable, that they were more completely unfledged, or for a longer time, than is common for the young of ordinary birds; for even the adult bird, judging from the Solenhofen specimen, was less completely feathered than usual.

(2.) They were unquestionably *walking* chicks. For Birds in the lower division of the class (*Præcoces* of Bonaparte) have the use of their legs immediately after leaving the egg, and seek their own food. A brood of Reptilian bird-chicks, with long tails and nearly naked bodies, creeping over the ground, would have looked exceedingly like young Reptiles—very much, indeed, as if the eggs of a Reptile had been hatched by mistake. Moreover, these Reptilian Birds were probably not only walking birds when young, but as much so as hens and turkeys are, if not more exclusively so, even when adults; for, in the inferior division of ordinary birds, the species are far inferior as flying animals to those of the superior division, and in some, as is well known, the wings only aid in running.

(3.) But the characteristics which have been mentioned under (1) and (2) are not of fundamental value, like that of the existence of gills in the young of hemitypic Reptiles, or that of the semi-oviparous method of reproduction in Oötocoid Mammals; and it would seem that there must have been some more profound Reptilian characteristic. It is therefore probable that the third distinction of Reptiles stated belonged also to the young Reptilian Bird; that is, it had incomplete circulation, and hence, an approximation to the cold-blooded condition of Reptiles. The heart may have had its *four* cavities complete, as in Birds, and in Crocodiles among Reptiles; but, in addition, there may have been a passage permitting a partial admixture of the venous and arterial blood, such as exists not only in Crocodiles but also in

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the young Bird during an early stage in its development. This peculiarity in the vascular system of the young Bird of the present day ceases with the beginning of respiration. But in the Reptilian birds it may have continued on through the early part, at least, of the life of the chick, or until it was fledged.

This conclusion is made to appear still more reasonable by the following comparison of the three obvious methods of subdividing Vertebrates, and the connection therewith of the characteristics of the hemitypic groups. These three methods are—

1. Into *viviparous* and *oviparous*; which places the dividing line between Mammals, and the inferior Vertebrates.

2. Into *warm-blooded* and *cold-blooded*, or those having perfect, and those having imperfect, circulation; which places the line between Mammals and Birds, on one side, and Reptiles and Fishes, on the other.

3. Into *pulmonate* and *branchial*, or those with lungs, and those with gills; which places the line between Mammals, Birds, and Reptiles, on one side, and Fishes on the other.

Now the characteristic of the *first* of these methods of subdivision is that on which the hemitypic group of the first class, or that of Mammals, is based. The characteristic of the *third* is that on which the hemitypic group of the third class, or the Reptilian, is based. Hence, the characteristic of the *second* should be, if the analogy holds, that on which the hemitypic group of the second class, or that of Birds, rests for its most fundamental distinction.

3. *Geological History*.—It has been observed, on page 78, that the Vertebrate sub-kingdom has well-drawn limits below, instead of tapering downward into Mollusks or Articulates. This feature of the sub-kingdom is further evident from the fact in geological history that the earliest species of Fishes were not of the *lower* group, that of Teliosts, but of the two higher, or those of Ganoids and Selachians. The Vertebrate type did not originate, therefore, in the sub-kingdom of Mollusks, or of Articulates; neither did it start from what might be considered as its base, that is, the lower limit of the class of Fishes; but in intermediate types, occupying a point between typical Fishes and the classes above.

Moreover, the inferior group did not come into existence until the Cretaceous period, in the latter part of geological history, when the Reptilian age was commencing its decline.

In the Devonian age, or closing Silurian, appeared the first Ganoids and Selachians. In the Carboniferous, Reptiles were introduced,—first, the inferior Amphibians, and then typical species. Afterward, in the early part of the Reptilian age, as Reptilian life was in course of expansion, there were the first of

the Reptilian Birds and the first of the Marsupials or hemitypic Mammals (with probably some typical species of each of these classes). Thus the Vertebrate type, commencing at the point of approximation of Reptiles and Fishes, expanded until each of its higher classes had representative species, before the inferior division of true or typical fishes—Teliosts—came into existence. Afterwards, in the Cenozoic, the true or typical Birds and Mammals had their full expansion.

The Vertebrate type, therefore, not only was not evolved along lines leading up from the lower sub-kingdoms, but was not, as regards its own species, brought out in lineal order from the lowest upward. The sub-kingdom has therefore most evidently a separateness and a roundness below, so to speak, or an entireness in its inferior limits, which belongs only to an independent system.

We find in the facts no support for the Darwinian hypothesis with regard to the origin of the system of life.

II. *The Classification of Animals based on the principle of Cephalization.*

No. I.

As the principle of cephalization is involved in the very foundation of the diverse forms that make up the Animal Kingdom, we may look to it for authoritative guidance with reference to the system that prevails among those forms. Some of its bearings on zoological classification have already been pointed out.* I propose to take up the subject more comprehensively; and, in the present article, to bring the light of the principle to bear on the relations of the Sub-kingdoms, Classes, Orders, and some of the tribes of animal life.

It is essential, first, that the methods or laws of cephalization be systematically set forth, that they may be conveniently studied and compared. The following statement of them is an extension of what has already been presented:—

As an animal is a *cephalized* organism (or one terminating anteriorly in a head), the anterior and posterior extremities have opposite relations. The subdivision of the structure into *anterior* and *posterior* portions has therefore a special importance in this connection. As these terms are used beyond, the *anterior* portion properly includes the head, which is the seat of the senses and mouth, with whatever organs are tributary to its purposes,

* Expl. Exp. Report on Crustacea, p. 1412. 1855; Amer. Jour. of Science and Arts [2], xxii. 14, 1856; xxxv. 67, xxxvi. 1, 1868.

anterior in position to the normal locomotive organs; the *posterior* portion is the rest of the structure. The anterior is eminently the cephalic portion. The digestive viscera from the stomach backward, and the reproductive viscera, belong as characteristically to the posterior portion.

It follows, further, from the cephalized nature of an animal, that its *primary centre of force*, or the point from which concentration and the reverse are to be measured, anteriorly and posteriorly, is in the head, near the anterior extremity of the structure. In an Insect or Crustacean, its position is between the mouth and the organs of the senses—over which part the cephalic mass is located. This is sustained by embryogeny; and also by the fact, that, as the two most fundamental characteristics of an animal are its being sense-bearing and mouth-feeding, the mouth, on descending to the simplest of animals, is the last part to become obsolescent. Only in the inferior Invertebrates is the position of the mouth approximately *central* in the structure, as explained on page 90.*

1. *Methods of Cephalization.*

The methods, according to which the grades of cephalization are exhibited, may be arranged under the following heads:

A. *Size (force-measured) of life-system*: each type, between Man at one extreme and Protozoans at the other, having its special range of variation in this respect.

B. *Functional*: or variations as to the distribution of the functions *anteriorly* and *posteriorly*, and as to their condition.

C. *Incremental*: or variations as to vegetative increment, that is, as to amplitude, and multiplicative development.

D. *Structural*: or variations in the conditions of the structure,—whether (1) compacted, or, on the other hand, resolved into normal elements; (2) simple, or complex by specialization; (3) defective, or perfect; (4) animal-like, or plant-like.

E. *Postural*: or variations as to posture. (Only in Vertebrates.)

F. *Embryological*: or variations connected with the development of the young.

G. *Geographical distribution.*

For greater convenience and uniformity, the methods under these heads are mentioned beyond as they appear when viewed along the *descending* line of grade, instead of the ascending. This is, in fact, the more natural way, since the typical form in a

* There may also be one or more *secondary* centres of force; but they are, as regards the subject before us, of comparatively small importance. The independent development of the abdomen and cephalothorax in Crustaceans is a case of the kind.

group—the fixed point for reference—holds a position towards the top of the group. The methods, as given, are therefore more strictly methods of *decephalization* than of cephalization; but the former are simply the reverse of the latter.

A. SIZE (OR FORCE) OF LIFE-SYSTEM.

1. *Potential*.—Exhibited in less and less force and size of life-system with decline of grade (and the reverse, with rise of grade); as that in passing from the type of Megasthenes (Quadrumanes, Carnivores, Herbivores, and Mutilates) to that of Microsthenes (Chiropters, Insectivores, Rodents, and Edentates); or from that of Decapods to that of Tetradeapods among Crustaceans—in which latter case, unlike the former, there is also *retroferent* decephalization; and so, generally, in passing from a higher to a lower type, it being equivalent to passing to a type of smaller and weaker life-system.

B. FUNCTIONAL.

2. *Retroferent*.—A transfer of functions backward that belong anteriorly in the higher cognate type.

Under this method, there are the following cases:—

a. A transfer of members from the cephalic to the locomotive series; as the transfer of the forelimbs to the locomotive series in passing from Man to Brute Mammals; that of a pair of maxillipeds or posterior mouth-organs to the locomotive series in passing from Insects to Spiders; that of two pairs of maxillipeds to the locomotive series in passing from Decapod to Tetradeapod Crustaceans.

b. A transfer of locomotive or prehensile power and function, more or less completely, from the anterior locomotive organs to the posterior.

c. A transfer of the locomotive function, more or less completely, from the limbs (these often becoming obsolete) to the body, and mainly to the caudal extremity.

Under *b* and *c*, the condition may be described as—

(a) *Prosthenic* (from the Greek *pro*, before, and *sthenos*, strong), if the anterior locomotive organs have their normal superiority.

(b) *Metasthenic* (from *meta*, after, &c.), if a posterior pair is the more important, and the anterior are weak or obsolete.

(c) *Urosthenic* (from *oura*, tail, &c.), if the posterior part of the body, or the caudal extremity, is the main organ of locomotion.

Ordinary flying Birds are *prosthenic*, while the *Præcoces* (Gallinaceous Birds, Ostriches, &c.), being poor at flying, or incapable of it, are *metasthenic*, and they thus exhibit their inferiority of grade. Hymenopters, Dipters, Lepidopters, &c., among Insects, are *prosthenic*, while Coleopters, Orthopters, Strepsipters, &c., in which the fore-wings (the *elytra*) do not aid in flight, or but little,

are *metasthenic*. Fleas, which are degradational species, related to Dipters, have the third or *posterior* pair of legs much the longest and strongest. Among Macrural Crustaceans, the strongest legs are, in the higher species, the *first* pair; in others inferior, the *second*; in others still inferior (the Penæids), the *third* pair.

Viewed on the ascending grade, this method is the *preferent*.

3. *Pervertive*.—A subjection of an organ to any abnormal function inferior to that normal to it; as in the adaptation of the nose of the Elephant to prehension; of the antennæ of many inferior Crustaceans to prehension or locomotion; of the maxillipeds of inferior Macrurans to locomotion; of the forehead in many Herbivores to purposes of defence.

The perverted nose of the Proboscideans is one of the indications of their inferiority to the Carnivores; but it is not necessarily a mark of Inferiority among Herbivores themselves, as the faculty of prehension is one of those especially characterising Carnivores and other higher Mammals, and nearly all Herbivores fail of it.

Viewed on the ascending grade, this method and the following may be included under the term *perfunctionative*.

4. *Defunctionative*.—Exhibited in the defectiveness or absence of the normal function of an organ; as in the absence of the function of prehension from the fore-limbs of Herbivores (this prehension in the fore-limbs belonging to the Mammalian type); and that of locomotion mostly from all the limbs in the Mutilates; that of locomotion from the female Bopyrus; that of locomotion from Cirripeds and other attached animals; that of the sense connected with the *second* pair of antennæ (and probably also the *first*, these organs being obsolete) in the Lernæas and Cirripeds, these antennæ being simply prehensile organs in a Lernæa, and constituting the base of the peduncle in an Anatifa.*

This degradation and loss of functions is connected often with the *elliptic* and *amplificative* methods of decephalization. It is connected with the latter in the Bopyrus, and also in Cirripeds and other attached species.

C. INCREMENTAL.

5. *Amplificative*.—Exhibited in an elongation or general enlargement of the segments or members, and an increased laxness of the parts. Includes the cases—

a. Lengthening, widening, or laxness in the *anterior* portion of the body; the same in the *posterior* portion.

* See "Expl. Exp. Report on Crustacea," p. 1393, and plate 96, where it is shown that the antennæ of the young Anatifa have a sucker-like organ for attachment, and become, in the metamorphosis, the bottom of the peduncle by which the adult Anatifa is attached.

* *b.* An abnormal enlargement of the general structure.

The elongation or enlargement which takes place with decline of grade is mainly *posterior*, it being small anteriorly, and sometimes none at all. In passing from the Brachyural to the Macrural type of Crustaceans, the change anteriorly is principally in an increased laxness and lengthening of the parts, with little increase in the dimensions of the body anterior to the mouth; while the abdomen (or *posterior* extremity) is enlarged 10 to 50 times beyond the bulk it has in the Crab. Descending from a snail to an oyster, there is diminution anteriorly and great enlargement posteriorly, and the animal is little more than a visceral sac.

In less marked cases of the *amplificative* method, there is only an attenuation or lengthening of the body and limbs, as in many Neuropters, Orthopters, Homopters, wading Birds, &c. The Lepidopters, also, in their very great expanse of wing, exemplify this method. In species that are attached, as the Cirripeds, the young are usually free; and it is only when they begin to out-grow, *amplificately*, the minute life-system (Entomostracan in the Cirripeds) that they become fixed. As attached animals, they often attain great size.

Viewed on the ascending grade, this method is the *concentrative*; and it is exhibited in the increased abbreviation and condensation of the anterior and posterior members and segments, or of the whole structure.

6. *Multiplicative*.—Exhibited in an abnormal multiplication of segments or members; as in Myriapods, Worms, Phyllopods, Trilobites, &c. There may be—

a. Simple Multiplicative; as in the superior Myriapods, the Chilopods, in which the body-segments, thus multiplied, have each its single or normal pair of members.

b. Compound Multiplicative; as in the Myriapods, of the Iulus division, or Diplopods (Chilognaths), in which there is a duplication of the pair of legs of a body segment. The name *Diplopod*, adopted by Gervais and some other authors, has the advantage of having thus a dynamical value.

The multiplicative method is, in general, a degradational one. When it affects only subordinate parts of the structure, as the length of the tail of Mammals, or of Reptiles, &c., the forms are not necessarily degradational. But when it affects the general structure, and the types are indefinite in segments, like the Myriapods, Worms, and Snakes, the forms are degradational. In Mammals, the tail may be said to have indefiniteness of limit; but, since this part is only an appendage to the body and has little functional importance, its elongation cannot properly be regarded as a mark of degradation, although one of inferiority.

When, however, the posterior extremity is, in magnitude and importance, a part of the main body structure itself, as in Snakes and Fishes, the case is properly an example of multiplicative degradation.

The abnormal number of segments under the multiplicative method may arise from a self-subdivision of enlarging normal segments, or from additions beyond the range of the normal number. The many joints of the antennæ in Crustaceans of the Cyclops group, the writer has shown to result through the former method, and the multiple segments of Phyllopods may be of the same origin: but there are no facts yet ascertained that would refer the multiplication of segments in Myriapods and Worms to this method.

Viewed on the ascending grade, this method is the *limitative*.

D. STRUCTURAL.

7. *Analytic*.—Exhibited in a resolving of the body-structure, or of an organ, more or less completely, into its equal normal elements, or in a tendency to such a resolution.

A relaxed state of the cephalic power leads to a relaxed and elementally-constituted structure. When this method characterizes strongly the general structure, the form is usually degradational; as in Myriapods, Worms, larvae of Insects,—these structures consisting of a series of nearly similar rings (the normal elements of an Articulate), without a subdivision into head, thorax, and abdomen. Fishes, of the Vertebrate type, are, as nearly as may be, in this elementalized condition. An approximation towards analysis or resolution of the body appears in the absence of the constriction between the head and thorax in Spiders and Crustaceans; and still further, in the absence of the constriction between the thorax and abdomen in the lowest of Spiders,—the Acaroids.

Under this method, there is, in no case, among adults, or larvae, a complete analysis or resolution of the head into normal segments; the closest approximation to it, Insecteans and Crustaceans, occurs in the Gastrurans (*Squilla* group). But here the mandibular, and one, two, or more maxillary segments are still united. In an Insect, the head contains six normal segments, and the thorax three; and yet the thorax has 3 to 5 times the bulk of the head;—showing a condensation in the head-part equal to 6 to 10 times that of the thorax. Concentration in an animal structure is therefore eminently cephalic concentration, or, in a word, *cephalization*,—the head being the part most condensed, and least liable to occur resolved into its elements.

The analytic method, viewed on the ascending grade, is the *synthetic*.

8. *Simplificative*.—Exhibited in increased simplicity of structure, and in an equality of parts that are normally identical. The cases are—

a. Simplicity from diminished number of internal or external organs for carrying on the processes of life; as in the absence of distinct respiratory organs, or of different parts in the digestive system, &c.; or the union of the sexes in one individual, &c.; —a simplification which reaches its extreme limit among Radiates in the Hydra, and among animals, in the Protozoans.

b. Simplicity from equality in parts normally alike; as, equality in the height of the teeth of some of the earliest of Tertiary Mammals; in the annuli of Worms. This case is related to the analytic.

Viewed on the ascending grade, this method is the *differentiative*, the facts exhibiting which are embraced under the well-known law of differentiation or specialization, which is fundamental in all development.

Differentiation internally, as it multiplies and perfects the means of elaborating the structure, is attended with an increasingly higher grade of chemical change, more perfect nutrition, and more complete decarbonization of the blood; and implies, therefore, improvement in all tissues, a more sensitive nervous system, and greater cephalic power and activity. And from the reverse comes the reverse effect.

9. *Elliptic*.—Exhibited in the defectiveness or absence of segments or members normally pertaining to the type of the order or class containing the species, and arising from *abnormal weakness* in the general system, or in an organ. It is exhibited especially in the degradational or inferior types. The cases are—

Incomplete or deficient (1) segments, or (2) members, in either (a) the *anterior*, or (b) the *posterior* portion of the body; as in the absence of some or all, of the teeth in Edentates; of the posterior limbs in Whales; of the abnormal appendages and posterior thoracic segments in some Schizopods or degradational Macrurans; of the antennæ, either one or both pairs, in many inferior Entomostracans; of wings in the Flea, &c.

This method of decephalization differs from the defunctionative in implying a deficiency not only of function but also of organ or member.

The incompleteness or deficiency of normal parts referred to above will be better appreciated if contrasted with deficiencies from other causes. The principal other causes are the following:—

(1.) A *high degree* of cephalization or cephalic concentration in the system.—Thus in the crab, the highest of Crustaceans, the abdomen is very small, and *elliptic* both in segments and members,

because of the *high degree* of cephalic concentration ; while in the Schizopods referred to above, and in the *Limulus* and many other inferior Crustaceans, the same deficiency comes from *weakness* of life-system or decephalization.

(2.) High development of one part of an organ, at the expense of other adjoining parts.—This principle may be said to include the preceding, since, in that, there is a high development of the anterior or cephalic portion of the structure at the expense of the posterior or circumferential. But here, there is reference to special organs rather than to the structure as a whole. Thus, in the foot of a Horse, there is an enlargement of one toe, normally the third, at the expense of the others, and this enlarged toe has the full normal strength that belongs to the foot under the Herbivore-type.

It is apparent from the facts in paragraphs (1) and (2), that there may be an *elliptic* method of *cephalization* as well as of *decephalization*. The Crab-type is a striking example of the former. The foot of the Horse, considering separately the *Horse-type*, is a case under the former rather than the latter ; for, in any related species, a lessening of the disparity of the toes would be evidence of weakness and inferiority *under that type*. Yet, as compared with the higher Carnivore-type, in which the life-system has the strength to develop all the toes in their completeness and fullness of vigour, with great strength of foot, the foot of the horse is *elliptic*, and a mark of inferior cephalization. In the typical Ruminants, the complete series of teeth is indicated in an embryonic state before birth ; but part of them fail of development, while the others—those specially characteristic of the type—go forward to great size and perfection. As in the foot of the Horse there is here an enlargement of one portion at the expense of the others. And this, under the Ruminant-type, is progress toward the highest condition of the type, or *cephalization* by an elliptic method. A Ruminant in which the teeth should be all equally developed would be one of too great feebleness of system to carry the structure to its typical perfection ; and such is the Eocene Anoplothere.* If, however, the Ruminants were referred to the Megasthene-type

* "Amongst the varied forms of existing Herbivora we find certain teeth disproportionately developed, sometimes to a monstrous size ; whilst other teeth are reduced to rudimental minuteness, or are wanting altogether : but the number of teeth never exceeds, in any hoofed quadruped, that displayed in the dental formula of the Anoplotherium. It is likewise most interesting to find that those species with a comparatively defective dentition, as the horned Ruminants for example, manifest transitorily, in the embryo-state, the germs of upper incisors and canines, which disappear before birth, but which were retained and functionally developed in the cloven-footed Anoplothere."—*Goodvir, British Assoc. Rep.* 1838. *Owen's Brit. Mamm.*, 1846, p. 433.

as represented in the Carnivores, the *deficiency* of teeth would be an example of *decephalization* by the elliptic method; for such a deficiency under the higher type of the Carnivores would be evidence of abnormal weakness.

The same principle is exemplified in Carnivores; for the size and number of the molar teeth are less the larger the canines. The *Machærodus*, with its huge tusks and but *three* molars to either side of a jaw, is a remarkable example. Again, in the Elephant, two incisors are developed into the great tusks of the upper jaw at the expense of the other incisors and canines; and jaws that look as if bearing profoundly the mark of degradation or *decephalization*, are hence compatible with high *cephalization* under the Herbivore-type.

It is not to be inferred that the enlargement of one part of an organ at the expense of the others, is *necessarily* an indication of *general* elevation of grade. Even in the case of the foot of the Horse, the elevation implied is elevation only under the Horse-type or among *Solidungulates*, and not elevation above all other Herbivores.

These examples are sufficient to illustrate the contrast between the elliptic method of cephalization and of *decephalization*; and also the fact, that a case of the former in one relation may be one of the latter in a higher, that is, if referred to a higher group as the standard type. The cases that would come under the elliptic method of *cephalization* (as that of the Crab) have been already referred to by the writer to the *concentrative*, they being a result of concentration in the life-system.

(3.) That simplicity of structure which is opposed to the specialized or differentiated condition of superiority of type.—It is evident that the examples of elliptic *decephalization*, taking this term in its most comprehensive sense, may include the various simplifications which mark unspecialised structures of inferior types. Yet we propose to restrict the term to those examples of deficiencies which are obviously connected with degradational or hypotypic conditions under any type.

Viewed on the ascending grade, this method is the *completive*.

10. *Phytosomic*.—Exhibited in a departure from the Animal-type through a participation in structural features of the Plant-type, that is, through a plant-like arrangement of the organs. The cases are—

a. A radiate arrangement of external organs; as in the *Bryozoans* and inferior *Tunicates*.

b. A radiate arrangement of internal as well as external organs; as in *Radiates*.

c. Perfect, or nearly perfect, symmetry in the radiation, instead of eccentric or irregular forms. Perfect symmetry is most general where the number of rays is based on the numbers 4 or 6

(which, it is to be noted, are multiples of 2 and 3), 4 being the number for the class of *Medusæ*, and both 4 and 6 occurring in that of *Polyps*. But if the number of rays is 5, as in the highest of *Radiates*, the *Echinoderms*, while examples of perfect symmetry occur, there are many cases of unsymmetrical forms (as in the *Spatangi*) in which the *Radiate* type seems to tend to emerge from phytoid towards true animal-like forms. In the regularly radiate, the mouth is central or very nearly so, while in the *Spatangi*, there is something of the fore-and-aft form of the animal.

Among species under the true animal-type there are forms showing an approximation to the central position which the mouth has in *Radiates*. In a *Limulus*, for example, the mouth-aperture is only one-half less remote from the anterior margin of the body than from the posterior (base of caudal spine). The *Limuli* are extreme in *amplificative* decephalization and in lowness of grade. Under the *multiplicative* method also, there is something similar in *Worms* and *Myriapods*. The head is here strictly at the anterior extremity; but the cephalic force has so feeble control, that joints multiply behind; and in the lowest of *Worms*, each separate segment is nearly equal in all functions to the cephalic segment. Moreover, in the embryological development of an *Annelid*, the first segment (with its pair of appendages) that is formed after the appearance of the head is not the anterior one close to the head, but the *eighth* (or one near this); and from this point the rings form in succession posteriorly, and also towards it from the head; as if, in these *multiplicate* species, there was a *secondary centre of force* distant from the front which preponderates over the *primary* one.

This method viewed on the ascending grade is the *holozoic* (from *ἅλος*, all, and *ζῷον*, animal); it is exhibited in a rise from the plant-like type to the true animal-like type.

E. POSTURAL.

11. *Postural*.—Exhibited in an increasing proneness in the position of the nervous system—the extremes being *verticality* in *Man*, and *horizontality* in the *Fish*.

F. EMBRYOLOGICAL.

12. *Prematurative*.—Exhibited in precocity of young or larvae.

Thus, the chicken, as soon as born, runs about and seeks its own food, while the young of those *Birds* which belong to the superior group,—the true flying *Birds*—remain helpless until able to fly; a fact recognised in Bonaparte's classification of *Birds*. So the young colt or calf (*Herbivorous*) is on its legs almost as soon as born; but the young kitten (*Carnivorous*, and higher in type) is for a considerable time helpless.

Prematurity has often been recognised as evidence of low

development and low rank; and the following is the explanation of it:—

When an animal has reached the condition required for locomotion and for the care of itself, it has already the essential faculties of an adult; and although these faculties of locomotion and self-feeding are of comparatively low grade, the animal possessing them is approximately mature in its cephalic forces, and afterwards rises but little with growth. Prematurity hence involves inferiority. The pupa-state of an Insect is a means of higher development the more perfect its inactivity. For this complete rest allows all the forces of the individual to be concentrated on the internal processes, and favours, therefore, that cephalic growth which makes a special demand on these forces; while in an *active* pupa (or rather the larve that passes through no pupa-state), activity, whether that of locomotion, or of digestion, constantly exhausts force; and only the balance, not thus run away with, goes towards the maturing process. With such an open outlet of force, the animal may mature physically, that is, grow and perfect its outer structure; but cephalically, or, in all those points of structure, as well as psychical powers, that are connected with superior cephalic development, it makes little advance.

Hence (a), those insects whose larves are essentially like the adults, and undergo no metamorphosis, are inferior in type,—as generally so recognised.

Again (b), those Insects (as most Hymenopterous) whose larves are footless grubs are superior in type to those (as the Lepidopterous) whose larves are most highly developed and active.

Viewed on the ascending grade, this method is the *permativative*.

13. *Gemmative*.—Exhibited in multiplication by buds. Budding may produce—

a. Perfect individuals, capable of egg-production.

b. Individuals capable only of budding, and giving origin to a perfect egg-producing individual, as the last of a series of buddings.

c. Caducous, or persistent buds; the *latter* leading to compound forms, either branching, lamellar, or massive.

This power of reproduction by buds occurs in many Worms, both superior and inferior; in Bryozoa and many Ascidian Mollusks; in Polyps and many other Radiates. The production of persistent buds is the lowest grade, and is common in the budding Mollusks and Radiates, but not the Articulates. Among budding Articulates, case *b* appears to be of lower grade than case *a*.

This method is allied to the *multiplicative*, p. 85. It is also *phytozoic* (p. 89), or a plant-like feature in animal life.

14. *Genetic*.—*Number of young or eggs*.—As is well known, there is a mark of grade in the number of eggs or young produced at a single period or in a given time—the number, other things equal, being inversely as the rank or grade of the species.

15. *Thermotic*.—*Temperature required for embryonic development*.—Another mark of grade is afforded by the temperature required for egg-development:—for, in general, the higher the temperature, the higher the grade. Thus, the eggs of Birds require heat above ordinary summer heat, while those of Reptiles do not. The embryos of Mammals require still higher and more uniformly continued heat until their maturity, the Oötocoids alone excepted, in which birth is premature. The eggs of some Hymenopterous Insects mature inside of the larvae of other Insects, where they are never exposed to a temperature of 32° Fahr.; while those of ordinary Lepidoptera and many other species mature in the summer heat, and may stand a temperature below 0° Fahr.

The necessity of a higher temperature indicates, ordinarily, that the chemical processes in the vital economy are of a higher or more delicate character, or those required for a higher grade of cephalization.

G. GEOGRAPHICAL DISTRIBUTION.

16. *Habitational*.—(1.) *Terrestrial species higher than Aquatic*.—This law, announced by Agassiz, is also directly dependent on the conditions determining the grade of cephalization.

a. In the case of *aquatic* species, the ova, as well as the adult animals, are bathed in a liquid that penetrates to the interior, and dilutes, to some degree, the nutrient or developing fluids; and, under such circumstances, the grade of chemical or vital evolution cannot be as high as in the atmosphere. The germ must therefore be one of an inferior kind. Aquatic animals are, in an important sense, *diluted* animals.

b. Again, *terrestrial* species whose ova are hatched in water, or whose young are aquatic, are for the same reason inferior, as a general rule, to those whose ova are hatched on the land.

Aquatic development or life is one of the most important marks of low grade. Among embryological characteristics, it has often a profounder value than prematurity. The *inferior division* of a class, order, tribe, and even subordinate group, is often one consisting either of *aquatic* species, or those that are *semi-aquatic* (aquatic in habit though not strictly so in mode of life, or aquatic in the young state when not in the adult).

(2.) *Living (a) in impure waters, or those abnormal in condition; or (b) in deficient light, as in shaded places, or the ocean's depths, a mark of inferiority*.—Muddy waters, or salt waters excessively

saline as in some inland lakes, or waters only brackish, are here included.

But *oceanic waters*, although saline, are not properly impure. Of the sub-kingdoms and the classes containing aquatic animals, the *highest* groups are those of *marine waters*. Thus, the highest of Mollusks, the Cephalopods, are marine; the highest of Radiates, the Echinoderms; the highest of Fishes, the Selachians; of Crustaceans, or the Maioid or Triangular Crabs; of Worms, the Dorsibranchs; of Acalephs, all but the Hydroids are marine; while *all* species of Echinoderms and Polyps are marine. Among the subordinate groups there are some fitted particularly for fresh water. Types that belong to fresh water sometimes have inferior species in brackish or salt water; and those that belong to salt water sometimes have inferior species in brackish or fresh water.

(3.) *Species of cold climates inferior to those of warm.*—According to the 15th canon, the highest oviparous animals should be tropical species; but not necessarily so the viviparous Mammals, since, with them, the requisite temperature for embryonic development is obtained within the parent.

An exception to this, as regards oviparous species, is afforded by Crustaceans; for, as shown by the writer, the highest kinds, the Maioid or Triangular Crabs, have their fullest development in the cooler temperate zone.

(4.) *Having a wide range with regard to any of the earth's physical conditions, as (a) climate, (b) height, (c) oceanic temperature, (d) oceanic depth, (e) hygrometric conditions, &c., commonly a mark of inferiority.*—For if the development of a high order of cephalized life requires rest for a while in the young, as, for example, the nursing time in the higher Mammals and Birds, and the Pupa state in Insects, and also an absence from diluting or impure waters and the presence of the full light of the sun, it should also equally demand precise or narrowly restricted limits in all physical conditions, these being essential to the more refined or delicate chemical or vital processes. Man is the chief exception to this law,—and for the reason that he is not simply in and of nature, but also above nature, and has the will and power to bring her forces under subjection, overcoming the rigours of climate, and subjugating other inimical agencies by his art. Protophytes and Man are the only species that have the range of the world—the one because so low, the other so high. The Dog accompanies Man in his wide wanderings: but only through the virtue which is in Man, who provides the artificial heat, protection, and food his brute attendant needs. Even the human race dwindles in extremes of climate, either hot or cold.

Recapitulation.—The following are the names of the several

methods of cephalization pointed out, both those based on the descending and ascending lines of grade.

	Descending.	Ascending.
A. Size of Life-system,	1. Potential.	1. Potential.
B. Functional, . . .	2. Retroferent.	2. Preferent.
" . . .	3. Pervertive.	3. } Perfunctionative.
" . . .	4. Defunctionative.	4. }
C. Incremental, . . .	5. Amplificative.	5. Concentrative.
" . . .	6. Multiplicative.	6. Limitative.
D. Structural, . . .	7. Analytic.	7. Synthetic.
" . . .	8. Simplificative.	8. Differentiative.
" . . .	9. Elliptic.	9. Compleitive.
" . . .	10. Phytozoic.	10. Holozoic.
E Postural, . . .	11. Postural.	11. Postural.
F. Embryological, .	12. Prematurative.	12. Prematurative.

The remaining terms fall into both columns.

With *ascending* grade, the changes are mostly *concentrative*; with *descending*, they are *diffusive* or *decentrative*.

2. Additional Observations.

1. *Typical, Degradational and Hemitypic forms.*—Typical species are those within type-limits, and *degradational* those outside of the same.* But, as groups of all grades have each their own type and type-limits, species may be typical in one relation, and degradational in another; as Fishes, for example, while degradational Vertebrates, have still their own type and type-limits, the Teliosts being the typical Fishes, or those within these limits.

The characteristics of a type, in any case, are those fundamentally distinctive of the group. As to that of the Animal Kingdom at large, we observe that an animal is (1) a fore-and-aft, (2) cephalized, (3) forward-moving organism. The type-idea is hence expressed in a structure having (1) fore-and-aft and dorsoventral polarity; (2) a head at the forward extremity containing the seats or organs of the senses, as well as the mouth and mouth organs; and (3) the power of locomotion, if not also limbs for the purpose. Consequently Radiates, as they fail in the first criterion, are not within type-limits; neither are any *attached* species of animal, and only in a partial degree species without limbs for locomotion.

Again, the Vertebrate-type, in addition to having the characteristics of the animal type and the vertebrate structure, is essentially terrestrial, and therefore the requisite limbs and structure

* The term *degradational* has no reference to any method of origin by degradation: it implies only that the forms so called represent or correspond to a degraded condition of the type.

for terrestrial life are in the type-idea. Fishes are therefore outside of type-limits, or are degradational species.

The Mammal-type, the highest under Vertebrates, in addition to the characteristics of the Vertebrate type, has that of being viviparous in its births, embracing under this quality that of sustaining the embryo by placental nutrition until its maturity (as is not true of the oviparous); and with this there is also that of sustaining the young for a while after birth, by suckling. Hence the Oötocoids, in which there is only imperfect placental nutrition, and birth is premature, and there is an approximation thus to oviparous species, constitute a degradational type.

The Megasthene-type, under Mammals, has its degradational group in the Cetaceans or Mutilates, which fail mostly of limbs, and are aquatic species; and the Carnivore, its degradational group in the Seal and related Pinnipeds. The latter have the type-structure of the Carnivores, while the Mutilates have the type-structure of neither Carnivores nor Herbivores, and are therefore an independent type under the division of Megasthenes.

Again, the Bird-type, in addition to the characteristics of the Vertebrate-type, embraces features adapting the animal to flying, as feathers and wings; perfect circulation; and also a vertebral column which is posteriorly limitate, instead of one admitting of a caudal elongation—somewhat as Insects and Spiders are *closed* types behind, in contrast with the *multiplicate* Myriapods. Hence the Reptilian Birds, having *indefinite* posterior elongation, and some other Reptilian characteristics, are outside of type-limits. So, again, under the subdivisions of Birds, species that have the wings unfledged or but half-fledged, and which, therefore, cannot lead an *aerial* life, are degradational; and species that have the feet imperfectly digitate by their being web-footed, and which therefore lead a *semi-aquatic* life, are semi-degradational in the group to which they may belong.

These examples are sufficient to illustrate the uses of the words typical and degradational.

It is of the highest importance, for the correct classification of species, that in all cases it should be rightly determined whether a degradational genus is degradational to the *family* to which it belongs, or to the *tribe* or *order*, or to a still higher division. Although Seals and Whales are similarly adapted to the water, it is plain to one familiar with the species that the former are degradational Carnivores, and the latter degradational Megasthenes, as stated above. But like cases come up in every part of the Animal Kingdom, and close study is necessary for a true decision. The first preliminary towards such a decision is a clear idea of the class-type, order-type, tribe-type, or subordinate type under which the genus or group falls.

The term *hemitypic* has been shown in the preceding paper to imply in general a grade of the degradational. But, in some groups, as in the class of Fishes among Vertebrates, it is applicable to cases which are not typical because of their being intermediate between the type of the group and a *superior* type or types (p. 77).

Typical groups, or more properly, the groups above the degradational, may be of several grades. Thus under Vertebrates, the classes of Mammals, Birds, and Reptiles, represent different grades of Vertebrate types, and the grades may be designated in order, *Alphatypic*, *Betatypic*, *Gammatypic* (from the first three Greek letters, α , β , γ). Under Mammals also, there are three grades, those of Man, Megasthenes, and Microsthenes; then, below these, the hemitypic or degradational Oötocoids. Under tribes, families, and genera, the number of grades may be large.

Degradational subdivisions are strictly *hypotypic*, or *below* the typical range.

Typical subdivisions, or those above the degradational, are not, in all cases, *true* typical, as well exemplified by the orders of Fishes: the Teliosts alone being true typical, and the Ganoids and Selachians, called *hemitypic* above, being properly *hypertypic*, or *above* the typical range. Another example of this is afforded by the subdivisions of Megasthenes. Carnivores and Herbivores are different grades of the *true typical*, the former the more perfect, or *eutypic*; while the Quadrumanes or Monkeys are *hypertypic*, being an *intermediate* type between the typical Megasthenes and Man; and the Mutilates (Cetaceans, &c.) are *hypotypic*. Among the Microsthenes, the Chiropters or Bats are *hypertypic*, the Insectivores and Rodents *true typical* of two grades, and the Edentates *hypotypic*.

Among the subdivisions of Mammals there are *three* grades of true typical; and of them man is *archetypic*, as he has been styled, being the *one perfect* type.

Degradational forms may be classed under three heads, as follows:—

1. *Degenerative*; in which the forms are thoroughly animal in type. The methods of decephalization which lead most commonly to degenerative forms are the analytic, multiplicative, elliptic, and defunctionative.

2. *Hemiphytoid*; when, without an *internal* radiate structure, the species are (a) attached to a support, like plants (see *defunctionative* method, p. 84); (b), budding (*gemmative*, p. 91); (c), radiate externally (*phytozoic*, case a, p. 89).

The externally radiate structure is a lower grade of hemiphytoid degradation than either, being attached, or gemmate.

3. *Phytoid* (from *φυρον*, a plant); when the structural arrange-

ments are internally, as well as externally, radiate (Phytozoic, case b).

As Radiates have no limbs, and but imperfect senses, the higher grades among them are manifested most prominently in the conditions of the nutritive system. Some of them (the Echinoderms) are superior, as animals, to the lower *hemiphytoid* species, such as the Bryozoans.

2. *Further exemplifications of the preceding methods of Cephalization.*—In order to give greater clearness to the explanations which have been made on the preceding pages, the application of the terms expressing the methods of cephalization to grades of species may here be further illustrated.

In the class of Crustaceans, the distinction between the 1st and 2d orders, or Decapods and Tetradeapods, depends on case a under the *retroferent* method—a transfer of members from the cephalic to the locomotive series. In connection with it, there is also an exhibition, to some extent, of the *analytic* method, more of the segments of the body in the latter being free, and all, more regular or normal in form.

Under Decapods, the difference between the 1st and 2d tribes, the Brachyural and Macrural, depends mainly on the *amplificative* method—there being in the latter, by an abrupt transition, greater length and laxness before and behind. Under the *analytic*, also, the lengthened abdomen in the Macruran has its normal number of segments and members.

Among the subdivisions of *Macrurans*, the *retroferent* method appears prominently in the transfer of force from the *first* pair of legs to the *second*, and, among the lower genera, to the *third* pair (see p. 83); the *amplificative*, in the length of antennæ in some families, and in the length of abdomen as compared with that of the cephalothorax in others; the *elliptic*, in the absence of posterior cephalothoracic members, and also the obsolescence of the abdominal members in many Schizopods or degradational Macrurans; the *pervertive*, in the outer maxillipeds taking the form and functions of feet, as in many inferior Macrurans.

Under *Tetradeapods*, the difference between the 1st and 2d tribes, or Isopods and Amphipods, depends on the very same methods as that between the 1st and 2d under the Decapods; that is, on the *amplificative*, as shown in the greater length of cephalothorax and the elongated abdomen, and on the *analytic*, the lengthened abdomen having its normal segments and approximately normal members.

Under the Amphipods, the *amplificative* method is variously illustrated; the *elliptic* in the obsolescent abdomen of the Caprellids, as well as in the absence or obsolescence in many species of two pairs of thoracic legs.

Again, in the class of Insecteans, the distinction between the 1st and 2d orders, or Insects and Spiders, depends on *case* (a) under the *retroferent* method (p. 83); and, in connection, there is an exhibition of an incipient stage of the *analytic*, the head and thorax in Spiders constituting a single mass (p. 86).

Under Insects, the difference between the two highest divisions, *Prosthenics* and *Metasthenics*, depends on *case* (b) under the *retroferent* method, or a transfer of the flying function mainly or wholly to the posterior pair of wings. And the third is a degradational group, in which, by the *amplificative*, *analytic*, and *elliptic* methods, the species (*Lepismæ*, &c.) are wingless and larve-like.

Among Herbivores, the Elephant shows superiority (1) in having, as in Carnivores, the teeth (its tusks) for defensive weapons; (2) in having, as in Carnivores, the power of prehension, a quality, however, transferred from the teeth to one of the organs of sense, the nose; this organ of prehension also aids in defence; (3) in having the normal number of toes; (4) in having pectoral mammæ, as in the highest *Megasthenes* or *Quadrumanes*, the highest *Microsthenes* or *Bats*, and also in *Man*. The great size is not a mark of overgrowth and inferiority, for the animal is neither stupid nor sluggish. The Ruminants are inferior to the Elephant in having, not an *inferior* organ of sense, but the forehead, or typically the most important part of the head, perverted to use for self-defence; and also in other ways. Among Ruminants, the Stag or Elk-type shows superiority to the Ox-type, in (1) its more compact and smaller head; (2) its less magnitude *posteriorly*; (3) its limbs adapted to fleet motion; (4) its fore-limbs adapted for climbing and clinging, giving them a special *prosthenic* character and great superiority to those of the Ox. The Horse-type shows inferiority to the Elephant-type, in (1) its long head and neck (*amplificate*); (2) its one-hoofed foot; (3) its being *metasthenic*, the hind legs serving as the principal organs of defence; and also in the characters mentioned above.

The discussion of the subject of classification farther on, will be found to be a continued exemplification of the laws of cephalization, and we refer to it for additional elucidation.

3. *The forms, resulting from the expression of the same law of cephalization in diverse groups, often similar; and hence come some of the analogies between groups, or their osculations.*—It is apparent that the grades of cephalization may have expression in any division of the animal kingdom, and that hence may come *parallel* results as to form. For example, there may be cases of *amplificative* decephalization—or of long-bodied or long-legged species—in the different orders or tribes of Insects; and, when so, the species, in these different groups thus characterized, will be, in a sense, representatives of one another, and the groups will “*osculate*”

at such points. One example is that of Orthopters and Neuropters through the Mantids in the former, and the Mantispids in the latter; also, that of Dipters and Neuropters, through the slender Tipulids of the former. The same may be exemplified among the orders of Birds. The degradational feature, for example, of webbed feet, or that of defective wings, may characterise the inferior species of different subdivisions, and so produce osculant groups; so may the *amplificative* feature of great length of limb and neck, the Herons among the Altrices, thus representing the Grallatores among the Præcoces.

The osculations or close approximations of classes, orders, tribes, &c., are thus often connected with like expressions of the methods of cephalization.

4. *Forms resulting from high and low cephalization sometimes similar.*—High and low cephalization often lead to similar forms, the former through cephalic concentration, the latter through cephalic and general feebleness; just as a thing may be small, when the material is condensed or concentrated, and equally small when dilute and there is little of it. Thus the Crab has a very small memberless abdomen, from a contracting of the sphere of growth through concentrative cephalization; on the other hand, the Schizopod has a memberless abdomen, through a limitation of the sphere of growth resulting from mere feebleness in the life-system. The abbreviated memberless abdomen of the Caprellid and the obsolescent spine-like abdomen of the Limulus are other examples among Crustaceans of this *elliptic* decephalization. The Butterflies have very large wings through the amplificative method; but some inferior nocturnal species have the wings narrow, through inferiority of grade, on the above principle, and not properly through concentration and elevation.

There is, in general, no danger of confounding the two cases, because the accompaniments in the structure of the superior species, as well as those of the inferior, commonly indicate their true relations, at once, to the mind that is well versed in the department of zoology to which the species belong; but there are many cases in which it is not safe to make a hasty decision.

5. *Uniformity of shape and size in any group greater among the higher typical species than among the lower typical or degradational species.*—On the higher typical level in any class, order, tribe, &c., the type is represented generally in its greatest number of species, and always under the least extravagance of form and size. Thus, Insects, the higher typical division of Insecteans, are vastly more numerous in species, and less diversified in size, form, and structure, than Crustaceans or Worms. And, under Insects,

the Hymenoptera have little variety of form of body, and form or size of wings, compared with the Neuroptera, Lepidoptera, Homoptera, and even the Coleoptera; and the Coleoptera, little compared with the Orthoptera. The fantastic shapes, in all cases, occur in the inferior typical or the degradational groups. In these, cephalization is of low grade, and as a consequence of this relaxing of the system, or its inferior concentration, the forms run off into varied extravagances.

6. *Classification hereby placed on a dynamical or sthenic basis.*—The laws of cephalization, as is apparent from the explanations which have been made, are based upon the idea that an animal is centralized force; and that the degree of concentration of this force may be exhibited in the structure; that, consequently, the various grades of species or groups become apparent, to some extent, through size and form, and their determination is thus, in part, a matter of simple measurement. Dimensions or spatial conditions have a relation to force in the animal kingdom as well as in that of the celestial spheres.

Rank or grade are thus brought to the rule and plummet, and classification, thereby, has a dynamical basis. The distinctions between groups have a dynamical or sthenic character, and all subdivisions in classification, when thoroughly understood, will have recognised sthenic relations.

It must, however, be kept in mind that the element of *size*, when used in the application of the principle, or as a mark of superiority, is not *absolute* size. For it is one of the laws of life that vegetative growth may enlarge a weak life-system to gigantic dimensions. Thus, the life-system of an Entomostracan takes great magnitude in a *Limulus*; of a *Tretradecapod*, in a female *Bopyrus*; of an *Edéntate*, in a *Megathere*; of a *Mutilate*, in a *Whale*. The body of a *Crab* has fifty times the dimensions of that of an *Insect*; and its head probably 100 times that of the head of an *Insect*, although an *Insect* is the superior species.

Neither is mere muscular strength an indication of grade; for there is force used in sustaining the structure which is greater the higher the organism; and superior to this, there is sensorial and other cephalic force. Were we to base our comparison between the grade of life-system in a *Crab* and that of a *Bee* on the ground of muscular strength, we should go far astray; and still wider from the mark, were we to rely on the relative sizes of the cephalic nervous masses; for this nervous mass in a common *Crab* (*Maia squinado* of European seas) has twenty-five to thirty times the bulk of that in a *Bee*. Man yields in size and muscular strength not only to the higher *Megasthenes*, but to the *Whales*, or lowest; and the brain in the *Elephant* and the *Whale*

outweighs his. The Megathere, although much more powerful than a Rodent, has not, on this account, as his structure and habits show, any claims to a place above the lowest of Microsthenes.

The terms *Megasthenes* and *Microsthenes* are not to be understood as signifying large Mammals and small Mammals, but Mammals of *strong life-system* and *weak life-system*. Comparing the typical species of *Megasthenes** with those of *Microsthenes*, there is some correspondence between average size of structure and strength of life-system. But a comparison of the typical of the former with the degradational of the latter leads to very false results.

An approximation to the right ratio is obtained from a comparison of the degradational species of each ; but this is of no importance in its bearing on the question, since vegetative growth is apt to give the greatest proportional enlargement to the *lowest* species.

These facts teach that relative size of body, or of brain, is no necessary test of relative rank. The ratio, in *bulk*, of 1 : 3 between the brain of an average Man and that of a gorilla tells nothing of the actual difference of life-system, or of brain-power. The relative *lineal* dimensions of *Microsthenes* and *Megasthenes* has been estimated at 1 : 4, which gives, for the relative *bulk*, 1 : 64. If this be the typical ratio between the life systems of the highest *Microsthenes* and highest *Megasthenes*, surely that between the highest *Megasthenes* and normal man—he constituting a *distinct order*—must be at least as great.

The same ratio of 1 : 4, as shown by the writer, is that for the mean size, lineally, of Tetracephalopods and Decapods, under Crustaceans. In two cases, then, consecutive orders differ by a like ratio, or approximately so, in dimensions. As has been remarked, deductions from mere size may be very erroneous ; yet there is no reason, in either of the above cases, to suppose the ratio of life-systems less than that thus indicated. May not, therefore, some similar ratio exist between other analogous consecutive orders, where size does not manifest it,—as, for example, between Spiders and Insects ? And is not the ratio a much greater one between the highest of Insecteans and highest of Crustaceans, since these subdivisions of Articulates are not orders but *classes* ? Important results may flow from following out the idea here touched upon.

* These orders of Mammals, make parallel series—the Chiropters or Bats of the *Microsthenes* representing the Quadrumanes of the *Megasthenes*, the Insectivores representing the Carnivores, the Rodents the Herbivores, and the Edentates the Mutilates.

After the preceding explanations, I proceed to exhibit some of the relations of the higher groups in zoological classification, as they appear in the light of this subject of cephalization.

(*To be continued in next number.*)

Synopsis of Canadian Ferns and Filicoid Plants. By
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Scotia.

The following Synopsis embraces a concise statement of what is known respecting Canadian ferns and filicoid plants. Imperfect as it is, I trust that it will prove useful to botanists and fern fanciers, and stimulate to renewed diligence in investigation. The whole number of species enumerated is 74. Of these 11 are doubtful. Farther investigation will probably lead to the elimination of several of the doubtful species, which are retained for the present with a view to promote inquiry; but a few additional species, as yet unknown within the boundaries of Canada, may be discovered. The above number (74) may be regarded, then, as a fair estimate—perhaps slightly in excess—of the actual number of ferns and filicoid plants existing in Canada. The number certainly known to exist, after deducting the species of doubtful occurrence, is 63.

• The number of species described in Professor Asa Gray's exhaustive "Manual," as actually known to inhabit the northern United States, that is to say, the country lying to the south of the St Lawrence River and great lakes, stretching to and including Virginia and Kentucky in the south, and extending westward to the Mississippi River, is 75. This number does not include any doubtful species.

The number described in Dr Chapman's "Flora," as inhabiting the Southern States, that is, all the states south of Virginia and Kentucky and east of the Mississippi, is 69.*

* Mr D. C. Eaton, M.A., is author of that portion of Dr Chapman's "Flora" which relates to the ferns.

From these statements it will be seen that we have our due share of ferns in Canada.

The whole number of ferns in all the American States, and the British North American Provinces, is estimated, in a recent letter from Mr Eaton, as probably over 100.

In the British Islands there are about 60 ferns and filicoid plants. In islands of warmer regions the number is greatly increased. Thus Mr Eaton's Enumeration of the true ferns collected by Wright, Scott, and Hayes, in Cuba, embraces 357 species. The proportions of ferns to phanerogamous plants in the floras of different countries are thus indicated by Professor Balfour, in the "Class Book of Botany," page 998, § 1604:—"In the low plains of the great continents within the tropics ferns are to phanerogamous plants as 1 to 20; on the mountainous parts of the great continents, in the same latitudes as 1 to 8 or 1 to 6; in Congo as 1 to 27; in New Holland as 1 to 26. In small islands, dispersed over a wide ocean, the proportion of ferns increases; thus, while in Jamaica the proportion is 1 to 8, in Otaheite it is 1 to 4, and in St Helena and Ascension nearly 1 to 2. In the temperate Zone, Humboldt gives the proportion of ferns to phanerogamous plants as 1 to 70. In North America the proportion is 1 to 35; in France 1 to 58; in Germany 1 to 52; in the dry parts of South Italy as 1 to 74; and in Greece 1 to 84. In colder regions the proportion increases; that is to say, ferns decrease more slowly in number than phanerogamous plants. Thus, in Lapland, the proportion is 1 to 25; in Iceland 1 to 18; and in Greenland 1 to 12. The proportion is least in the middle temperate zone, and it increases both towards the equator and towards the poles; at the same time it must be remarked, that ferns reach their absolute maximum in the torrid zone, and their absolute minimum in the arctic zone."

Canada consists of a belt of land, lying to the north of the St Lawrence River and the great lakes. By these it is separated, along nearly the whole extent of its south-eastern and western boundaries, from the northern United States, which thus enclose Canada on two sides. A striking resemblance, amounting almost to identity, is therefore to be looked for in the floras of the two countries. Yet species appear in each that are absent in the other.

104 *Synopsis of Canadian Ferns and Filicoid Plants.*

The species of ferns and filicoid plants which are certainly Canadian, number	63
Of these there inhabit the Northern States,	58
Do. do. Southern States,	38
Do. do. Europe,	36

The following table is designed to show some of the geographical relations of our Canadian ferns. The first column (I.) refers exclusively to the occurrence of the species within the Canadian boundary. The plus sign (+) indicates that the species is general, or at least does not show any decided tendency towards the extreme eastern or western, or northern or southern parts of the province. The letters N, S, E, W, &c., variously combined, indicate that the species is so limited to the corresponding northern, southern, eastern, or western parts of the province, or at least has a well-defined tendency to such limitation. The mark of interrogation (?) signifies doubt as to the occurrence of the species. The second column (II.) shows what Canadian species occur also in the Northern States, that is the region embraced by A. Gray's Manual; and the third column (III.) those that extend down south into Chapman's territory. The fourth column (IV.) shows the occurrence of our species in Europe; C in this column indicating Continental Europe, and B the British Islands. The fifth or last column (V.) shows the species that extend northwards into the Arctic circle—35 in all, of which, however, only 14, or perhaps 15, are known to be arctic in America. Am, As, Eu, and G indicate respectively Arctic America, Arctic Asia, Arctic Europe, and Arctic Greenland. The information contained in the last column has been chiefly derived from Dr Hooker's able Memoir in the Linnean Transactions (vol. xxiii. p. 251).

Hitherto no attention whatever has been paid, in Canada, to the study of those remarkable variations in form to which the species of ferns are so peculiarly liable. In Britain, the study of varieties has now been pursued by botanists so fully as to show that the phenomena which they present have a most important bearing upon many physiological and taxological questions of the greatest scientific interest. The varieties are studied in a systematic manner, and the laws of variation have been to a certain extent ascertained. And as the astronomer can point out the existence of a

planet before it has been seen, and the chemist can construct formulæ for organic compounds—members of homologous series—in anticipation of their actual discovery, so, in like manner the pteridologist now studies the variations of species by a comparative system, which enables him to look for equivalent forms in the corresponding species of different groups. Studies so pursued are calculated to evolve more accurate and definite notions as to the real nature of species, and the laws of divergence in form of which they are capable. I would therefore earnestly invite Canadian botanists to a more careful study of the *varieties* of the Canadian ferns, after the manner of Moore and other European leaders in this comparatively new path. The elasticity, or proneness to variation, of the species in certain groups of animals and plants has been somewhat rashly used to account for the origin of species, by what is called the process of variation. It seems to tell all the other way. Innumerable as are the grotesque variations of ferns, in forkings, and frillings, and tassellings, and abnormal veinings, &c. (see the figures in Moore's works), we do not know of a single species in which *such* peculiarities have become permanent or general, that is *specific*, so that the species can be traced back to such an origin; surely something of the kind would have happened had all species originated by a process of variation.

*Tabular View of the Distribution of Canadian Ferns and Allied Plants over certain parts of the Northern Hemisphere.**

NAME.	I.	II.	III.	IV.	V.
	Canada.	Northern States.	Southern States.	Europe.	Arctic Circle.
POLYPODIACEÆ.					
1. Polypodium vulgare, . . .	+	+	+	C.B.	Eu.
2. P. hexagonopterum, . . .	+	+	+	...	...
3. P. Phlegopteris, ...	+	+	...	C.B.	Eu. G.
4. P. Dryopteris, ...	+	+	...	C.B.	Eu. Am. G.

* In the above Table, the doubtful species are included; but all reference to varieties is omitted.

NAME.	I.	II.	III.	IV.	V.
	Canada.	Northern States.	Southern States.	Europe.	Arctic Circle.
5. <i>P. Robertianum</i> , ...	+	+	...	C.B.	...
6. <i>Adiantum pedatum</i> , ...	+	+	+	...	...
7. <i>Pteris aquilina</i> , ...	+	+	+	C.B.	Eu.
8. <i>Pellaea atropurpurea</i> , ...	S.	+	+	...	...
9. <i>Allosorus Stelleri</i> , ...	+	+	...	...	...
10. <i>Cryptogramma acrostichoides</i> , ...	W.W.	...	...	?	Am.
11. <i>Struthiopteris germanica</i> , ...	+	+	...	C.	Eu.
12. <i>Onoclea sensibilis</i> , ...	+	+	+	...	...
13. <i>Asplenium Trichomanes</i> , ...	+	+	+	C.B.	...
14. <i>A. viride</i> , ...	N.E.	...	...	C.B.	Eu. G.
15. <i>A. angustifolium</i> , ...	S.W.	+	+	...	...
16. <i>A. ebeneum</i> , ...	+	+	+	...	...
17. <i>A. marinum</i> , ...	E. ?	...	...	C.B.	...
18. <i>A. thelypteroides</i> , ...	+	+	+	...	...
19. <i>A. montanum</i> , ...	?	+	+	...	Eu.
20. <i>A. Ruta-muraria</i> , ...	?	+	+	C.B.	Eu.
21. <i>Athyrium Filix-femina</i> , ...	+	+	+	C.B.	Eu.
22. <i>Woodwardia virginica</i> , ...	S.W.	+	+	...	...
23. <i>Scolopendrium vulgare</i> , ...	W.W.	+	...	C.B.	...
24. <i>Camptosorus rhizophyllus</i> , ...	W.	+	+	...	...
25. <i>Lastrea dilatata</i> , ...	+	+	+	C.B.	Eu. Am.
26. <i>L. marginalis</i> , ...	+	+	+	...	...
27. <i>L. Filix-mas</i> , ...	??	...	...	C.B.	Eu. G.
28. <i>L. cristata</i> , ...	+	+	...	C.B.	...
29. <i>L. Goldieana</i> , ...	W.	+	...	...	...
30. <i>L. fragrans</i> , ...	N.W. ?	+	...	...	As. Am. G.
31. <i>L. Thelypteris</i> , ...	+	+	+	C.B.	...
32. <i>L. Nov-Eboracensis</i> , ...	+	+	+	...	...
33. <i>Polystichum angulare</i> , ...	+	+	...	C.B.	Eu.
34. <i>P. Lonchitis</i> , ...	N.W.	+	...	C.B.	Eu. Am. G.
35. <i>P. acrostichoides</i> , ...	+	+	+	...	...
36. <i>Cystopteris fragilis</i> , ...	+	+	+	C.B.	Eu. Am. G.
37. <i>C. bulbifera</i> , ...	+	+	+	...	...
38. <i>Dennstaedtia punctilobula</i> , ...	+	+	+	...	...
39. <i>Woodsia ilvensis</i> , ...	+	+	+	C.B.	{ Eu. As. Am. G.
40. <i>W. alpina</i> , ...	+	...	...	C.B.	Eu. G.
41. <i>W. glabella</i> , ...	+	+	...	...	Am.
42. <i>W. obtusa</i> , ...	?	+	+	...	...
43. <i>Osmunda regalis</i> , ...	+	+	+	C.B.	...
44. <i>O. cinnamomea</i> , ...	+	+	+	...	...
45. <i>O. Claytoniana</i> , ...	+	+	+	...	...
46. <i>Schizæa pusilla</i> , ...	?	+	...	...	...

NAME.	I.	II.	III.	IV.	V.
	Canada.	Northern States.	Southern States.	Europe.	Arctic Circle.
OPHIOGLOSSACEÆ.					
47. Botrychium virginicum, . .	+	+	+	...	Eu. G.
48. B. lunarioides, ...	+	+	+	?	...
49. B. Lunaria, ...	N.	...	...	C.B.	Eu. G.
50. Ophioglossum vulgatum, .	?	+	+	C.B.	Eu.
LYCOPODIACEÆ.					
51. Plananthus Selago, ...	N. ?	+	+	C.B.	{ Eu. As. Am. G.
52. P. lucidulus, ...	+	+	+	C.	...
53. P. alopecuroides, ...	??	+	+	...	...
54. P. inundatus, ...	+	+	+	C.B.	...
55. Lycopodium clavatum, . .	+	+	+	C.B.	Eu. G.
56. L. annotinum, ...	+	+	+	C.B.	Eu. Am. G.
57. L. dendroideum, ...	+	+	+	...	...
58. L. complanatum, ...	+	+	+	C.	Eu. As.
59. Selaginella spinulosa, . .	N.E.	+	+	C.B.	Eu. G.
60. Stachygynandrum rupestre, .	+	+	+	...	...
61. Diplostachyum apodum, . .	+	+	+	...	...
MARSILEACEÆ.					
62. Azolla caroliniana, ...	S.	+	+	...	...
63. Salvinia natans, ...	??	...	+	C.	...
64. Isoetes lacustris, ...	+	+	+	C.B.	Eu. G.
EQUISETACEÆ.					
65. Equisetum sylvaticum, . .	+	+	...	C.B.	Eu. Am. G.
66. E. umbrosum, ...	+	+	...	C.B.	Eu.
67. E. arvense, ...	+	+	...	C.B.	{ Eu. As. Am. G.
68. E. Telmateja, ...	W.	+	...	C.B.	...
69. E. limosum, ...	+	+	...	C.B.	Eu.
70. E. hyemale, ...	+	+	...	C.B.	Eu.
71. E. robustum, ...	+	+	...	...	...
72. E. variegatum, ...	N.E.	+	...	C.B.	{ Eu. Am.? G.
73. E. scirpoides, ...	+	+	...	C.	{ Eu. As. Am. G.
74. E. palustre, ...	N.	...	...	C.B.	Eu. Am.

Nat. Ord. POLYPODIACEÆ.

POLYPODIUM.

P. vulgare, Linn.—Fronde linear-oblong or somewhat lanceolate, more or less acuminate, deeply pinnatifid, in some forms almost pinnate; lobes (or pinnæ) linear-oblong, obtuse, often acute, rarely acuminate, entire or crenate or serrate; sori large; very variable as regards outline of the frond, form, &c., of the lobes, and serrature. *P. vulgare*, Linn., A. Gray, Moore, &c. *P. virginianum* of English gardens. *P. vulgare*, var. *americanum*, Hook., Torrey Fl. N. Y., ii. 480.—On rocks in the woods, not rare around the city of Kingston; abundant on the rocky banks of the St Lawrence, in Pittsburg; in the woods at Collins's Bay; and on Judge Malloch's farm, a mile west from Brockville; Gananoque lakes and rivers; Farmersville; Newboro-on-the-Rideau; Toronto; on the great boulder of the Trent Valley, near Trenton; on rocks west from Brockville, outcrop of Potsdam Sandstone at Oxford, and Hull mountains near Chelsea, C.E., B. Billings, jr.; near Gatineau Mills, D. McGillivray, M.D.; Mount Johnson, C.E., and Niagara River, P. W. MacLagan, M.D.; Brighton, in the crevice of a rock in a field, and abundant on rocky banks, right bank of the Moira, above Belleville, J. Macoun; Ramsay, Rev. J. K. M'Morine, M.A.; north-west from Granite Point, Lake Superior, R. Bell, jr.; mountain top, near Mr Brydges's house, Hamilton, C.W., Judge Logie; River Rouge and lower end of Gut Lake, W. S. M. D'Urban; Cape Haldimand, Gaspé, John Bell, B.A.; Red River Settlement, Governor M'Tavish; Pied du cap Tourmente, M. L'Abbé Provancher; L'Orignal and Grenville, C.E., J. Bell, B.A. The habitats above cited show that although this fern is not so common in Canada as in Britain, it is nevertheless widely distributed. It is common in New York State, according to Professor Torrey; and in the Northern States generally, according to Professor Asa Gray; rarer in the South, according to Dr Chapman.

P. hexagonopterum, Mich.—Fronde triangular in outline, acuminate, pinnate, hairy throughout; pinnæ broadly lanceolate, pinnatifid; lowest pair of pinnæ larger than the others, not deflexed; lobes of the pinnæ linear-oblong or lanceolate, strongly toothed, or almost pinnatifid. The decurrent pinnæ have a tendency to form conspicuous irregular angled wings along the rachis. Stipe not scaly except at the base. Rhizome long, slender, ramifying. Whole plant much larger than *P. Phegopteris*, and quite a different species. *P. hexagonopterum*, Michx., A. Gray, &c. The figure in Lowe's Ferns, vol. i. p. 143, tab. 49, is a little too much like *Phegopteris*. *P. Phegopteris* *γ. majus*, Hook. Fl. Bor. Amer., ii. p. 258. Hooker's *β. intermedia* of *Phegopteris* is *connectile*, Willd., which A. Gray refers to *P. Phegopteris*, L. *Phegopteris hexagonoptera*, J. Sm. Cat., p. 17.—Canada, Goldie in Hook. Fl. B. Amer.; Chippawa, C. W., P. W. MacLagan, M.D.; Mirwin's Woods, near Prescott, rare, B. Billings, jr.; near Westminster Pond, London, W. Saunders. Not by any means so general in Canada as in New York State, where, Professor Torrey states, it is common.

P. Phegopteris, Linn.—Fronde acutely triangular in outline, acuminate, pinnate; the pinnæ linear-lanceolate, pinnatifid, lowest pair deflexed; lobes of the pinnæ oblong, scythe-shaped, obtuse, approximate, entire; rachis hairy and minutely scaly to the apex of the frond, as well as the mid-ribs of the pinnæ. *P. Phegopteris*, Linn., A. Gray, Moore, &c. *Phegopteris vulgaris*, J. Sm. *P. connectile*, Michx., Pursh Fl. Am. Sept., ed. 2, vol. ii. p. 659.—Canada, Hooker; Black Lead Falls and De Salaberry, west line, W. S. M. D'Urban; Ramsay, Rev. J. K. M'Morine, M.A.; Nicolet, P. W. MacLagan, M.D.; Prescott, damp woods, not common, Osgood Station of the Ottawa and Prescott Railway, also Gloucester, near Ottawa, growing on the side of a ravine, and Chelsea, C.E.; B. Billings, Jr.; opposite Grand Island, Lake Superior, R. Bell, jr.; L'Original and Harrington, J. Bell, B.A.

P. Dryopteris, Linn.—Fronde thin, light-green, pentangular in outline, consisting of three divaricate triangular subdivisions, each of which is pinnate, with its pinnæ more or less deeply pinnatifid; pinnules oblong, obtuse, nearly entire; stipe slender and weak, not glandulose. *P. Dryopteris*, Linn., A. Gray, Moore, &c. *Phegopteris Dryopteris*, J. Sm.—Abundant in the woods around Kingston; Ramsay, Rev. J. K. M'Morine, M.A.; very common in woods about Prescott, B. Billings, jr.; Montreal and Nicolet Rivers, C.E., P. W. MacLagan, M.D.; Belleville, common in the woods, J. Macoun; opposite Grand Island, Lake Superior, R. Bell, jr.; River Rouge, Round Lake, Montreal, De Salaberry, west line, and Black Lead Falls, W. S. M. D'Urban; Newfoundland, Labrador; Somerset and St Joachim, M. L'Abbé Provancher; L'Original, J. Bell, B.A.

Var. β. erectum.—Fronde erect, rigid, with a very stout and very long glabrous stipe (18 inches long); beech woods at Collins's Bay, near Kingston, with the normal form. This variety resembles *P. Robertsonum* in general aspect, but is not at all glandulose.

P. Robertsonum, Hoffman.—A stouter plant than *P. Dryopteris*; fronds more rigid and erect; rachis, &c., closely beset with minute-stalked glands. *P. Robertsonum*, Hoffman, Moore, &c. *P. calcareum*, Sm. *P. Dryopteris*, var. *calcareum*, A. Gray.—Canada, Moore and other authors; United States, Gray and others. This species is commonly spoken and written of as a Canadian Fern. Not having had an opportunity of seeing Canadian specimens, I cannot cite special habitats. The minutely glandulose rachis serves at once to distinguish it.

ADIANTUM.

A. pedatum, Linn.—Stipe black and shining, erect, forked at top, the forks secundly branched, the branches bearing oblique triangular-oblong pinnules. *A. pedatum*, Linn., A. Gray, &c., Lowe's Ferns, vol. iii. pl. 14. Abundant in vegetable soil in the woods around Kingston; woods around the iron mines at Newboro-on-the-Rideau; Farmersville; Toronto; Montreal, Chippawa, Wolfe Island, and Malden, P. W. MacLagan, M.D.; Belleville, in rich woods, abundant, J. Macoun; Ramsay, Rev. J. K. M'Morine, M.A.; Ke-we-naw Point, R. Bell, jr.; at the Sulphur Spring, and common everywhere about Hamilton, Judge

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Logie; Lake Huron, Hook. Fl. B. A.; De Salaberry, west line, W. S. M. D'Urban; on the Gatineau, near Gilmour's rafting ground, D. McGillivray, M.D.; London, W. Saunders; St Joachim and Isle St Paul, Montreal, M. L'Abbé Provancher; West Hawkesbury and Grenville, C.E., J. Bell, B.A. Apparently common everywhere in Upper Canada. I cannot speak so definitely of the Lower Province. This is one of our finest Canadian ferns; "the most graceful and delicate of North American ferns," says Torrey. It is easily cultivated. Fine as it is in the Canadian woods, I have specimens even more handsome from Schooley's Mountains (A. O. Brodie, Ceylon Civil Service); their fan-like fronds spread out in a semicircle, with a radius of $2\frac{1}{2}$ feet. It is not a variable species in Canada. T. Moore, in "Index Filicum," gives its distribution as N. and N.W. America, California to Sitka, North India, Sikkim, Nepal, Gurwhal, Simla, Kumaon, Japan. There is a var. β . *aleuticum*, Rupr., in the Aleutian Islands.

PTERIS.

Pt. aquilina, Linn.—Stipe stout, 1 to 3 feet high, frond ternate, branches bipinnate, pinnules oblong lanceolate, sori continuous under their recurved margins. *Pt. aquilina*, Linn., A. Gray, Moore, &c.—Abundant on Dr Yates's farm in Pittsburg, and elsewhere about Kingston; Water-down Road, Hamilton, common, Judge Logie; Chippawa and Malden, C.W., P. W. MacLagan, M.D.; Ramsay, Rev. J. K. M'Morine, M.A., Prescott, common, B. Billings, jr.; Belleville, very common on barren ridges, J. Macoun; Grand Island, Lake Superior, R. Bell, jr.; Red Lake River, also between Wild Rice and Red Lake Rivers, and Otter Tail Lake and River, between Snake Hill River and Pembina, &c., J. C. Schultz, M.D.; Black Lead Falls, and Portage to Bark Lake, W. S. M. D'Urban; Gatineau Mills, very common, D. McGillivray, M.D.; Lakefield, North Douro, Mrs Traill; New Brunswick, Hook. Fl. Bor. Amer.; L'Orignal, J. Bell, B.A.; London, W. Saunders.

α . *vera*.—Pinnules pinnatifid (the normal or typical form of Moore), Dr Yates's farm, Kingston.

β . *integerrima*.—Pinnules entire (a sub-variety), common in Canada and westward. There are various other sub-varieties, differing in size, pubescence, &c.

γ . *decipiens*.—Frond bipinnate, thin and membranous, lanuginose, pinnules pinnatifidly toothed, or, in small forms, entire, barren; L'Anse à Cabielle, Gaspé, John Bell, B.A. This is a very remarkable fern, resembling a *Lastrea*, and in the absence of fructification, it is doubtfully referred to *Pteris aquilina*, yet the venation seems to indicate that it belongs to that species, which is remarkable for its puzzling forms. Being at a loss what to make of this fern, I sent it to Mr D. C. Eaton, M.A., who is justly looked up to by American botanists as our best authority on American ferns, and he likewise failed to recognise it. I hope some visitor to Gaspé will endeavour to obtain it in a fertile state, and thus relieve the doubt.*

* Since the above was written, I have had an opportunity of studying the forms and development of *Pteris aquilina*, and am quite satisfied that the doubtful plant is a state of that species, not old enough to be fertile.

[Var. *δ. caudata* appears occasionally in lists. I have as yet no satisfactory evidence of its occurrence in Canada proper. The nearest approach to it is a specimen from the Hudson's Bay territories, probably from the Red River District (Governor M'Tavish). In the South it is a very distinct form, of which there are beautiful specimens in Wright's Cuban Plants (No. 872), and is very close to the *Pteris esculenta* of Australia.]

PELLÆA.

P. atropurpurea, Link.—Stipe and rachis almost black, shining, 6 to 12 inches high, frond coriaceous, pinnate, divisions opposite, linear-oblong or somewhat oval. *Pteris atropurpurea*, Linn. *Platyloma atrop.*, J. Sm., Torr Fl. N. Y., ii. p. 488. *Allosorus atropurpureus*, A. Gray. *Pellæa atropurpurea*, Link., Fée, J. Sm. in Cat., Eaton.—Niagara River, at the Whirlpool, three miles below the Falls. This fern seems to retain its fronds all winter, for I have fertile specimens, in a fine state, collected at the Whirlpool at the end of February 1859 by A. O. Brodie. Dr P. W. MacLagan has also collected it there. It is not common anywhere on the American Continent so far as I can learn. Mr Lowe speaks of it as in cultivation in Britain, "an evergreen frame or greenhouse species, not sufficiently hardy to stand over winter's cold." There must be some other reason for want of success in its cultivation in Britain.

ALLOSORUS.

A. Stelleri, Ruprecht.—Fronds pale-green, thin and papery, 3 to 9 inches long, bipinnate or tripinnate, some of the smaller barren fronds scarcely more than pinnate; pinnæ five or six pairs; lobes of the barren frond, rounded, oval, veiny; of the fertile frond, much narrower, linear-lanceolate, firmer; sori at the tips of the forked veins along the margins, stipe red, whole plant glabrous. A beautiful and delicate fern, growing in the crevices of rocks, rare. *Allosorus Stelleri*, Ledeb. Fl. Rossica. *Allosorus gracilis*, Presl., A. Gray, Torrey Fl. N. Y. ii. p. 487. In a letter from Mr T. Moore (1857), he mentioned to me that he had learned from specimens from Dr Regel, St Petersburg, that "the North American *Allosorus gracilis* was the old *Pteris Stelleri* of Amman, so that it spreads from North America through Siberia to India, whence Dr Hooker has it." *Allosorus minutus*, Turcz. Pl. Exs. *Cheilanthes gracilis*, Klf. *Cryptogramma gracilis*, Torrey. *Pteris Stelleri*, Gmelin. *Pteris minuta*, Turcz. Cat. Pl. Baik. Dah. *Pt. gracilis*, Michaux.—Near Lakesfield, North Douro, C.W., on rocks, Mrs Traill; abundant in crevices of limestone rocks, on the rocky banks of the Moira, Belleville, Co. Hastings, J. Macoun; Lake of Three Mountains, W. S. M. D'Urban; Canada to the Saskatchewan, Hook. Fl. Bor. Am.; Dartmouth, Gaspé, John Bell, B.A. This is a northern species, and rare in the United States.

CRYPTOGRAMMA.

C. acrostichoides, R. Br.—"Remarkable for its sporangia extending far down on the oblique veins, so as to form linear lines of fruit." I

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have not seen the plant. It is referred by Sir William Hooker to *Allosorus crispus* (A. Gr. in Enum. of Dr Parry's Rky. Mtn. Plants). *Cryptogramma acrostichoides*, R. Br., Moore. *Allosorus acrostichoides*, A. Gr.—Ile Royale, Lake Superior. Placed in Dr Hooker's Table as a Canadian species that does not extend into the United States. It has recently been found on the Rocky Mountains. *Allosorus crispus* is general throughout Europe, and occurs at Sitka, in North-West America. Mr Moore observes that the Eastern (Indian) species, *A. Brunoniana*, is very doubtfully distinct from the European plant.

STRUTHIOPTERIS.

S. germanica var. *β pennsylvanica*.—Rhizome stout, erect; fronds tufted; sterile ones large pinnate, erect-spreading, deeply pinnatifid; the fertile ones erect, rigid, with revolute contracted divisions, wholly covered on the back by sporangia. A very graceful fern, well suited for cultivation in gardens. *Struthiopteris pennsylvanica*, Willd., Pursh, J. Sm. Cat. *S. germanica*, Hooker, Torrey Fl. N. Y. ii. p. 486, Gray. *Osmunda Struthiopteris*, Linn.; *Onoclea Struthiopteris*, Schkr.; *Onoclea nodulosa*, Schkr., according to Hooker. Torrey refers *O. nodulosa*, Michx., to *Woodwardia angustifolia*.—Frankville, Kitley; Longpoint; Lansdowne; Hardwood Creek; usually found along the margins of creeks, &c.; common in rich, wet woods near Prescott, and abundant around Ottawa, B. Billings, jr.; low rich grounds, Belleville, abundant along Cold Creek, J. Macoun; Re-we-naw Point, Lake Superior, in low ground, at times under water, R. Bell, jr.; Ramsay, Rev. J. K. M'Morine, M.A.; near Lakesfield, North Douro, Mrs Traill; field beyond Waterdown, Hamilton, Judge Logie; Osnabruck and Prescott Junction, Rev. E. M. Epstein; near Montreal, W. S. M. D'Urban; Assiniboine River, John C. Schultz, M.D.; Canada, to the Saskatchewan, Hook. Fl. Bor. A.; Pied du Tourmente, M. L'Abbé Provancher. This is the commonest plant in the Bedford Swamps; Gaspé and L'Orignal, J. Bell, B.A.; London, W. Saunders. Found in the western part of New York State, but rare according to Torrey.

ONOCLEA.

O. sensibilis, Linn.—Rhizome creeping; barren frond broad, leafy, deeply pinnatifid; fertile ones erect, spicate, contracted, doubly pinnate, with small revolute pinnules, enclosing the sporangia, not at all leafy. *Onoclea sensibilis*, Linn., A. Gr., J. Sm., &c. Lowe's Ferns, vol. vi. pl. 1.—In woods along the banks of the Little Cataraqui Creek in great abundance, and in moist swampy places in the woods in various other places about Kingston; west end of Loberough Lake; Becancour, M. L'Abbé Provancher; London, W. Saunders; common in marshy ground at Hamilton, Judge Logie; Lakesfield, North Douro, Mrs Traill; St John's, C. E., Niagara and Malden, P. W. MacLagan, M.D.; Belleville, in low marshy places, abundant, J. Macoun; Ramsay, Rev. J. K. M'Morine, M.A.; Amagos Creek, Lake Superior, R. Bell, jr.; Prescott, common, B. Billings, jr.; on the river shore, Gatineau Mills, D. M'Gillivray, M.D.; L'Anse au Cousin, Gaspé and L'Orignal, J. Bell; Nova Scotia. This curious fern has been cultivated in England since

1699; at Kew, since 1793. It is very variable as regards the outline and subdivision of the barren frond.

Var. *β. bipinnata*.—Fronds bipinnate; perhaps not a constant form. Fertile fronds of this variety originated the *O. obtusilobata*, Schkr. Pêche River, and near Cantley, Hull, D. M'Gillivray, M.D.

ASPLENIUM.

A. Trichomanes, Linn.—Frond small, narrow, linear, pinnate; pinnae roundish-oblong or oval, oblique, almost sessile, crenate: rachis blackish brown, shining, margined; sori distant from the midrib. *Asplenium Trichomanes*, Linn., Moore, Gray, &c., Lowe's Ferns, vol. v. pl. 22. *Asp. melanocaulon*, Willd., Pursh. Fl. Sept. Americ. ii. p. 666. *Asp. anceps*, Lowe.—Inhabits rocky river banks, &c., but is not common in Canada. On rocky banks, at Marble Rock, on the Gananoque River; Namainse, dry ground on the top of a mountain, R. Bell, jr.; rocky woodlands west from Brockville, rare, B. Billings, jr.; Montreal, Jones's Falls and Niagara, P. W. MacLagan, M.D.; Lake Medad, Hamilton, Judge Logie; Pittsburg, near Kingston, John Bell, B.A.; Pied du cap Tourmente, M. L'Abbé Provancher; near Belleville, J. Macoun.

β. delicatulum.—Frond narrower, pinnae much smaller, thinner, and wider apart than in the normal form. This is a sub-variety, passing by intermediate states into the typical plant, which is the common form of northern Europe. The variety is the prevalent form in Canada, but also occurs farther south in the United States, for I have specimens from Catskill (A. O. Brodie), and is not confined to the American continent, for Professor Caruel, the acute author of "Flora Italiana," sends specimens of a similar form from Florence. There is an *Asp. Trich.* var. *majus* in Cuba (according to Mr Eaton's Enumeration of Wright's Cuban ferns). *A. anceps* is a Madeiran form, not distinguishable, so far as I can see, from common European states of *A. Trichomanes*.

A. viride, Hudson.—Frond small, linear, pinnate; pinnae roundish-oblong or oval, more or less cuneate at base, slightly stalked, crenate or slightly lobed; rachis bright green; sori approximate to the midrib; in outline of frond and general aspect resembles the preceding species. *A. viride*, Hudson, Flora Anglica, 385; Sm., Bab., Moore, &c. *A. Trichomanes, β. ramosum*, Linn.—This beautiful alpine fern was found in Canada for the first time last summer, having been collected in considerable quantity at Gaspé, C.E., by John Bell, B.A., who formed one of a party of the Provincial Geological Survey. It was previously known to occur sparingly in N.W. America, at one spot on the Rocky Mountains, and in Greenland. Mr Bell's discovery of its occurrence in Gaspé is therefore extremely interesting in a geographical point of view. The Gaspé specimens although young, agree perfectly with the typical European form of *A. viride*, of which I have a full series of Scotch examples, as well as others collected in Norway by T. Anderson, M.D. In young specimens the pinnae are usually large, thin, and more cuneate and lobed than in the mature plant, in which they are roundish-ovate.

A. angustifolium, Michx.—Frond large (1 to 3 feet high), annual, lanceolate, pinnate; pinnae long, linear-lanceolate, acute; fertile fronds

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more contracted than the barren ones, "bearing sixty to eighty curved fruit dots on the upper branches of the pinnate forking veins," (Eaton). *A. angustifolium*, Michaux, A. Gray, Eaton, J. Smith, Lowe's Ferns, vol. v. pl. 24.—In Canada this fern appears to be confined to the extreme south-western point of the province;* Malden, P. W. MacLagan, M.D.; at the Oil Wells, township of Enniskillen, Lady Alexander Russell. For information of the latter station I am indebted to the kindness of Judge Logie of Hamilton. This fern appears to be still rare in cultivation among the fern fanciers of Europe. It was introduced to Britain in 1812 by Mr John Lyon of Dundee.

A. ebeneum, Aiton.—Frond erect, lance-linear, pinnate; pinnae numerous, lanceolate (the lower oblong), sessile, slightly auricled at base and finely serrate; rachis blackish-brown, shining. *Asplenium ebeneum*, Aiton, Hortus Kewensis, ed. 2, vol. v. p. 516, Gray, Eaton, J. Smith, Lowe's Ferns, vol. v. pl. 2. *A. polypodioides*, Schkr.—Rocky woods, Brockville, B. Billings, jr.; the only locality in Canada from which I have seen specimens.† Although so rare with us, this species appears to be not uncommon in the United States. Gray speaks of it as "rather common;" I have specimens from Schooley's Mountains, West Point, N. Y., Providence, Philadelphia, &c. Judging from Mr Eaton's indication in Chapman's Flora, it again seems to decrease in the south, so that its present headquarters are in the Northern States.

[*A. marinum*, Linn.—Frond broad and leafy, linear-lanceolate, tapered above, pinnate; pinnae ovate-oblong or linear, oblique, shortly stalked, rarely pinnatifid, the upper ones confluent, stipe brownish, rachis brown below, green and winged above, sori large, linear, oblique; grows on rocks. *Asplenium marinum*, Linn., Moore, J. Smith, &c. *A. latum*, Hort.—New Brunswick, E. N. Kendal, in Hook. Fl. Bor. Am. I cannot learn that this fern has been subsequently found in North America, and hope, therefore, that botanists will look for it on the rocky shores of New Brunswick. It usually grows out of the crevices of shore cliffs, and is very limited in its geographical range, growing, according to Moore, only in the western part of Europe, crossing from Spain to Tangiers on the African coast, and being again met with in Madeira, the Azores, and Canary Isles.]

A. thelypteroides, Michaux.—Fronds large oblong-ovate, pinnate; pinnae lanceolate, acuminate, from a broad sessile base, and deeply pinnatifid, the lobes oblong, minutely toothed. *Asplenium thelypteroides* Michaux, Pursh, Bigelow, Torrey, Beck, Darlington, Gray, Eaton. *Diplazium thelypteroides*, Presl, J. Sm.—In rich woods, De Salaberry, west line, W. S. M. D'Urban; Mirwin's woods, &c., Prescott, B. Billings, jr.; Beloeil Mountain, P. W. MacLagan, M.D.; moist woods near the Hop Garden, Belleville, rare, J. Macoun (a deeply serrated, leafy form); Ramsay, J. K. M'Morine, M.A.; St Joachim, M. L'Abbé Provancher; London, W. Saunders. Not a common fern in Canada; perhaps more plentiful in the United States. I have a fine series of specimens from Schooley's Mountains (A. O. Brodie), and others from Providence.

* Subsequently found in the Belleville district by Mr Macoun.

† Subsequently found near Belleville by Mr Macoun.

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β. serratum.—Lobes of the pinnæ ovate-oblong, approximate, strongly and incisely serrate. This may be regarded as a sub-variety.—Belleville, J. Macoun.

[*A. montanum*, Willd., which extends along the Alleghanies, has not yet been found in Canada, but may possibly occur. It grows on cliffs.]

[*A. Ruta-muraria*, Linn.—The wall-rue, a small species, which grows in the crevices of limestone cliffs in the Northern States, and is common on stone walls and old buildings in Britain, is to be looked for in Canada.]

ATHYRIUM.

A. Filix-femina, R. Br.—Fronde ample (1-3 feet long), broadly oblong-lanceolate, bipinnate; pinnæ also lanceolate; pinnules ovate-lanceolate or oblong, incisely toothed. Grows in large tufts, the fronds delicate, of a bright green hue. Lady Fern of the poets. *Athyrium Filix-femina*, R. Br., Spreng., Roth., Hook., Moore, &c. *Aspidium Filix-femina*, Swartz, Pursh, Beck. *Aspidium asplenoides*, Swartz, Willd., Pursh. *Asplenium Athyrium*, Schkr. *Asplenium Michauxii*, Spreng. *Asplenium Filix-femina*, A. Gray Man., p. 595. *Nephrodium asplenoides* and *Filix-femina*, Michx. *Asplenium angustum*, Willd., Pursh.—Common in the woods, as near Kingston, Toronto, Trenton, &c.; Péche River, Ottawa, Dr M'Gillivray; Temiscouata, Chip-pawa and Malden, P. W. MacLagan, M.D.; Belleville, moist woods, very common, several varieties, J. Macoun; Ramsay, Rev. J. K. M'Morine, M.A.; mouth of the Awaganissis Brook, Gulf of St Lawrence, C.E., and Schibwah River, Lake Superior, R. Bell, jr.; Cemetery grounds, Hamilton, and on Princes Island, Judge Logie; Hamilton's Farm and base of Silver Mt., W. S. M. D'Urban; Mountain Fall, H. B. T., Governor M'Tavish; Snake Hill River, John C. Schültz, M.D.; L'Anse à la Barbe, Gaspé and L'Orignal, John Bell, B.A.; St Tite, M. L'Abbé Provancher; London, W. Saunders.

β. angustum.—Fronde narrow, linear-lanceolate; pinnæ rather crowded; pinnules not pinnatifid, but incisely toothed, with recurved margins; sori short, curved (*Aspidium angustum*, Willd.?)—Farmersville; Delta; Belleville, J. Macoun.

γ. rheticum.—Fronde rather small, firm, narrowly lanceolate in outline; pinnæ more or less distant, and narrowly lanceolate; pinnules incisely toothed or deeply pinnatifid, linear, or more frequently lanceolate-acute, and acquiring a linear aspect from the reflection of the lobes, often crowded with confluent sori.—Dr Yates's farm, on the banks of the St Lawrence, near Kingston; near Montreal, Rev. E. M. Epstein, M.D.; near Lakefield, North Douro, Mrs Traill.

δ. rigidum.—Fronde small, rigid; pinnules approximate, connected at the base by a broad decurrent membrane, sori confined to the lower part of each pinnule.—Lakefield, North Douro, Mrs Traill.

There are other forms of this species, dependent in many cases, no doubt, upon situation; some with thin veiny fronds of great size, bearing few scattered sori. One form, very like the British var. *molle*, was gathered at Belleville by Mr Macoun. I know no fern more variable than this. Our Canadian forms require careful examination.

WOODWARDIA.

W. virginica, Willd.—Fronde pinnate; pinnae lanceolate, pinnatifid; sori arranged in line on either side of the midribs of pinnae and pinnules. *Woodwardia virginica*, Willd.; A. Gray Man. p. 593. (*Doodia*, R. Br.)—Millgrove Marsh, C.W., Judge Logie; sphagnous swamp near Heck's Mills, ten miles from Prescott, Augusta, C.W., B Billings, jr.; Pelham, C.W., P. W. MacLagan, M.D.; Belleville, J. Macoun.

(To be continued in next number.)

REVIEWS AND NOTICES OF BOOKS.

1. *Victoria Toto Cælo; or, Modern Astronomy Recast.* By JAMES REDDIE, F.A.S.L., Hon. Mem. Dial. Soc. Edin. Univ. London, 1863. Hardwicke.
2. *Quadrature du Cercle.* Par un Membre de l'Association Britannique pour l'avancement de la Science (JAMES SMITH). Bordeaux, 1863.

Well said the poet, though he did not anticipate the present application of his lines—

“The times have been,
That, when the brains were out, the man would die,
And there an end: but now they rise again,
With twenty mortal murders on their crowns,
And push us from our stools.”

Quietly ignored by the British Association, of which they are members, or, worse, informed politely that their lucubrations are of that order which the French Academy (“in consequence of a resolution already ancient”) so wisely refuses to entertain, Messrs Reddie and Smith rush into print in Latin and French, the so-called languages, *par excellence*, of true science, with the barely concealed hope that at last the world will do them justice. So far their cases are parallel; but in respect of patient endurance of well-merited neglect, or still more richly-deserved castigation, at the hands of common-sense, Mr Smith is far in advance of his fellow-martyr. He deserves, therefore, some little consideration (which we can hardly extend to Mr Reddie, for a reason presently to be shown), and we shall endeavour in a word or two to show where and how he errs.

It is certain, even to Mr Smith, that the area of a circle is half the product of its radius and circumference.

It is also certain that the circumference is proportional to the radius. In scientific works this ratio (whatever it may be) is denoted by $2\pi:1$ where π is some definite number.

From these two facts it follows that the area of a circle is proportional to the square of its radius, and that the proportion is $\pi : 1$. These form the starting-point of Mr Smith's work.

Now, if we merely compare one circle with another, as to area or circumference, we shall have continual verifications of the foregoing facts, whether we assume π to be $3\frac{1}{2}$, as Mr Smith does, or 20,000,000,000,000, which we would advise him to try. All the numerical work, (with a slight exception) in Mr Smith's pamphlet, consists of deductions of one of the above facts from the other two, and would be equally successful whatever number we assume for π .

Mr Smith, of course, breaks down when he comes to apply his supposed value to compare the circumference of a circle with that of an inscribed polygon (which we cannot but suppose he will allow to be shorter). But if π be $3\frac{1}{2}$, the circumference of a circle whose radius is 1 is 6.25, and that of the inscribed polygon of twenty equal sides (which can be constructed by pure geometry, and is therefore *not* liable to suspicion, as results of logarithmic tables are supposed to be by squarers of the circle) is 6.2573... as Mr Smith may easily assure himself by calculation. How does he get over this? But we need not inquire, nothing is impossible to a squarer of the circle.

Mr Reddie's former difficulty lay in his not being able to conceive the idea of mass or inertia as distinct from, and capable of existence without, weight. In this he was, of course, entirely wrong and unutterably silly.

He has now discovered that the only motions we are cognisant of are *relative*, and here he is indisputably right, though a little late in the field. But when he asserts, as a consequence, that Newton's view of the solar system, or rather that of Copernicus and Kepler, falls to the ground, he has completely ignored the fact that Newton deals with *relative* motion only. He will find his main argument (that the moon's path is throughout concave towards the sun) in most good treatises on astronomy; and will find, if he would inquire into what has been done before proceeding to act for himself, that all this is perfectly consistent with Kepler, gravitation, laws of motion, &c., &c. So much for his latest performance; where on earth or in heaven will he *next* break ground?

But a word with him before we close. We have done no more than hold up to deserved, but *kindly*, ridicule his preposterous ideas, which, since he not only published them, but *sent* them to us for review, were public property, so far as our honest but discerning criticism was concerned. He retorts upon us as "a shabby hack" (p. 36). We were "mean enough deliberately to make a mis-statement, finding nothing we dared gainsay" (p. 52). We are "a mendacious and impertinent writer" (p. 52). We have



"put ourselves out of the category of gentlemanly writers" (the same pregnant p. 52), because we animadverted on the conduct of certain journals in praising his curiously absurd essay, instead of pointing out its errors, or passing it over in silence. *Tantane animis cœlestibus iræ?*

To the last of the above charges we are afraid we must again expose ourselves. Mr Worms, of whom we had hoped we had heard the last, has permitted the publication, by Mr Reddie, of a note in which he states that he considers our estimate of his book (*ante*, vol. xvii. p. 104) "quite beneath his notice." Messrs Worms and Reddie, though *both* in egregious error, do not very cordially agree—except in their opinion of us. Now to every word of our remarks on Mr Worms we most positively adhere, and we are profoundly afflicted (for the sake of British science) that a journal of such deservedly high character as the "Athenæum" has in this instance (though it *did* detect Mr Reddie) unguardedly given its sanction to a most absurd mixture of original nonsense and spoiled extracts. Let us add in conclusion, that our good-humour and impartiality are not to be soured or distorted by abuse, and that, in spite of the terrible epithets Mr Reddie has applied to us, we shall keep on the gloves as usual in our next encounter with him, which, unless appearances are deceitful, is not likely to be long deferred.

Sketch of Elementary Dynamics. By W. THOMSON and P. G. TAIT. Edinburgh: MacLachlan and Stewart.

We do not remember ever to have seen a work on dynamics in which the fundamental principles of the science were more clearly and beautifully explained than they are in the pamphlet which we have just perused.

Any one who has glanced over the volumes on elementary mechanics (and their number is legion) which form the text-books of our schools, must, we think, have perceived in most of these a want of philosophical method and clearness of conception displayed by the author in his treatment of the subject.

We appeal especially to the student who has read one of these text-books for information, if it did not require a very great mental effort on his part to recognise the different branches as really forming one science. We think that such an one must have often wondered at the unnatural separation maintained between the parallelogram of forces and that of velocities, and sometimes also speculated as to what becomes of the motion when two non-elastic bodies impinge against each other in opposite directions.

Then, again, in works of a somewhat more ambitious character, in which the subject is treated analytically, the mode by which you are led to a result is very much the same as that by which the railway traveller who journeys by night arrives at his destination—the process is not invigorating, and there is no appreciation of the beauties of the way. So when you have once formed your differential equation, and abandoned yourself to the labour of solving it, you have quite lost sight of the physics and the geometry of the problem.

In the present pamphlet, which we are content to view only as an instalment, the authors have, we think very successfully, introduced much of the beauty of the geometrical method with very little of its cumbrousness. They have also availed themselves to the fullest extent of that great discovery of modern times—the conservation of energy; and we shall afterwards adduce an example of their happy employment of this principle. We must add to this a philosophical arrangement and a unity of conception in order to characterise the book. So novel are many of its solutions, that our first emotion is that of surprise, as we recognise some very old friend in his new dress; while our second is altogether one of pleasure from his improved appearance.

We now quote a few lines from the commencement of the volume:—

“Dynamics is the science which investigates the action of force. Force is recognised as acting in two ways,—1st, So as to compel rest or to prevent change of motion; and, 2d, So as to produce or to change motion. Dynamics, therefore, is divided into two parts—Statics and Kinetics.

In Kinetics it is not mere *motion* which is investigated, but the relation of *force* to motion. The circumstances of mere motion, considered without reference to the bodies moved or to the forces producing the motion, or to the forces called into action by the motion, constitute the subject of a branch of pure mathematics, which is called ‘Kinematics.’”

The first part of the pamphlet is therefore devoted to Kinematics. Here, after defining velocity, it is shown, that if two velocities be represented by the sides of a parallelogram, their resultant is represented by its diagonal. Acceleration, in its most general sense, is defined as the “*rate of change of velocity, whether this change take place in the direction of motion or not;*” also, “the *moment* of a velocity or a force about any point is the rectangle under its magnitude, and the perpendicular from the point upon its direction.” It is then shown, that the moment of the resultant velocity of a particle about any point in the plane of the components is equal to the algebraic sum of the moments of the components.

We have entered thus into detail in order to introduce to our readers the following very beautiful proof of the "equable description of areas :"—

"When the acceleration is directed to a fixed point, the path is in a plane passing through that point; and in this plane the areas traced out by the radius-vector are proportional to the time employed. . . . Evidently there is no acceleration perpendicular to the plane containing the fixed and moving points, and the direction of motion of the second at any instant; and there being no velocity perpendicular to this plane at starting, there is therefore none throughout the motion; thus the point moves in the plane. Again, if one of the components (of the resultant velocity of a particle) always passes through the point (about which the moments are reckoned), its moment vanishes. This is the case of a motion in which the acceleration is directed to a fixed point, and we thus prove, that in the case supposed, the areas described by the radius-vector are proportional to the times; for the moment of velocity, which in this case is constant, is evidently double the rate at which the area is traced out by the radius-vector."

We have only time, before leaving this division of the subject, to request our readers' attention to the novelty and beauty of the authors' proof that the evolute of a cycloid is a similar and equal cycloid; and with this remark we pass on to the second part of the pamphlet, in which dynamical laws and principles are discussed. And first, we note that the term *centre of inertia* is advantageously substituted for *centre of gravity*.

"The *centre of inertia* of a system of equal material points (whether connected with one another or not), is this point whose distance is equal to their average distance from any plane whatever. The centre of inertia of any system of material points whatever (whether rigidly connected with one another, or connected in any way, or quite detached), is a point whose distance from any plane is equal to the sum of the products of each mass into its distance from the same plane divided by the sum of the masses.

"The sum of the momenta of the parts of the system in any direction is equal to the momentum, in that direction, of the whole mass collected at the centre of inertia."

The following is the authors' statement of the second law of motion, and its corollary the parallelogram of forces :—

"When any forces whatever act on a body, then, whether the body be originally at rest or moving with any velocity and in any direction, each force produces in the body the exact change of motion which it would have produced if it had acted singly on the body originally at rest.

"A remarkable consequence follows immediately from this view of the second law. Since forces are measured by the changes of motion they produce, and their directions assigned by the directions in which these changes are produced; and since the changes of motion of one and the same body are in the directions of, and proportional to, the changes of velocity, a single force, measured by the resultant change of velocity, and in its direction, will be the equivalent of any number of simultaneously acting forces. Hence,

"The resultant of any number of forces (applied at one point) is to be found by the same geometrical process as the resultant of any number of simultaneous velocities."

After stating the third law of motion, which asserts the equality of action and reaction, the authors then deduce, as immediate consequences of the second and third laws, the following amongst other important propositions:—

"(a) The centre of inertia of a rigid body moving in any manner, but free from external forces, moves uniformly in a straight line.

"(b) When any forces whatever act on the body, the motion of the centre of inertia is the same as it would have been had these forces been applied, with their proper magnitudes and directions, at that point itself."

We present the following as the authors' view of the *conservation of energy*:—

"A limited system of bodies is said to be *dynamically conservative*, if the mutual forces between its parts always perform or always consume the same amount of work during any motion whatever by which it can pass from one particular configuration to another. The whole theory of energy in physical science is founded on the following proposition:—

"If the mutual forces between the parts of a material system are independent of their velocities, whether relative to one another or relative to any external matter, the system must be *dynamically conservative*.

"For if more work is done by the mutual forces on the different parts of the system in passing from one particular configuration to another, by one set of paths than by another set of paths, let the system be directed, by frictionless constraint, to pass from the first configuration to the second by one set of paths and return by the other, over and over again for ever. It will be a continual source of energy without any consumption of material, which is impossible."

We have only time to direct the reader's attention to the beautiful proof given in page 40, that "a particle attracted to a

fixed point, with a force inversely as the square of the distance, describes a conic of which the point is a focus ;" and

We conclude with the following ingenious application of the law of energy :—

" When an incompressible liquid escapes from an orifice, the velocity is the same as would be acquired by falling from the free surface to the level of the orifice.

" For we may neglect (provided the vessel is large compared with the orifice) the kinetic energy (energy of motion) of the bulk of the liquid ; the kinetic energy of the escaping liquid is due to the loss of potential energy of the whole by the depression of the free surface. Thus the proposition at once."

It is hardly necessary, after these quotations, to make any observation, except to recommend this little pamphlet, as one full of beautiful and ingenious solutions, and as well worthy of being read by all who feel an interest in the subject, but especially by those who mean to study the more complete treatise which the authors have promised to the public.

Climate : An Inquiry into the Causes of its Differences, and into its Influence on Vegetable Life. By C. DAUBENY, M.D., F.R.S., &c., Professor of Botany and of Rural Economy in the University of Oxford.

This small volume contains four lectures delivered by Professor Daubeny before the Natural History Society of Torquay in February last, on Climate and its influence on vegetation. Whilst the subject is handled by the learned professor in its more popular and practical bearings, there is at the same time contained in these able lectures a large amount of valuable scientific information, embodying the recent results arrived at by meteorologists with respect to the atmosphere and its phenomena. In the discussion of the question, the author defines the climate of a country to be its relations to temperature, light, moisture, winds, atmospheric pressure, and electricity ; but amongst these the first place must be conceded to the intensity of the heat, and to its distribution over different portions of the year, as it is this which in a great degree regulates the other conditions, and is also itself of all others the one most indispensable for the exercise of the functions of vegetable and animal life. This important element of heat is derived from the living bodies scattered over the globe, from the various processes carried on by natural agencies or through the instrumentality of man, from the interior of the earth, and from

solar radiation. Of these sources whence heat is derived, it is only necessary for the meteorologist to consider the heat received from the sun's rays, because that received from the other sources is too inconsiderable in amount to be appreciable, except in rare and exceptional cases.

Professor Dove has calculated the normal temperatures of the different parts of the globe; that is, what would be the temperature of each place provided the earth presented a solid uniform surface everywhere to the sun's rays. These normal lines are of necessity parallel to the equator, or coincident with the parallels of latitude—the temperature increasing on each side of the equator to 10° of latitude, and thence decreasing proportionally on either side toward the poles. But, on account of the unequal distribution of land and water, the irregularities of the surface, the prevailing atmospheric and oceanic currents, and other concurrent causes modifying the climates of particular localities, few places possess what may be called their normal temperatures, some falling short of it, but a still greater number exceeding the temperature assigned. Thus London, in latitude $51^{\circ}30'$, ought to have a mean temperature of $39^{\circ}0'$, whereas observation assigns to it one of $50^{\circ}8'$, or $11^{\circ}8'$ higher. Everywhere on the Atlantic shore of North America the temperature is lower than in Europe. The respective mean temperatures of Nantes in France and St John's in Newfoundland, both in corresponding latitudes, are $54^{\circ}9'$ and $38^{\circ}4'$,—the difference in favour of the former being $16^{\circ}5'$. If the temperature of different places be compared for the seasons, it will be found that the discrepancy is still more striking. Thus, whilst the winter temperature of Drontheim, lat. 63° , is $+23^{\circ}3'$, that of Yakoutzk, lat. 62° , is $-36^{\circ}4'$, showing an enormous difference of $60^{\circ}0'$; whilst the winter temperature of Quebec is $14^{\circ}2'$, that of Penzance, 4° nearer the pole, is $44^{\circ}2'$, or $30^{\circ}0'$ higher than that of Quebec. Again, the summer temperature of Edinburgh is $57^{\circ}2'$, while in Moscow, in the same parallel of latitude, it is $65^{\circ}4'$, or about $8^{\circ}0'$ higher.

"Climates, then, may be divided into equable and excessive, according to the degree in which the mean temperature of the summer and winter differs from that of the entire year; and with reference to the growth of vegetables, far more importance must be attached to the heat prevailing during summer than to the mean temperature of the climate collectively taken.

"Thus even in Russia and Siberia fine crops of wheat and other kinds of corn are obtained, because the summer temperature rises to the requisite point for ripening the seed, whilst in the north of Scotland, the Orkneys, and the Faroe Islands, although the mean temperature of the year is higher, these crops do not succeed.

"It may be doubted, however, whether even the mean temperature of the summer season affords a sufficient clue to all the variations in the character of the vegetation which are attributable to heat. A plant is not like a spring which is pushed forwards a certain number of degrees by the application of a definite force, and when that pressure is removed, returns again to its original position; for when the stimulus of heat is applied to it, its organs undergo a degree of development, which they retain even although the temperature should afterwards be reduced. Hence it is necessary to note the extremes of temperature to which a country is liable, as well as the mean of its summer and winter climate.

"Should it happen, for instance, that the cold in a low or sub-tropical latitude ever approaches even for a single night to the zero of Fahrenheit, certain trees, such as the Orange, would infallibly perish; and hence they can never be indigenous in countries subject to such contingencies.

"The general climate of the British Isles is so exceptionally mild, that we have introduced the plants of warmer regions generally into cultivation, and begun to consider them as in a manner naturalized; but that they are not so, and could never have established themselves in the soil without the aid of man, became evident from the effects of the rigorous winter of 1860-61,—one of those seasons of unusual severity which, however, are sure to occur within a certain cycle of years, and to entail the destruction of all these denizens of a more temperate climate, which, rashly presuming upon the mildness of many preceding winters, had begun to regard this as their home."

Among the causes which affect climate are the relative distribution of land and water; the proximity to a large continent or to a wide expanse of sea; the chilling influence of mountains which retain the winter snow during the greater part of the year, or push down glaciers into the valleys; and the shelter afforded by forests or hills of moderate height. Of these, the relative power of land and water, with respect to the reception, radiation, and transmission of heat is the most important influence. The following extracts show the changes produced by different arrangements of sea and land in different countries:—

"Thus it is only on extensive plains, at a distance from snow-capped mountains, that a high range of temperature can maintain itself even during the day in a northern latitude. On the other hand, over such level tracts as those of Russia and Siberia, great heats prevail in summer even in comparatively northern regions.

"Moscow, for instance, in north latitude $55^{\circ} 48'$, where the mean temperature of the coldest month is only 13° of Fahr., enjoys in July a heat of $66^{\circ} 4'$; and at Yakoutzk, a Siberian town, in latitude 62° , at which the mean temperature of January

is $36^{\circ} 37'$ below zero, the thermometer in July rises to nearly 69° of Fahr.

"In islands, on the contrary, situated in northern parallels, the radiation of heat in summer exerts a much feebler influence; so that Stromness, in lat. $58^{\circ} 57'$, has a summer temperature of only 54° ; and Unst, the most northern of the Shetland Islands, in lat. 60° , one only of 52° .

"Hence whilst at Christiania, in lat. $59^{\circ} 55'$, fine timber abounds, and crops of wheat and other grain ripen, inasmuch as the mean summer temperature rises nearly to 60° , none but the hardiest kind of barley will grow in the Hebrides, between the parallels of 56° and 58° ; and the trees are there reduced to a few of the robust species, which are both stunted and uncommon.

"In winter, however, the case is reversed. The extreme cold of extensive continents, such as Russia and Siberia, is caused in part by the radiation of heat from their surface during the long nights of these northern latitudes, and still more through their participation in the climate of regions more northerly than themselves, owing to the winds which commonly come from that quarter in the winter season, and which, bringing with them the temperature of the Arctic circle, first condense the moisture into snow, and afterwards impart to the countries they pass over the dry and cutting cold which characterises them.

"But on the sea the circumstances are different, owing to a property peculiar to water, which would seem specially designed as a provision for mitigating the intensity of cold.

"This is its arriving at its greatest density, not at the point at which it freezes, but 8° above, so that whilst it goes on progressively contracting in volume down to 40° , it afterwards again expands, until it falls to the temperature of 32°

"Hence owing to this constant circulation of the lighter and heavier portions of the water, the whole must attain the temperature of 40° before any ice is formed upon its surface, and accordingly it is hardly possible that a deep lake should be frozen over even by the longest and most intense frost that can occur.

"The effect of this constant circulation throughout all portions of a body of water in mitigating the severity of insular climates is sufficiently apparent.

"The ocean may, in fact, be regarded as a store-house of heat, which it dispenses to the air passing over its surface, thus rendering it impossible that the latter should ever attain the same extreme degree of cold which it acquires on a continent.

"Hence the equable character of the climate in insular situations, which has been pointed out as prevailing during the summer, holds good also, for the reasons just given, in the winter likewise."

Again, the currents of the atmosphere and of the ocean exer-

cise a powerful influence over the climate of a country; and it is to the influence of two such currents that the British Isles owe their singularly mild, equable, and healthy climate. The Florida Gulf stream impinges on our western shores, bringing with it the warmer temperature of southern latitudes; and since the prevailing direction of the wind is south-west, a higher temperature is imparted to the British Isles in common with the whole of Western Europe. The south-western parts of Ireland and Great Britain receive the greatest benefit, and these places accordingly are the favourite residences of invalids during winter, and they may also be noticed as famous for the early growth of vegetables for the London market. The western coasts of South America may be referred to as illustrating the effects of a cold oceanic current flowing from the Polar Seas, and thus reducing the temperature of those parts by which it passes. Thus the mean temperature of Callao in 12° S. Lat. is only $68^{\circ}.9$, while that of Rio Janeiro in 23° S. Lat. is $73^{\circ}.8$.

The following important practical remark is made in discussing the temperature of the soil:—

“It is known that every plant requires a certain amount of heat, varying in the case of each species, for the renewal of its growth at the commencement of the season.

“Now when this degree of heat has spurred into activity those parts that are above ground, and caused them to elaborate the sap, it is necessary that the subterranean portions should at the same time be excited by the heat of the ground to absorb the materials which are to supply the plant with nourishment. Unless the latter function is provided for, the aerial portions of the plant will languish from want of food to assimilate. Indeed, it is even advisable that the roots should take the start of the leaves, in order to have in readiness a store of food for the latter to draw upon.”

It is only in well-drained well-pulverised soils that plants enjoy the fullest benefit of this consideration; for, as has been shown in the Scottish Meteorological Society's Report for the quarter ending December 1862, the effect of draining is,—by drawing off the water, and thus filling the interstices of the soil with air which is about the worst conductor of heat,—to prevent the penetration of frosts into such soils to any great depth.

The next most important element of climate is the moisture of the air.

“We must, however, carefully distinguish between the humidity present in the atmosphere in the form of vesicular vapour, and that existing in it in an invisible or *aëriform* condition.

“All liquids have a tendency to pass into vapour, until checked by the pressure of their *own atmosphere*; and this tendency,

which goes by the name of the tension of vapour, increases in each case in proportion to the advance of its temperature.

"Whenever, therefore, the soil and subsoil are not entirely destitute of humidity, the atmosphere above must contain a certain amount of water, not as vesicular vapour, but in a perfectly aëri-form condition.

"Now the effects produced upon living beings by the presence of moisture in the atmosphere are entirely different, according as it exists in one or other of these states.

"Vesicular vapour, manifesting itself in the form of fog or mist, causes, as every one knows, a sensation of chill, owing to the abstraction of heat from our persons, caused by the moisture which attaches itself to them, and likewise, for the same reason, interferes with the healthy functions of the skin, and even of the lungs.

"But in an aëri-form condition the very opposite effect takes place.

"Professor Tyndall has lately pointed out that humid air, or air containing much moisture in a transparent or an aëri-form condition, exerts a remarkable influence both in absorbing and in radiating heat. Owing to the former property, aqueous vapour acts as a kind of blanket upon the ground, and contributes in a very striking manner to the retention of its heat.

"Hence when the air is perfectly divested of moisture, as in the sandy deserts of Africa, in Siberia, and even in Australia, the cold at night is almost insupportable, owing to the absence of that protection which is afforded by aqueous vapour when present in the atmosphere; whilst during the day the rapid abstraction of moisture from the surface of plants and animals, caused by the dryness, is equally deleterious to both.

"And as the radiation of heat from a body is always equal to its power of absorbing it, it follows that air containing much moisture will, when it rises into the higher regions, sink rapidly in temperature, in consequence of the heat it sends forth into space; and, indeed, according to Tyndall, the amount radiated from air saturated with moisture is 16,000 times as great as that of air perfectly dry.

"One cause, therefore, of the profuse rains that occur in the tropics may be the cooling of the heated air, which rises from the earth into the higher regions of the atmosphere, and which, when it arrives there, radiates its heat freely into space, and thus has its capacity for moisture reduced.

"Professor Tyndal calculates, that 10 per cent. of the heat radiated from the earth in this country is stopped by 10 feet of the air which lies nearest to the ground.

"It would appear from the recent investigations of M. Duchartre

in France that the refreshing influence of rain and dew upon plants does not arise from the moisture deposited upon their surfaces being absorbed,—for not a particle of water enters into them through the leaves or stem,—but from its supplying the ground with water for the roots to absorb; and likewise, as I should infer, from the check which its presence affords to a too rapid radiation of heat from the parts above ground, by which the prejudicial effects of a too rapid cooling are provided against.”

The last two lectures are devoted to the discussion of the effect of these influences on vegetation, and the way on which climate determines the vegetation of a country. Palms and most other monocotyledonous trees are ill suited to the climates of temperate regions, because, from the liquid nature of the sap with which they are filled, they are liable to freeze,—an act which, by the expansion it occasions, proves of most fatal consequence at all times to the vegetable organisation. On the other hand, dicotyledonous trees, which belong to temperate countries, are protected from frosts either by numerous layers of bark, abounding in air-vessels, or are provided with essential oils and other juices not susceptible of freezing, such as are found in the bark and wood of conifers. The valuable rule on this subject laid down by Decandolle may be briefly stated. The power each plant possesses to resist the extremes of temperature is in direct proportion to the oils contained in the juices, the viscosity of the juices, the smallness of the cells and vessels, and the quantity of air entangled between the parts of the vegetable tissues, or the protection afforded by hairs, down, or air-vessels; whilst the liability to suffer from great heat or from frost is proportional to the quantity of water in the tissues, the mobility of the juices, the size of the cells and vessels, and the absence of air in the external layers.

But the summer and winter climates of a country are what chiefly determine its vegetation.

“There are certain plants, like the Vine, which require an intense heat in summer to bring their fruit to maturity, but yet are capable of resisting a severe cold during winter; they thrive, for instance, on the borders of the Rhine, or in Switzerland, where the winters are very severe; but scarcely even ripen their fruit in England, even in Devonshire or Cornwall, where the thermometer rarely falls to the freezing point of water.

“Other plants, on the contrary, like the Myrtle, cannot resist cold, but do not demand during any part of the year an exalted temperature.

“Thus they luxuriate near the southern coasts of England, but do not show themselves on the Continent, until we reach a much lower latitude than that of this country.

“The former class of plants, therefore, may be said to be

adapted for an excessive climate, and therefore thrive best on continents; the latter for an equable one, and consequently succeed most upon islands.

"It may also be remarked, that annuals are adapted for continental climates, as they require heat for the ripening of their seeds, but die away in winter; whilst perennials are better calculated for islands, as it is essential that the winter should not be so rigorous as to destroy them, but not equally necessary that the summers should be always hot enough to ripen their seeds, a failure in this respect for several successive years not entailing the destruction of the species."

Here the extreme temperatures of a country, as distinguished from the mean temperature both of its seasons and its year, must be carefully kept in view. For that which determines the flora of a country, or the plants which will thrive and live in its climate, is not whether the plants will live through such winters as ordinarily occur, but whether they will survive the severest frosts that have happened, and may therefore be expected to recur in a succession of years. But the number of species which admit of cultivation is greater than that of those which grow wild, inasmuch as the former can be maintained by the art of man protecting them during winter, or replacing them with new plants when the old ones happen to be destroyed by frost. This is particularly the case with those whose cultivation is remunerative. As man has little or no power to change the climate, it is only by studying the climate of his neighbourhood in all its details that he can take every advantage of it, and thus rise superior to it, and in some sense overcome it. The subject of local climate is one deserving more attention than it has yet received. The exposure of a piece of ground, its sloping or level character, its relatively low or high position, the nature of its subsoil or its drainage, its sheltered or exposed situation, and its relation to moisture or dryness, may all conspire to give it as mild and genial a climate as is enjoyed some degrees southward.

We conclude this notice with the following extract, showing the effect different seasons have on agricultural produce:—

"It may be useful for the farmer to possess the data for estimating the influence which a summer, warmer or colder, wetter or drier than ordinary, has exerted upon the production of his farm, so as not to be misled in his calculations as to the advantages or disadvantages of any novel plan of cultivation.

"These data have in part been supplied by Mr Lawes in an elaborate paper published in the eighth volume of the Royal Agricultural Society's Journal for 1848, in which he shows, that in 1844, 1845, and 1846, the difference in the amount of produce was in accordance with the general character of their respective seasons.

"Thus it will be seen by turning to the table referred to in the note,* that, in 1844, when there were only 81 rainy days, and when the mean summer temperature was $57^{\circ} 5'$, the farm produced 16 bushels of corn to the acre, and the weight of the bushel was $60\frac{1}{2}$ lb., whilst the grain bore to the straw as high a ratio as about 82 to 100.

"In 1846, when there were 93 rainy days, and when the mean temperature was as high as 59° , the yield was about 17 bushels per acre, and the weight of a bushel 68 lb.; but the proportion of grain to straw was lower, namely 76 to 100, showing that the yield of corn had been influenced by temperature, but that the quantity of straw had been increased by the amount of rain.

"Lastly, in 1845, when the number of rainy days was greater, though the temperature had been lower than in either of the two other years,—the former being 110, the latter 55,—the yield was much greater, amounting to 23 bushels per acre, but the weight of the corn per bushel less—namely, 56 lb.; and the increase in the produce of straw such, that the grain only bore to it the proportion of 49 to 100.

"These figures appear to show that, although the greater quantity of rain was favourable to the amount of grain, yet that it tended to increase in a still greater ratio that of straw; and that the higher the temperature of the year was, the heavier the grain would prove, so as to make up in quality for its deficiency in actual quantity.

"Thus it would appear, that, under the same treatment, the produce may vary in the proportion of 7 bushels per acre, according to the difference of season, which is equivalent to one quarter of the normal produce; or, calculating this at 30 million quarters in a good year, may be as much as $7\frac{1}{2}$ million deficient."

* The following table indicates the effect of climate upon the quantity and quality of the produce of the unmanured piece of the experimental wheat-field, (during three seasons); the average results of the variously manured, &c., are also given:—

	1844.	1845.	1846.
Corn per acre in bushels, ...	16	23	17.25
Straw per acre in pounds, ...	1120	2712	1513
Weight of Corn per bushel in pounds, ...	$60\frac{1}{2}$	$56\frac{1}{2}$	$68\frac{1}{2}$
Percentage of Corn to straw (straw 1000), ...	821	534	797
<i>Mean of all the Plots.</i>			
Weight of Corn per bushel in pounds, ...	$60\frac{1}{2}$	$56\frac{1}{2}$	68
Percentage of Corn in straw (straw 1000), ...	868	490	765
Mean temperature, ...	$57^{\circ} 5'$	$55^{\circ} 2'$	$59^{\circ} 1'$
Rainy days in $80\frac{1}{2}$ weeks, ...	81	110	93

Manual of the Metalloids. By JAMES APJOHN, M.D., F.R.S., M.R.I.A., Professor of Chemistry in the University of Dublin. London: Longman & Co. 1864. 12mo. Pp. 596.

In his preface the author states that the work has been written as one of Galbraith and Haughton's series of Manuals on different branches of science. The object has been to produce a condensed, but at the same time tolerably comprehensive treatise, in which no topic of importance should be omitted, while all would be discussed with as much brevity as is consistent with clearness. It is intended as a handbook in Chemistry for students in medicine and engineering.

In the Introduction, the laws of Combination, Chemical Notation and Nomenclature, the Relations of Atomic Weights, the Law of Volume, Atomic Volume, the Unitary System of Atomic Weights, Isomerism, Chemical Formulæ, Isomorphism, Dimorphism, Reaction of Bodies on each other, the causes which determine Decompositions, and the Division of Simple Bodies are considered. The following are the fifteen substances which he considers as metalloids:—Oxygen, Hydrogen, Nitrogen, Sulphur, Selenium, Tellurium, Chlorine, Bromine, Iodine, Fluorine, Phosphorus, Arsenic, Boron, Silicon, and Carbon. The use of the simpler forms of algebraic calculation has enabled the author to give precision and brevity to many of his statements. At the present day, it is clear that any one who wishes to study chemistry, must be properly instructed in arithmetic and the elements of algebra.

In speaking of coal gas, Dr Apjohn remarks:—

“Coal Gas.—A mixture of inflammable gases, to be burned for the light, and sometimes for the heat it yields, has been derived from different sources, viz., from turf, vegetable and animal oils, and from rosin. The gas from turf has a low illuminating power, and is now seldom made; and that yielded by rosin and the oils, though of good quality, is relatively so costly, that its manufacture has also been abandoned. Our streets and houses are now illumined exclusively by gas derived from bituminous coal; and the subject, therefore, of the gaseous hydrocarbons cannot be considered as sufficiently discussed without some remarks upon the preparation, composition, properties, and uses of this most important combustible substance. As an introduction to this subject, attention is directed to the following table, which gives the ultimate composition in 100 parts of a Welsh anthracite, of five specimens of English bituminous coal, and of a variety of brown coal, or

lignite. The last two columns of the table give, the one the Ash, the other the Coke, yielded by each specimen.

	Carbon.	Hydrogen.	Nitrogen.	Sulphur.	Oxygen.	Ash.	Coke.
Anthracite, Wales . . .	91.44	8.36	0.21	0.79	2.58	1.52	92.20
Coking Coal, Newcastle .	81.41	5.88	2.05	0.75	7.90	2.07	66.70
Cannel Coal, Wigan . .	80.07	5.53	2.12	1.50	8.09	2.70	60.86
Coal, Wolverhampton . .	78.57	5.29	1.84	0.89	12.88	10.30	57.21
Wallsend Coal, Elgin . .	76.09	5.22	1.41	1.53	5.05	10.70	58.40
St Helen's Coal, Lancashire	75.80	5.21	1.92	0.90	11.89	5.17	65.50
Methill Brown Coal . .	65.96	7.78	0.96	0.75	9.23	15.32	...

"All these coals but the first include a considerable amount of bitumen, whose principal constituents are carbon and hydrogen; and this, when exposed to an elevated heat in cast-iron or fireclay tubes, called *retorts*, is resolved into coke, which remains in the retorts, and various volatile products, which are expelled; these latter being tar, an ammoniacal liquid called gas liquor, and a mixture of various gases.

"The tar and gas liquor are by a reduction of their temperature condensed, and the gas, after having undergone a certain process of purification, is conducted into the *gasometer*, from whence, by a regulated pressure, it is transmitted through metal pipes to the various points at which it is to be consumed. The retorts used in the process are usually 7 feet in length and about 1 foot in diameter, the cross section being, not a circle, but an ellipse. Five of them are usually set in the same furnace, and, when sufficiently heated, each is charged with 150 lbs. of coal. It takes from four to five hours to work off this charge, and the products per cent. are the following, being the means of four experiments by Mr Barlow on the Pelton main Newcastle coal:—

	lbs. per ton.
Gas	475.0
Coke	1540.0
Tar	112.5
Gas Liquor	112.5
	2240.0
	100.00

The specific gravity of the gas was 0.653, and its volume from 1 ton of coal, 9500 cubic feet; or 4.24 cubic feet for every pound of coal.

"The crude aëriform product, as has been already stated, is not

a single gas, but a mixture of several, of which the following may be considered as a pretty complete enumeration :—

Hydrogen	H.
Marsh Gas	C_2H_4 .
Olefiant Gas	C_4H_4 .
Butylene (Oil Gas), traces	C_8H_8 .
Carbonic Oxide	CO.
Carbonic Acid	CO_2 .
Sulphide of Hydrogen	HS.
Ammonia	NH_3 .
Nitrogen	N.
Cyanogen, a trace	NC_2 .
Vapour of Water	HO.
„ of Bisulphide of Carbon	CS_2 .
„ of Benzole	$C_{12}H_6$.

* * * * *

“The determination of the exact composition of a coal gas is a task of considerable difficulty; but if the ammonia, carbonic acid, and sulphide of hydrogen be first removed, its analysis may be accomplished in the following manner :—

“1. The oxygen is absorbed by a ball of papier-maché soaked with an alkaline solution of pyrogallie acid.

“2. The heavy hydrocarbons, or luminiferous constituents, are next condensed with absolute sulphuric acid in the manner already explained.

“3. The residual gas, which is a mixture of hydrogen, marsh gas, carbonic oxide, and nitrogen, is burned in a eudiometer with a known volume of oxygen, the quantity used being more than is sufficient for the conversion of their carbon into carbonic acid, and their hydrogen into water, and the volume left accurately measured. A ball of hydrate of potash is then introduced into the tube, and the diminution of volume noted,—which corresponds to the volume of carbonic acid produced by the combustion of the marsh gas and carbonic oxide.

“4. There now remain but the nitrogen of the coal gas and any excess of the oxygen used for the combustion. The latter is got by adding at least 2 volumes of pure hydrogen, again exploding in the eudiometer, and noting the condensation. This divided by 3 will be the volume of the oxygen in excess, and of course the nitrogen and the oxygen consumed are had by difference.

“5. The aggregate of the oxygen in the gas, its nitrogen, and the heavy hydrocarbons, is now subtracted from the volume of gas with which the experiments were begun, and a residue is obtained representing the sum of the volumes of the hydrogen, marsh gas, and carbonic oxide. Let the bulk of this sum in cubic

inches be v , the amount of oxygen consumed be v' , and the volume of the carbonic acid formed be v'' ; and designating by x , y , and z , the respective bulks of the hydrogen, marsh gas, and carbonic oxide, we will have the three following equations:—

“(1.) $x + y + z = v$. (2.) $\frac{1}{2}x + 2y + \frac{1}{2}z = v'$. (3.) $y + z = v''$.”

“Equation (1) does not require any explanation.

“Equation (2) expresses the fact that for its combustion hydrogen requires half its volume, marsh gas twice, and carbonic oxide half its volume of oxygen.

“Equation (3) assumes, what is also the result of experiment, that marsh gas and carbonic oxide, when burned with oxygen, give their own volume of carbonic acid.

“From the solution of this simple equation we deduce the values of x , y , and z . They are as follows:—

$$\begin{aligned} x &= v - v'' \\ y &= \frac{2v' - v}{3} \\ z &= v'' - \frac{2v' - v}{3}. ” \end{aligned}$$

These extracts are sufficient to show the mode in which the subjects are treated. We consider the work as an excellent contribution to the teaching department of chemistry; as well worthy of the reputation of its author, and as admirably fitted for the use of students.

On the Popular Names of British Plants ; being an Explanation of the Origin and Meaning of the Names of our Indigenous and most commonly cultivated Species. By R. C. ALEXANDER PRIOR, M.D., F.L.S., Fellow of the Royal College of Physicians of London. 8vo. London: Williams and Norgate. 1863.

This is a curious work, written by a medical man of high literary attainments, and who at the same time possesses extensive and accurate botanical knowledge. It is a welcome addition to our botanical literature. The author's acquaintance with the languages of Greece and Rome, and with those of modern Europe, including Scandinavia, and his intimate knowledge of plants, acquired during travels to various parts of the Old and New World, render him peculiarly qualified for the very difficult task which he has undertaken, and give force and authority to his opinions. We recommend the work as well worthy of perusal, and as containing many interesting remarks on the etymology of the common names of British plants.

The Pines and Firs of Japan; illustrated by upwards of 200 wood-cuts. By ANDREW MURRAY, F.L.S., Assistant Secretary to the Royal Horticultural Society. 8vo. Pp. 124. London: Bradbury and Evans. 1863.

Coniferous trees and shrubs have of late years attracted much attention, and many useful plants of this Order have been introduced into Britain. Expeditions to Oregon, Columbia, and various parts of North America, as well as to China, Japan, and other countries, have been successfully carried on; and, while additions have been made to our botanical knowledge, valuable plants have been secured for our plantations. Many landed proprietors in this country are now fully alive to the importance of the subject, and have contributed liberally to expeditions which had for their object the introduction of hardy conifers likely to yield valuable timber.

In the present work we have an account of the coniferæ of Japan from one who has devoted much attention to the subject, and who was one of the originators of an expedition to Oregon, the result of which was the acquisition of several valuable conifers fitted for the climate of Britain. He is also the editor of the magnificent work which is now being published by Messrs Lawson and Son. The author states in the preface, that the two British botanists, who, by their explorations in Japan, have recently added so much to our knowledge of the vegetable products of that country,—viz. Mr Fortune and Mr John G. Veitch,—have kindly placed their stores of specimens and information at his disposal.

The original works on the flora of Japan, the author remarks, are Kæmpfer's "*Amœnitates Exoticæ*," Thunberg's "*Flora Japonica*," and Siebold and Zuccarini's "*Flora Japonica*." The first two are works published in 1712 and 1784, and the latter is a costly illustrated work, of which the portion relating to conifers was published in 1842. Recent travellers have made many additions, and have corrected errors in former observations. The species described in this work are eighteen:—*Abies Alcocquiæana*, *A. firma*, *A. Fortunei*, *A. Jezoensis*, *A. Kæmpferi*, *A. microsperma*, *A. polita*, *A. Tsuja*, *A. Veitchi*, *Cunninghamia sinensis*, *Larix japonica*, *L. Leptolepis*, *Pinus Bungeana*, *P. densiflora*, *P. Koraiensis*, *P. Massoniana*, *P. parviflora*, *Sciadopitys verticillata*. The work is executed with great care, and is illustrated by excellent wood-cuts. As a digest of Japan coniferæ, it is an important contribution to science.

Flora Australiensis: a Description of the Plants of the Australian Territory. By GEORGE BENTHAM, F.R.S., F.L.S., assisted by FERDINAND MUELLER, M.D., F.R.S. and L.S., Government Botanist, Melbourne, Victoria. Vol. I. Ranunculaceæ to Anacardiaceæ. Published under the authority of the several Governments of the Australian Colonies. 8vo. Pp. 508. London: Lovell Reeve and Co. 1863.

This is the first part of one of those colonial floras which are now in the course of publication under the authority of the Government of this country and of the colonies. Already several important works of a similar kind have been either completed or commenced, such as Bentham's *Flora of Hong Kong*, Grisebach's *Flora of the West Indies*, Thwaites' *Ceylon Flora*, Harvey and Sonder's *Flora of the Cape of Good Hope*; besides the more expensive illustrated Floras of Tasmania and New Zealand, by Dr Hooker. We may look ere long for a *Flora of Africa*, which has been to a certain extent explored by Vogel and M'Williams, Barth and Barter in the Niger Valley, Vogel and Petherick in the White Nile and Nubia, Welwitsch in Loanda, Speke and Grant in Eastern tropical Africa, Kirk and Mellor in the Livingstone Expedition, and Gustav Mann on the shores, islands, and mountains of the Bight of Benin. It is to be hoped that the researches of the various travellers in South America, including Humboldt, Bonpland, Kunth, Spix and Martius, St Hilaire, Ruiz and Pavon, Aublet, Endlicher, Cambessedes, Miers, Zuccarini, Gardner, Spruce, and others, may soon be embodied in a work similar to that which is now before us.

The work is to embrace a description of all the plants of the Australian continent and of Tasmania. The *Flora of New Zealand* will not be included. In the introduction Mr Bentham gives Outlines of Botany, with special reference to Local Floras. This consists of—1. A general view of the Organs of Plants, with definitions and descriptions; 2. The Principles of Classification and Systematic Botany; 3. Vegetable Anatomy and Physiology; 4. Remarks on the Collection, Preservation, and Determination of Plants; 5. An Index of terms and glossary.

The author then proceeds to describe the Australian plants in the class of Dicotyledons, and in this volume he embraces the Polypetalous orders, extending from Ranunculaceæ to Anacardiaceæ, and included in the series Thalamifloræ. The work promises to be one of great value. It is conducted by a botanist of high reputation, who is well qualified for the task, and it is

carried on with the aid of the unrivalled herbarium of Sir William Hooker, and with the assistance of Dr Mueller, the zealous and able director of the Botanic Garden at Melbourne, who has already done much to illustrate the Australian flora in his "*Fragmenta Phytographiæ Australiæ*," and other publications. This important flora could not be placed in better hands, and the completion of it will be hailed as a boon to science in general, and more especially to those who are prosecuting botany in the great continent of Australia.

Air-Breathers of the Coal Period: a Descriptive Account of the Remains of Land Animals found in the Coal Formation of Nova Scotia, with Remarks on their bearing on Theories of the Formation of Coal and of the Origin of Species. By J. W. DAWSON, LL.D., F.R.S., F.G.S., Principal of McGill University. 8vo. Pp. 81. Montreal: Dawson Brothers, 1863.

In the carboniferous period, the author remarks, though land plants abound, air-breathers are few, and most of these have only been recently recognised. We know, however, with certainty, that the dark and luxuriant forests of the coal period were not destitute of animal life. Reptiles crept under their shade, land-snails and millipedes fed on the rank leaves and decaying vegetable matter, and insects flitted through the air of the sunnier spots. Great interest attaches to these creatures—perhaps the first-born species in some of their respective types, and certainly belonging to one of the oldest land faunas, and presenting prototypes of future forms equally interesting to the geologist and the zoologist.

The author, who is well known as a sound and able geologist, after describing various species of *Hylonomus*, *Baphetes*, *Dendrerpeton*, *Hylerpeton*, *Eosaurus*, *Xylobius*, *Pupa*, which occur in the Nova Scotia carboniferous strata, concludes with some valuable remarks on the nature of the animals and on the state of the globe at the time when they lived on it. He states, "In the coal measures of Nova Scotia, while marine conditions are absent, there are ample evidences of fresh-water or brackish-water conditions, and of land surfaces, suitable for the air-breathing animals of the period. Nor do I believe that the coal measures of Nova Scotia were exceptional in this respect. It is true that in Great Britain, evidences of marine life do occur in the coal measures, but not, so far as I am aware, in circumstances which justify the inference that the coal is of marine origin. Alternations of

marine and land remains, and even mixtures of these, are frequent in modern submarine forests. When we find, as at Fort Lawrence in Nova Scotia, a modern forest rooted in upland soil forty feet below high-water mark, and covered with mud containing living *Tellinas* and *Myas*, we are not justified in inferring that this forest grew in the sea. We rather infer that subsidence has occurred. In modern salt marshes it is not unusual to find every little runnel or pool full of marine shell-fish, while in the higher parts of the marsh land plants are growing, and in such places the deposit formed must contain a mixture of land plants and marine animals with salt grasses and herbage, the whole *in situ*."

There are some interesting remarks on the bearings of the facts on the origin of species, and the author brings forward statements which convey strong impressions of the permanence of species. On the subject of transmutation, he gives the following judicious observations :—

" If we could affirm that the air-breathers of the coal period were really the first species of their several families, they might acquire additional interest by their bearing on this question of origin of species. We cannot affirm this ; but it may be a harmless and not uninteresting play of fancy, to suppose for a moment that they actually are so, and to inquire on this supposition as to the mode of their introduction. Looking at them from this point of view, we shall first be struck with the fact that they belong to all of the three great leading types of animals which include our modern air-breathers—the Vertebrates, the Articulates, and the Mollusks. This at once excludes the supposition that they can all have been derived from each other within the limits of the coal period. No transmutationist can have the hardihood to assert the convertibility, by any direct method, of a snail into a millipede, or of a millipede into a reptile. The plan of structure in these creatures is not only different, but contrasted in its most essential features. It would be far more natural to suppose that these animals sprang from aquatic species of their respective types. We should then seek for the ancestors of the snail in aquatic gasteropods, for those of the millipede in worms or crustaceans, and for those of the reptiles in the fishes of the period. It would be easy to build up an imaginary series of stages, on the principle of natural selection, whereby these results might be effected ; but the hypothesis would be destitute of any support from fact, and would be beset by more difficulties than it removes. Why should the result of the transformation of water-snails breathing by gills be a *Pupa* ? Would it not much more likely be an *Auricula* or a *Limnea* ? It will not solve this difficulty to say that the intermediate forms became extinct, and so are lost. On the contrary, they exist to this day, though they were not, in

so far as we know, introduced so early. But negative evidence must not be relied on; the record is very imperfect, and such creatures may have existed though unknown to us. It may be answered that they could not have existed in any considerable numbers, else some of their shells would have appeared in the coal formation beds, so rich in crustaceans and bivalve mollusks. Further, the little Pupa remained unchanged during a very long time, and shows no tendency to resolve itself into any thing higher or to descend to any thing lower. Here, if anywhere, in what appears to be the first introduction of air-breathing invertebrates, we should be able to find the evidences of transition from the gills of the prosobranchiate and the crustacean to the air-sac of the pulmonate and the tracheæ of the millipede. It is also to be observed that many other structural changes are involved, the aggregate of which makes a pulmonate or a millipede different in every particular from its nearest allies among gill-bearing gas-teropods or crustaceans.

It may be said, however, that the links of connection between the coal reptiles and the fishes are better established. All the known coal reptiles have leanings to the fishes in certain characters, and in some, as in *Archegosaurus*, these are very close. Still the interval to be bridged over is wide, and the differences are by no means those which we should expect. Were the problem given to convert a ganoid fish into an *Archegosaurus* or *Dendrerpeton*, we should be disposed to retain unchanged such characters as would be suited to the new habits of the creature, and to change only those directly related to the objects in view. We should probably give little attention to differences in the arrangement of skull bones, in the parts of the vertebræ, in the external clothing, in the microscopic structure of the bone, and other peculiarities for serving similar purposes by organs on a different plan, which are so conspicuous so soon as we pass from the fish to the batrachian. It is not, in short, an improvement of the organs of the fish that we witness so much as the introduction of new organs. The foot of the batrachian bears perhaps as close a relation to the fin of the fish as the screw of one steam-ship to the paddle wheel of another, or as the latter to a carriage wheel, and can be just as rationally supposed to be not a new instrument but the old one changed.

"Again, our reptiles of the coal do not constitute a continuous series, nor is it possible that they can all, except at widely different times, have originated from the same source. To suppose that *Hylonomus* grew out of *Dendrerpeton* or *Baphetes*, and *Eosaurus* out of either, startles us almost as much as to suppose that *Baphetes* grew out of *Rhizolus*, or *Hylonomus* out of *Palæoniscus*. It either happened, for some unknown reason, that many kinds of

fishes put on the reptilian guise in the same period, or else the vast lapse of ages required for the production of a reptile from a fish must be indefinitely increased for the production of many dissimilar reptiles from each other; or, on the other hand, we must suppose that the limit between the fish and reptile being once overpassed, a facility for comparatively rapid changes became the property of the latter. Either supposition would, I think, contradict such facts bearing on the subject as are known to us.

"We commenced with supposing that the reptiles of the coal might possibly be the first of their family; but it is evident from the above considerations, that, on the doctrine of natural selection, the number and variety of reptiles in this period would imply that their predecessors in this form must have existed from a time earlier than any in which even fishes are known to exist, so that if we adopt any hypothesis of derivation, it would probably be necessary to have recourse to that which supposes at particular periods a sudden and as yet unaccountable transmutation of one form into another—a view which, in its remoteness from any thing included under ordinary natural laws, does not materially differ from that currently received idea of creative intervention with which, in so far as our coal reptiles can inform us, we are for the present satisfied."

* * * * *

"Humble though the subjects of this paper are, we see in them the work of Supreme Intelligence, introducing new types upon the scene, and foreshadowing in them those higher forms afterward to be created. It is this, their Divine origin, and the light which they throw on the plan and order of the creative work, of which we ourselves form a part, that gives them all their interest to us. They are the handiwork of our Father and our God, traces of His presence in primeval ages of the earth, evidences of the unity of His plan and pledges of its progressive nature, adding their feeble voices to the testimony of revelation in respect to the history of creation in its earlier stages, and to the carrying on of that plan which still involves the extinction of many things from the present world, and the elevation of others into new and glorious manifestations. Their place in the system of nature and in the order of the world's progress, their uses in their own time, and their relations to other beings as parts of the great cosmos, are the points that chiefly interest us; and if any one desires to understand more in detail how they were created, we wish him all success in his inquiries, but warn him not to suppose that this great mystery is to be solved by a reference merely to material agencies, apart from that Spiritual Power who is the essence of forces, the origin of laws."

The First Principles of Natural Philosophy. By W. T. LYNN. London: Van Voorst, 1863.

This is in many respects a creditable little work—totally distinct in kind from the peculiar absurdities produced by the Reddie and Worms school of self-constituted philosophers.

To some of its errors, which, we must say, are attributable to those of more pretentious treatises, we shall presently advert. But we allow that the author expresses himself well and clearly on many points, and that his treatise is so far what it was intended to be—a *very* simple guide to the first elements of Mechanics and Optics.

In some cases he has shown considerable acuteness, though only brought into action by his want of knowledge. Thus his remark, p. 40, about *intensity* of motion, though quite wrong, shows that he has an idea of *Vis viva* as distinguished from Momentum.

The author has, however, followed too closely authorities whose value is really much less than is generally believed; such as the bulk of the Cambridge and Dublin Text-Books. Most, we may almost say all, of these—are admirable in their mathematical development: but there is, in general, a fearful amount of looseness, and even inaccuracy, whenever first principles or *experimental* results are concerned.

Thus Mr Lynn says (p. 20),

“It is at once apparent that if a force acts continuously on a body, it will generate in that body a continually increasing motion.”

He has not, so far as we can see, added the proviso that the force must *not* act at right angles to the direction of motion.

Again (in pp. 20, 22) he speaks of a *constant*, as distinguished from an *impulsive*, force—meaning by the erroneous first term a *continuously acting* force. He seems also (in p. 23, for instance) to fancy that an accelerating force must be of constant magnitude. In the same page, after giving an exceedingly good and complete investigation of the space described from rest by a body in any time, when acted on by a uniform force in its line of motion, he seems to be dissatisfied with his work—for he goes on to say,

“This proposition is so important and fundamental that it will be proper to give a strict demonstration of it.”

Then he goes on with limits, and limiting ratios, as if to make doubtful to the beginner, in 4 pages, what he has just before satisfactorily proved (by general reasoning) in 8 lines.

§ 41, as it stands, is simply untrue. It requires most copious and careful limitations, which are not even hinted at in the text.

In § 45 we find another of these cases of rash assertion (about the attraction of spheres)—requiring a great amount of unsupplied qualification.

To mention but *one* more serious blunder—we find that *water is a non-elastic fluid!* (p. 47.) Shades of Canton and Oerstedt, have your discoveries come to this? Is not elasticity the tendency of a compressed or distorted body to recover its volume or form when the disturbing cause is removed? Is not water compressible and *perfectly elastic*?

The Philosophy of Geology: a brief Review of the Aim, Scope, and Character of Geological Inquiry. By DAVID PAGE, F.R.S.E., F.G.S. 12mo. Pp. 155. William Blackwood and Sons, Edinburgh and London, 1863.

The object of this work is to direct attention to some of the higher aims of geological science, to the principles which ought to guide geologists in their generalisations, and to what may be ultimately anticipated of geology in her true and onward progress. "The philosophy of geology is the study of the structural arrangement and composition of this earth, the causes that have produced this structure and arrangement, the laws by which this causation is upheld, and the comprehension of the whole in time as constituting a continuous and intelligible world history." The author first discusses the objects of inquiry, defines what geology is, and shows that speculations as to the origin of the earth are inadmissible. "Geology is but the physical geography of former ages. Every rock system retains some evidence of the conditions of its period; and the determination of these conditions, the causes that produced them, and the life by which they were accompanied, is the spirit and purport of geology." Certain forces have been called into play, in order to produce geological changes on the globe, and these forces act according to uniform natural laws, so that we are enabled in some measure to argue from the present as to what took place in the past. At the same time we must bear in mind that over limited areas cataclysmal phenomena occur, depending on earthquakes, volcanoes, or floods, which may interfere with our reasonings, and which show the necessity of being cautious in our inductions. We must beware of the error of those who say that all things continue as they were from the creation, and that nothing has been done *per saltum*. Floods and convulsions have occurred, and local phenomena may present

themselves which we cannot account for, and which, in the meantime, we must be content to leave unexplained.

In referring to theories of the earth, the author remarks :—

“The philosophy of our science is thus neither to ignore nor reject hypotheses, but merely to receive them as tentative processes and provisional aids towards the attainment of veritable theory. . . . Granting the value of hypotheses in occasionally directing the way to a solution of our problems, it cannot be denied that indulgence in speculation has been the great bane of geology. . . . The earlier progress of geological inquiry was encumbered by its absurdities, which would have been simply ridiculous but for the discredit they attached to the science, and the obstacles they threw in the way of its acceptance. There is nothing so easy as generalisation, where the facts are few; nothing more difficult than adherence to a course of induction where the field of observation is wide, and the facts numerous and complicated. On this ground the many ‘theories of the earth’ that prevailed towards the end of last century and the beginning of the present, are in some degree excusable; but now that geology has taken her proper place among the natural sciences, all such attempts should meet with the most steadfast discountenance and reprobation.”

The subjects of the Direction of Drifts, Chemical Formation, Metamorphism, Mineral Veins, &c., are discussed. The author points out that in regard to them there are many difficult problems in geology still to be solved. The question of Time as a factor in geology is considered; and it is shown that while we admit a prodigious amount of geological time, we have no means of approximating to its duration. Calculations founded on the deposit made by rivers of the present day are very fallacious. All that can be done at present is to arrange the stratified formations into systems, groups, and series, each section representing a relative but inadequate amount of time, but occupying its own proper chronological position. A proper nomenclature and classification of formations is an important object, and in this point of view palæontology has done great service. At the same time, there can be no doubt that much remains to be done in these departments. Geologists have often drawn very rash conclusions from fossil specimens, both as regards their nature and the indications which they afforded of soil and climate. A deficient knowledge of the zoology and botany of the present epoch is one great cause of geological error. The following remarks of Mr Page deserve attention :—

“While we must admit the vast benefits conferred on our science by palæontology, let us take care that we are not misled by the dicta of its earlier cultivators into beliefs which are at variance with the known principles of biology and physical geo-

graphy, and which extended observation in geology refuses to confirm. As a general maxim, it is true that the most trustworthy tests of the sequence of the stratified systems are the fossil organisms which these systems contain. It is also true, that over limited tracts like Britain, or even over the area of Europe, contemporaneous deposits are characterised by the same fossil species; but it may not be true that the same species never recur at more stages than one in the same formation, nor may it be true that formations containing the same species in widely separated regions are strictly contemporaneous. The truth is, there has been a tendency of late to carry the argument of fossil evidence beyond its legitimate limits. Under the change of external conditions, a species—say a marine one—may gradually shift its ground many degrees south or north, and so become extinct in its original area, and yet, after hundreds of feet of sediment had been deposited on that area, a reversal of conditions may occur, and establish the species once more on its primal habitat. We would thus have the same species occurring at two different stages of the same great formation, separated, it may be, by some thousands of years in time, though only by a few hundred feet of sediment. Again, under the slow oscillation of sea and land, species terrestrial and marine may have been gradually transferred from the latitudes and longitudes of Europe to the latitudes and longitudes of America; and thousands of years after they had become fossil in the eastern hemisphere, they may have been flourishing in the western."

After considering the appearance of life on the globe, the author calls attention to the theory of Progression, and more especially discusses the Darwinian views as to external condition, embryonic phases, use and disuse of organs and natural selection, and he concludes that all these "are but subordinate factors of one great law, and that we must know much more of the forms that have become extinct, and more of the variations that are now taking place in existing plants and animals before we can hope to approach the solution of the all-important problem."

The origin of man naturally becomes a matter of consideration. On this point the author says:—

"Whatever be the plan of development, it must of necessity embrace the whole scheme of vitality. There can be no severance in the great creational idea of life; and whatever theory be adopted, it must be applicable alike to every constituent member of the system. The highest as well as the lowest, man as well as the monad, forms part and parcel of one continuous evolution; and whatever the ordainings of the past, they exist in the present, and must operate in the future. If by any genetic law the radiata have given birth to the articulata, the articulata to the mollusca, and the mollusca to the vertebrata—nay, were it only the great

sections of the vertebrata that were so genetically connected—the fishes giving rise to the reptiles, the reptiles to the birds, and the birds to the mammals,—still this would be sufficient to prove man's inseparable association with the same scheme of development. Whatever may be the law that determines the origination of other species, to the same law must we ascribe the origin of man. Philosophy has no alternative. Science has nothing to gain, but everything to lose, by the adoption of any other opinion. We must look, therefore, for man's precursor in the order that stands next beneath him in the zoological scale; and wide as the gap may appear, we may suppose it either bridged over by intermediate forms that became extinct during the tertiary period, or, if the rate of ascent be more rapid in the higher than in the lower orders, to have been passed over *per saltum*, or at most by the intervention of very few such intermediate species. But though one form be descended from another—the higher from the next lower in the scale of creation—such descent differs widely from that of ordinary generation, inasmuch as qualities unknown in the lower form begin to manifest themselves in the higher. Whence then, these newer qualities and higher functions? Clearly, not from the predecessor, who did not possess them; not from the law, which is simply a mode of operation; but from the Lawgiver, who ordained and continues to sustain the method of development. Similar as the framework of the monkey may be to that of man—nay, were it a hundred times more closely resembling—yet every superaddition of reason, gift of speech, moral feeling, and religious sentiment in man, is in reality a new creation—a creation as special as if it had proceeded from the audible 'Let it be' of the Creator. To the devout and philosophic mind the secondary law of causation is the great 'Let it be,' ever ringing through nature as audibly as on the morning of its primal utterance."

Our author, therefore, is opposed to the views of those who think that man is merely an ennobled ape, and that he has a Simian origin. The advocates of this opinion say, that because the lowest ape does not differ from the highest ape in the conformation of its skeleton more than does the highest ape from man, therefore man was originally an ape. This, in our opinion, is a complete *non sequitur*. These statements would merely show that the Creator, in forming animals and man, followed a great type, and that out of a small amount of materials He moulded all the varied forms seen in the animal creation. It shows unity of design and wonderful wisdom. But animals have other characteristics than their bony skeleton affords, and so has man. We must consider their anatomy in a physiological point of view, and then we shall at once see marked and evident distinctions.

On the question of the Antiquity of Man, our author, while he

does not agree with Lyell as to the enormous lapse of time since man's appearance on earth, is disposed to believe that man is older than is generally allowed in the received chronology.

"Admitting the same genetic law for the human race as for the rest of animated nature—and, philosophically speaking, there is no other course left for the adoption of science—the question next arises, At what period of the geological scale did man make his appearance? . . . So far as research has been prosecuted in the different quarters of the globe—and at the outset it must be confessed how insignificant the area that has been examined—no remains of man or of his works have been discovered till we come to the lake-silts, the peat-mosses, the river-gravels, and the cave-earths of the Post-tertiary period. In these have been detected tree canoes and stone hatchets, rude implements of flint and bone, the embers of the fires he kindled, and occasional fragments of his own skeleton. As yet these have been chiefly discovered within the limited area of southern and western Europe, and we have scarcely any information from the corresponding deposits of other regions. Till these other regions shall have been examined—and especially Asia, where man historically flourished long prior to his civilisation in Europe—it were premature to hazard any opinion as to man's *first* appearance on the globe. But taking the facts such as geology finds them—viz., the occurrence of stone implements in conjunction with the remains of Irish-deer, mammoth, hippopotamus, rhinoceros, cave-lion, and other creatures long since extinct in Europe, and this in deposits of considerable geological antiquity—it is evident that man has been an inhabitant of the globe much longer than is popularly believed. It is true that the antiquity of some of the containing deposits, especially the river-drifts, is open to question, and it is also quite possible that the remains of the extinct quadrupeds may in some instances have been reassorted from older accumulations; but even allowing for these, geologists have sufficient in the valley-deposits of France and England, the caves of southern and western Europe, and the lake-silts of the same area, to convince every mind capable of appreciating the evidence, that mankind has existed even there (to say nothing of Asia) long antecedent to the time assigned by the patristic chronologers as the date of his creation.

"But while the nature of the deposits, their situation, and their mode of formation, indicate the lapse of many thousand years (estimating by the usual modes of geological computation), we must be careful not to run into the opposite extreme, and conjure up ages of fabulous duration. Historically we have no means of arriving at the age of these deposits; geologically we can only approximate the time by comparison with existing operations; while palæontologically it must be borne in mind that the

associated animals are among the most recently extinct or exterminated. It is a sound maxim in palæontology, that the more ancient any specific form is, the more widely it differs from existing species of the same genus. Structural variation is, in fact, the measure of antiquity.

“ In dealing, then, with the antiquity of man, we have both its lithological and palæontological, and to some extent also its historical, aspects to consider, before we can arrive at any truly philosophical conclusion. Lithologically, we see that great changes have taken place in the physical geography of the districts where human remains have been discovered, and the deposits in which they occur are frequently also of considerable thickness, and of a nature that implies slow and gradual accumulation. As geologists, we may feel convinced that more than six or eight thousand years have elapsed since their formation, but how much more, we have, in the present state of our science, no means of definitely determining. Palæontologically, we perceive that other animals whose remains are associated with those of man do not differ very widely from species still existing, and are therefore constrained to oppose that enormous antiquity which some geologists are disposed to contend for. Historically, all is silent on the subject of these remains; but while the mammoth and rhinoceros may have died out of Europe within the last five or six thousand years without attracting the notice of the rude inhabitants, the present state of human civilization seems incompatible with such a brief period as five or six thousand years for its development.”

We certainly must differ from our author as to the antiquity of man. Whatever may be said in regard to animals and plants, we can have no doubt as to the creation of man, and we have scriptural data which certainly seem to fix his appearance on earth within tolerably definite limits. We think that geologists are completely at sea on this subject, and want data on which to found their conclusions. The author recommends in other parts of his work cautious induction and a sifting of facts, and we would apply to him in this case the exhortation which he gives to others. No doubt he does not give a decided statement,—his trumpet here gives an uncertain sound, and he feels the necessity of treading warily on ground which may ere long be found to yield under his feet. Those who advocate progressive development, and the transmutation of the ape into man, find great difficulty in setting aside the statements of the Bible as to man's creation, and hence they are glad to lay hold of any speculation as to man's antiquity which they think will throw discredit on the sacred volume, and put it out of the category of a Divine and truthful record. Hence the avidity with which the so-called facts as to the antiquity of man are seized hold of. Already, however, the Darwinian hypothesis has been attacked by able naturalists and geologists such as Flourens and

Dawson, and the foundations on which the Lyellian speculations as to man have been based are tottering. There is great doubt and uncertainty among geologists, and we are satisfied that much remains to be done. At the same time we feel sure, that, as in all former cases, Science and Revelation will be found not to be at variance, and the attempt to throw distrust on the Bible record will fail.

The future of this planet is considered by Mr Page in his work, and he refers to the changes which may be expected to occur ere this state of things comes to an end, and a new race of beings people the earth. As to the future of this world "The Philosophy of Geology" cannot give us information; but we know from an unerring testimony that "the heavens and the earth which are now, by the same word are kept in store, reserved unto fire against the day of judgment;" and that "the heavens shall pass away with a great noise, and the elements shall melt with fervent heat, the earth also and the works that are therein, shall be burned up;" and that we are to "look for new heavens and a new earth, wherein dwelleth righteousness." (2 Peter iii.) Here, then, is a prophesied cataclysm, a *per saltum* occurrence totally inconsistent with the progressive-development theory, the supporters of which tell us that "no cataclysm has desolated the whole world, and that we may look with some confidence to a secure future of equally inappreciable length," in which, "judging from the past, we may infer safely that not one living species will transmit its unaltered likeness to a distant futurity."

We have perused Mr Page's clear and interesting volume with much interest. It gives a comprehensive view of the actual state of the science of geology, and contains valuable suggestions as to the future prosecution of it. It brings before the student the difficulties which stand in his way—the danger of rash speculation—and the need of accurate data, before drawing conclusions regarding the lithological and palæontological history of our globe. Differing as we do from the author in some particulars, still the cautiousness with which he makes his statements in regard to controverted points, will, we doubt not, show to the young observer that he must not be led away at once by the theories of those geologists who have shown a disposition to dogmatise on these matters. We are all too apt "*jurare in verba magistri*," and anything which makes us doubt the theories given *ex cathedra*, may be useful in making us sift the points for ourselves and investigate fully the data whence conclusions may have been rashly drawn. We must examine carefully and observe accurately before we can theorize. We think that the book will be valuable to young and local observers, by showing them the work to be done, the mode in which it is to be done, and the doubtful speculations which beset their path.

PROCEEDINGS OF SOCIETIES.

Botanical Society of Edinburgh.

Thursday, 12th November 1863.—Professor MACLAGAN, President,
in the Chair.

His Royal Highness Prince Alfred was elected by acclamation an
Honorary Fellow of the Society.

The President delivered an opening Address.

The following Communications were read :—

I. *Notes on the Fertilisation of Orchids.* By WM. RUTHERFORD, M.D.
(This paper appears in the present number of this Journal.)

II. *Synopsis of Canadian Ferns and Filicoid Plants.* By Professor
LAWSON.
(This paper appears in the present number of this Journal.)

III. *Notes on some New and Rare British Mosses, and on the Occurrence
of Trichomanes radicans in the Island of Arran, Firth of Clyde.*
By Mr JOHN SADLER.

Bryum Duvalii, Voit.

Whilst lately engaged examining the genus *Dryum* in Dr Greville's
collection of *Musci*, recently added to the University Herbarium, I came
upon two barren specimens marked "*Bryum turbinatum*, Ben Voirlich,
1823." On close examination, I found them to be the true *Bryum
Duvalii* of Voit. The specimens were collected by Dr Greville himself;
thus he is the discoverer of the plant in Britain, having gathered it fully
thirty years before the late Colonel Madden met with it near Waterford
in Ireland, as recorded in the Society's Transactions, vol. vii. p. 6. The
following are the known British stations for this moss :—

1. Ben Voirlich, 1823. Dr Greville.
2. Near Waterford, Ireland, 1852. Colonel Madden.
3. Hart Fell, near Moffat, 1858. Dr William Nichol.
4. Helvellyn, 1859. Mr John Nowell.
5. Ben Lawers, 1860. Mr William Bell.

Mnium stellare, Hedw.

On the Ochils, near Bridge of Earn. August 1863. J. S.
Grimmia leucophaea, Grev.

Rocks between Kinghorn and Pettycur. June 1863. J. S.
Schistidium maritimum, B. et S.

Rocks between Kinghorn and Pettycur. June 1863. J. S.
Didymodon recurvifolius, Tayl.

Ben Voirlich. 1863. Mr M'Kinlay.
Dicranum circinatum, Wils.

Ben Voirlich. 1863. Mr M'Kinlay.
Glyphomitrium Daviesii, Schwæg.

Bowling Glen. May 1863. Mr Galt.

In a letter which I lately received from Mr John Robertson, Glasgow, he says:—"I have been to Glen Turret and Ben Chonzie, and the following are some of the few rarities I collected. A solitary tuft of *Tortula tortuosa* in fruit, on Ben Chonzie; *Bryum roseum*, but not with capsules. In a fruiting state I found *Oligotrichum hercynicum*, *Tetraplodon mnioides*, *Splachnum ampullaceum*, *S. sphaericum*, *Encalypta ciliata*, *Climacium dendroides*, &c."

Mr Sadler exhibited specimens of *Trichomanes radicans* which he had received from Mr Walter Galt, Glasgow, accompanied by the following note, dated 26th August 1863:—"I enclose you fronds of *Trichomanes radicans*, collected by Mr George S. Combe in the Island of Arran, Firth of Clyde. It occurs in two separate patches, one of which is about 3 feet square, seemingly a natural habitat."

Thursday, 10th December 1863.—Professor BALFOUR, President,
in the Chair.

A letter was read from Major Cowell, conveying the thanks of H.R.H. Prince Alfred for his election as an Honorary Fellow of the Society.

I. *Notice of the Occurrence of Polypodium calcareum, near Aberdeen.*
By Mr JAMES ROBERTSON.

Mr Robertson states that he had discovered this plant in August 1862, growing in the debris of a limestone quarry in Scotston Moor, near Aberdeen, along with *P. Dryopteris*. He was disposed to look upon the plant as being wild in that locality. Professor Dickie, however, believes that it has been introduced, and he has learned that a gentleman's gardener in the neighbourhood was in the habit of planting ferns in waste places. Specimens of the plant were exhibited from the Scotston station.

II. *Account of the Vegetation of the Cliffs of Kilkee, County Clare, Ireland.* By N. B.-WARD, Esq.

In compliance with Professor Balfour's request, I send a brief account of the vegetation of the Cliffs of Kilkee, and its neighbourhood, which I visited last summer, in company with Professor and Mrs Harvey, and an old friend, Mr Snell, with my daughter. During our stay, we visited Loophead, at the mouth of the Shannon, and an intermediate portion of the cliffs, on which Baltard Castle is situated. Five days were spent at Kilkee, one at Baltard Castle, and one at Loophead. The vegetation at the three places was so perfectly identical, as to lead us to the conclusion, that the same geological structure prevailed throughout, consisting of hard grits, shales, &c., a conclusion which was confirmed by a subsequent visit to Mr Jukes of Dublin.

Kilkee is situated on the west coast of Ireland, exposed to the tide force of 2000 miles of unbroken seas,—the waves of which roll in with such power, as to furnish abundant food to periwinkles, located on rocks 200 feet above high water-mark, and to supply the wants of marine plants which cover the summits of cliffs, varying in height from 150 to 400 feet. That physiological law by which plants, under adverse circumstances, produce their flower and fruit, if they can do nothing else, is here strikingly exemplified. Looking at the stunted character of the vegetation, one might imagine oneself in a high alpine region—many species

not attaining more than a tenth or twelfth of their usual size. Thus we find here a number of plants, simulating the appearance of the inhabitants of alpine regions—e. g., *Aster Tripolium*, in full flower, from half an inch to an inch and a half in height; and equally stunted, not starved, forms of *Samolus Valerandi*, *Euphrasia officinalis*, *Jasione montana*, *Erythræa Centaurium*, *Ranunculus Flammula*, &c. The higher portions of the cliffs are cushioned by continuous tufts of the common thrift, *Armeria maritima*, which, when in full flower, must be exceedingly beautiful. The lower grounds are carpeted by *Anagallis tenella*, *Ranunculus Flammula*, *Hydrocotyle vulgaris*, &c., whilst the hill sides are dotted with generally solitary plants of *Erythræa Centaurium*, the broad-leaved variety; the little ridges towards the sea are lined with *Glaux maritima*, the drier spots being covered with *Radiola Millegrana*, &c.

The following is a list of all the plants which were seen in flower:—

Ranunculus Flammula, in great abundance in wet places, towards the lower portion of the cliffs, in company with *Anagallis tenella*, *Juncus bufonius*, &c.; *Silene maritima*, *Spergularia rubra*, var. *marina*; *Radiola Millegrana*, on little elevated ridges; *Polygala vulgaris*, *Potentilla Tormentilla*, *P. anserina*, *Sedum anglicum*, the great ornament of exposed and bare rocks here and in many other parts of Ireland, from the beauty of its flower and the rich decaying tints of its foliage; *Aster Tripolium*, many specimens in full flower, not an inch in height; *Bellis perennis*, *Achillæa Millefolium*, *Senecio Jacobææ*, *Carduus pratensis*, *Leontodon autumnale*, *Calluna vulgaris*, very sparingly; *E. Tetralix*, very sparingly; *Jasione montana*, *Campanula rotundifolia*, *Glaux maritima*, coating the seaward face of the high ridges; *Anagallis tenella*, everywhere in moist places; *Samolus Valerandi*, *Euphrasia officinalis*, rarely exceeding half an inch in height; *Erythræa Centaurium*, *Thymus Serpyllum*, *Armeria maritima* cushioning the whole of the upper surface of the cliffs; *Plantago maritima*, *P. Coronopus*, *Juncus bufonius*, *Luzula campestris*, *Carex flava*, *Aira caryophyllea*, withered; *Melica cærulea*, very sparingly. The paucity of grasses was remarkable.

III. Notice of the Discovery of *Fucus distichus*, L., at Duggerna, County Clare, Ireland. By Professor HARVEY.

In a letter to Dr Greville, Professor Harvey says:—

"In a summer excursion to Kilkee, last July, in company with N. B. Ward, we found what I take to be the true *Fucus distichus* of Linnæus. I have no authentic specimen of the Arctic plant, nor have I seen one, but the specimens exactly agree with the description of authors, as well as with the figures, though none of the latter do this plant justice. I enclose specimens for your herbarium. Unfortunately we were rather late in the season, and the fruit had mostly dropped off, leaving truncated branches. Some, however, were in fruit. I suppose it is in perfection either in winter or spring, and I mean, if I can manage it, to visit the station next Easter.

"It grows on a remarkable rock facing the sea, near low water mark, but rising much above low water level—that is to say, the rock is at the outer edge of a long reef, but rises above the reef level. The fucus grows in patches on little ledges of the perpendicular side of the rock, along with *Gigartina mamillosa*, &c. It has quite a peculiar aspect when growing. The stipes or base of stem is thick and rigid, and stands erect, while the fronds are just sufficiently limber to bend over, but not to lie flat, so that the patch looks like a miniature grove of weeping willows."

In the University Herbarium (which now contains Dr Greville's algae) there are specimens of *Fucus distichus* from Faroe, sent by Professor Hornemann, and from Newfoundland, sent by Bory in 1831, which appear to correspond with the Irish specimens.

IV. Notice of the Occurrence of *Sagina nivalis* (Lindblom), on Ben Lawers. By Professor BALFOUR.

In October last, Mr J. T. Boswell Syme, wrote to me in the following terms:—"Will you be so kind as to look in your herbarium under *Alsine rubella*, to see if you have got specimens of *Sagina nivalis*, Lindbl. Under this name, I have this plant with the following label from the Edinburgh Botanical Society, '*Alsine rubella*, Ben Lawers, Perthshire, Aug. 25, 1847. Dr Balfour.' I have come to the genus *Sagina* for the new edition of English Botany, and I feel great doubts as to whether or not I should include *S. nivalis*. Babington mentioned it in the third edition of his Manual, but omitted it from the fourth and fifth." On examining the plants in my herbarium and my duplicates, I found that three specimens of *Sagina nivalis* were fastened down on a sheet along with specimens of *Alsine rubella*. The specimens of the former were put together and quite distinct from the latter, implying that I had looked upon them as peculiar. They are marked 'Ben Lawers, 1847,' and they were gathered by me on the 25th August. There were only a few pupils with me, viz., Mr Charles Murchison, Mr F. J. Ivory, Mr Gilby, Mr Hewitson, Mr Hugh Balfour. Among my duplicates I could only detect two specimens remaining, one of which I sent to Professor Babington. I have no doubt that there are others which have been distributed, as a number of duplicates were contributed by me to the Society. In my notes of the excursion, I refer to our getting *Alsine rubella*, and from the indication given, I think that I know the particular locality. Babington refers to the plant as having been gathered on Glassmeal, by Mr Backhouse; and Hooker and Arnott, in the eighth edition of their "British Flora," mention *Sagina nivalis* as found in the Isle of Skye and Clova. They say that the plant is distinguished from *S. subulata*, by being almost quite glabrous. It is possible that their plant may be *S. saxatilis*. The genera *Alsine* and *Sagina* are very nearly allied. The chief characters are derived from the styles and valves of the capsule, which in *Alsine* are usually three, while in *Sagina* they are four or five. In *Alsine rubella* the sepals are distinctly 3-nerved, whereas in *Sagina nivalis* they are obscurely 1-nerved. By this character the plants can be easily separated.

I now show the original specimens gathered by me on Ben Lawers on 25th August 1847.

The following are the characters of the plant as given by Fries:—*Sagina nivalis*, Lindbl. Caulibus cespitosis, foliis subulatis mucronatis glabris, pedunculis brevibus strictis, sepalis quinque ovatis obtusis membranaceo-marginatis petala integra vix æquantibus. Lindbl. "Bot. Not." 1845, p. 66; Fries, "Summa Veg. Scand.," 156; Fries, "Nov. Mant." iii. 31; *Sagina intermedia*, Ledebour "Flora Rossica," i. 339; *Spergula saginoides*, *S. nivalis*, Lindbl. in "Phys. Sallak. Tidskr.," 1838, p. 128; *Arenaria cespitosa*, "Fl. Dan.," t. 2289.

The plant is found in moist places of the Dovrefeld. Gathered by Lindblom at Sprenbacken in Kundshö, above Kongsvold, and at Drivelven. It is a perennial, and flowers in August and September. The discoverer

of this plant states that it bears the same relation to *Sagina saxatilis*, that *Sagina stricta* bears to *S. procumbens*. The species exhibits two forms, one congested and erect, the other lax, with elongated procumbent stalks. The petals are entire, while in *Sagina saxatilis* they are slightly emarginate.

Babington states that he has specimens from Fries in his Herb. Norm. (xii. 51) gathered by Blytt in the Dovrefeld in Norway. He also says: "It is to be remarked that Blytt finds it in Norway, Fenzl has it from the extreme north-east of Siberia, and *Flora Danica* from Greenland."

V. *Remarks on Monoecious Spikes of Maize.* By Mr JOHN SCOTT.

(This paper will appear in next number of the Journal.)

VI. *On the Cultivation of the Quiniferous Cinchona in British Sikkim.*

By Dr THOMAS ANDERSON, Superintendent of the Botanic Garden, Calcutta.

The cultivation of *Cinchona* at Darjeeling, has been carried on successfully. The following is a return of the *Cinchona* plant in the nurseries at that place, on the 15th June 1863:—

<i>Cinchona succirubra</i> , ...	1024
<i>C. Calisaya</i> , ...	53
<i>C. officinalis</i> , ...	573
<i>C. micrantha</i> , ...	695
<i>C. pahudiana</i> , ...	2275
Total, ...	4620

The cultivation of *Cinchona* at Darjeeling was attended with very great difficulties at first; but these have now been overcome, and there is every reason to believe that the plantation will be successful.

In the commencement of June 1863, I supplied Dr Simpson, the European Civil surgeon of Darjeeling, with about two lbs. of fresh leaves of each of the following species: *C. succirubra*, *C. officinalis*, and *C. micrantha*. Decoctions prepared with water slightly acidulated with sulphuric acid, were very bitter, and three patients suffering from well-marked intermittent fever were cured by the administration of these preparations alone. Towards the end of June Dr Simpson and I endeavoured to examine chemically the nature of the leaves of *Cinchona succirubra*, and detected quinine in them.

VII. *On the Cultivation of Tea in India.* By WILLIAM JAMESON, Esq., Surgeon-Major, Superintendent of the Botanic Garden, Saharanpore.

In a former communication I estimated the quantity of waste and other lands fitted for cultivation with Tea, throughout the Kohistan of the North-Western Provinces, and Punjab, and Dhoons, and showed that by them the enormous quantity of 385,000,000 lbs. might be there raised. But in this estimate I excluded the Kohistan of Huzarah and Rawul Pindee, of Cashmere, Jummoo, and the protected Seikh States. The following estimate of the yield of the British territory is nearer the mark,

and as a general return when in full bearing, 100 lbs. per acre may be given :—

	Acrea.	100 lbs. per Acre.
Kohistan of Rawul Pindee and Huzarah, ...	20,000	2,000,000
Kangra Valley, ...	35,000	3,500,000
Kazloo, ...	35,000	3,500,000
Munndee, &c., ...	40,000	4,000,000
Protected Hill States, ...	10,000	1,000,000
Jonsar Bawer, ...	10,000	1,000,000
Dehra Dhoon, ...	100,000	10,000,000
Western Gurhwal	180,000	18,000,000
Kumaon, ...	3,500,000	350,000,000
	3,930,000	393,000,000

—a quantity nearly equal to the whole export trade of China, and with high cultivation the figures might easily be doubled, and thus not only allow an immense quantity for the consumption of the Indian community, but at the same time afford a vast supply for export to other countries.

In February last, at the request of the Lieutenant-Governor of the Punjab, I proceeded to the Kohistan of the Rawul Pindee Districts and Huzarah, there to establish the Tea plant, which has been most successfully done—the plants removed from the Kangra Plantations, and transplanted at Seelah, now growing with vigour.

It is no longer an experimental Tea cultivation in the North-Western Provinces, it having passed from experiment to fact. It has been proved by data which cannot be gainsaid, that the cultivation of the Tea can be profitably conducted; that the Tea prepared is admirably fitted for the Home and Indian markets; and that, if properly conducted and backed by capital, the undertaking presents a safe and profitable investment.

VIII. On some Economic Plants of India. By Dr HUGH F.C. CLEGHORN.

1. The Box Tree (*Buxus sempervirens*).—This tree, grown at Koolor, has been tested by Dr Alex. Hunter, at the Madras School of Arts, and the wood is found valuable for engraving.

In Mr M'Leod's arboretum at Dhurmsalla the tree grows well. The arboretum contains many introduced Himalayan trees of great interest, as well as many European fruit trees adapted to this hill station. It is, perhaps, the only collection of indigenous alpine trees in the Punjab, the nearest to it being that of Mr Berkeley at Kotghur. I hope the day is not far distant when the Punjab Agri-Horticultural Society will have a hill garden associated with it at one of the sanatoria of the province.

The Himalayan box appears to be identical with the tree common all over South Europe, from Gibraltar to Constantinople, and extending into Persia. It is found chiefly in valleys at an elevation of 3000 to 6000 feet. I have met with it from Mount Tila, near Jhelum, to Wangtu bridge on the Sutlej. It is variable in size, being generally 7 to 8 feet high, and the stem only a few inches thick, but attaining sometimes a height of 15 to 17 feet, as at Mannikarn in Kullu, and a girth of 22 inches as a maximum. The wood of the smaller trees is often the best for the turner and wood-engraver. It is made into little boxes by the villagers for holding ghee, honey, snuff, and tinder. At the medical stores in Sealkote it is

turned into pill-boxes, and it appears to be adapted for plugs, trenails, and wedges. The wood is very heavy, and does not float; it is liable to split in the hot weather, and should be seasoned, and then stored under cover.

2. The Olive, *Zaitoon*, which has also been tested for wood-engraving at the Madras School of Arts, is another plant of the Mediterranean flora, which ranges from the coast of the Levant to the Himalaya. It varies a good deal in the shape of its leaves and in the amount of ferruginescence, hence the synonyms *cuspidata* and *ferruginea*, but it does not appear to differ specifically from the *Olea europæa* (Mount of Olives), the emblem of peace and plenty. The finest specimens I have seen are in the Kaghan and Peshawur valleys, where the fruit resembles that of rocky sites in Palestine or Gibraltar. The wood is much used for combs and beads—and is found to answer for the teeth of wheels at the Madhopore workshops.

3. *Urtica heterophylla*, a kind of Indian nettle, is plentiful in Simla, having followed man to the summit of Jako, attracted by moisture to an elevation unusual for any member of the family. It is found within the stations of Dalhousie and Dharmasalla, and at many intermediate points. The quantity is surprising wherever the soil has become nitrogenous by the encamping of cattle. The growth at this season (July) also is luxuriant in shady ravines near houses, where there is abundance of black mould; but the sting being virulent, the plants are habitually cut down as a nuisance, both by private persons and municipal committees.

There are other plants of the nettle tribe, particularly the *Boehmeria salicifolia*, "siharu," used for making ropes (to which attention has been directed by Dr Jameson); this plant does not sting, and is abundant at low elevations. The produce of this might be turned to good account, though not yet recognised as merchantable fibre.

4. *Cultivation of Bamboo*.—Mr M^rLeod, Financial Commissioner in the Punjab, writes thus to the Commissioners of Umballa and Jullundhur:—

"As it is desired to extend the growth of the bamboo as widely as possible throughout the Punjab, and some of the districts of your division possess them in greater or less abundance, I have to request that you will ascertain whether any of the four following varieties have borne seed during the present year, and inform me of the result of your inquiries.

1. The hollow Bamboo of the plains.

2. Solid Bamboo of the lower hills, of which spear handles and clubs are usually made.

3. The Nirgali or small Bamboo of the hills, growing at elevations from 5 to 8000 feet.

4. The Garoo, or still smaller hill Bamboo, growing at higher elevations, probably up to 12,000 feet.

"It would be interesting also to ascertain, if possible, from the people, the intervals which lapse between the seasons of flowering of the several varieties. A point on which the more observant might readily furnish information, as, after flowering and yielding seed, the entire tract of bamboo which has seeded, simultaneously dries up and perishes, fresh plantations springing up from the seeds which have been scattered by the old stock.

IX. Dr Alex. Hunter, Secretary of the Agri-Horticultural Society of Madras, transmitted reports as to the cultivation of Peruvian cotton at Chingleput by Dr Shortt, and in the Kistna District by Mr E. B. Foord. Both reports are satisfactory. The following is a statement which he also transmitted:—



Statement showing the Quantity of Cotton carried on the Madras Railway in the Years 1861-62 up to June 1863.

	1861. Indian Maunds, or 82 2-7 lbs.		1862. Indian Maunds, or 82 2-7 lbs.		1863. Indian Maunds, or 82 2-7 lbs.	
January, ...	6,846	10	694	0	7,886	10
February, ...	10,519	10	3,535	30	10,305	...
March, ...	6,134	15	3,346	10	4,471	30
April, ...	1,390	30	5,466	30	13,597	30
May, ...	3,046	20	21,795	10	50,500	20
June, ...	7,238	30	17,457	10	71,193	18
	35,175	35	52,295	10	157,954	28
July, ...	8,174	20	29,499	20	...	...
August, ...	6,357	20	20,381	0	...	...
September, ...	3,721	30	24,979	10	...	...
October, ...	5,043	30	14,173	0	...	...
November, ...	6,836	20	16,157	30	...	...
December, ...	12,607	20	17,151	20	...	...
Total, ...	77,917	15	174,637	10	175,954	28
Average per month in the 1st 6 months, ...	5,862	25	8,715	35	26,325	31
Average per month in the year, ...	6,493	4	14,355	4	26,325	31

Table showing the Monthly Export of Cotton from Madras, and its Official Value from 1860 to 1863.

	1860.		1861.		1862.		1863.	
	Quantity.	Value.	Quantity.	Value.	Quantity.	Value.	Quantity.	Value.
	Cwt.	Ra.	Cwt.	Ra.	Cwt.	Ra.	Cwt.	Ra.
January, ...	4,973	82,154	11,162	1,52,904	10,340	2,09,191	7,312	3,35,555
February, ...	5,605	1,02,290	26,439	3,53,754	9,096	1,87,300	24,485	11,96,878
March, ...	7,499	1,09,380	6,035	89,637	12,820	2,52,881	25,780	12,68,555
April, ...	10,591	1,66,235	6,748	1,30,242	7,430	1,80,941	12,755	6,44,706
May, ...	5,010	78,105	11,247	1,68,556	17,433	3,89,017	26,260	14,36,706
June, ...	1,663	25,132	20,113	3,12,498	36,115	8,80,529	55,875	31,00,255
July, ...	5,829	1,04,696	15,528	2,51,887	35,114	9,46,471	75,269	43,03,488
August, ...	21,246	3,22,288	21,061	3,40,859	39,062	15,94,115	...	...
September, ...	4,661	69,988	5,750	95,312	39,262	18,69,655	...	...
October, ...	19,003	2,65,371	6,391	1,17,888	5,749	2,85,660	...	...
November, ...	11,961	1,48,934	7,615	1,44,850	10,264	5,12,605	...	...
December, ...	6,992	1,01,454	2,315	46,747	18,844	3,28,133	...	...
	105,033	15,76,027	140,404	22,05,134	2,41,529	82,36,496	...	...

Dr Hunter also reports that the American Saw Gin for cleaning cotton has been introduced with great success.

X. Extract of Letter from WILLIAM JAMESON, Esq., Surgeon-Major,
Saharunpore, to Professor BALFOUR, July 9, 1863.

I send two small packets of seeds.

1. Seeds of the Folel or Phulwah (*Bassia lutyraea*) which is now just ripening here. From the seeds of this tree a kind of butter is extracted which is valuable in rheumatism. It is used in lamps, and as it gives a fine inodorous light, it is prized for night-lights. The tree grows to a height of from 30 to 40 feet, flowers in October, and ripens its seeds in July. It was supposed to be confined to Eastern Kumaon and Nepal, but this is a mistake, as it is common at Bhimtal, where I have an extensive tea plantation. Bhimtal is ten miles from the plains and twelve from the Sanatorium at Nynsee Tal. The Folel or Phulwah is met with growing at altitudes of from 4000 to 4500 feet. It will do well therefore in your green-houses, but it is not sufficiently hardy to withstand your winters. Where it is met with snow falls annually, but only remains a short time on the ground.

2. Seeds of Bamboo (*Bambusa arundinacea*), which flowered this year in the garden at Saharunpore. Other plants of Bamboo also flowered last year. As the flowering of the Bamboo rarely occurs in our gardens, and as the seeds appear to be good, a small supply may be useful to you.

I also enclose a few seeds of *Eremostachys superba*, which may be a novelty. It flowers in April, and is met with in hot, low localities, as at the Chowki, in the Mohur Pass, in the Sevalik range, and at Jewalah Moki in the Kohistan of the Punjab.

The museum building in the Saharunpore garden is now progressing rapidly, and I trust to see it finished about the end of the season. When filled with specimens, it will, I trust, be one of the most interesting collections in India. I am collecting botanical specimens useful in the arts and sciences from all parts of India, and as soon as the collection is sufficiently extensive a catalogue will be printed.

Two great exhibitions of arts and manufactures are to take place in India,—one at Lahore in November and December 1863, and the other at Calcutta in January 1864. In these we have a move in the right direction, as, under one roof, all the raw products and the articles manufactured in the respective countries will be brought together, and the wants and requirements of each district ascertained; at the same time will be shown what each can give in return, and send respectively into the market.

To open up the country, railways are rapidly extending; but amongst the engineers the cry is—We have no sleepers. Over hundreds of miles in the Himalayas the Cheer (*Pinus longifolia*) is met with in millions, forming trees from 10 to 18 feet in girth four feet from the ground, and in height varying from 80 to 120 feet. These noble trees are every where, I might say, met with in the mountains at altitudes from 3000 to 6000 feet—occurring in two varieties,—one with the wood white and twisted, and easily acted on by the weather, and thus useless in architecture or for railway sleepers; the other, generally met with on the northern slopes of mountains at altitudes of from 5000 to 6000 feet, has reddish-white timber, close-grained and highly resinous. This timber, of which millions occur in the Himalayas, is admirably fitted for architectural purposes, and if kyanised or creozotised would also make first-rate sleepers for railways. Nothing, however, has been done, and the cry of the engineers is,—We cannot get on with our work, because the trees met with in the country yielding timber, fitted for sleepers, are limited. To

remove this impression, so far as the North-West Provinces are concerned, I am doing my endeavour, and ere long I trust to see the so-called difficulty to the rapid progress of railways overcome. When once the railways are finished, Government, particularly that of the Punjab, will, through time, find difficulty in feeding the engines, unless every where measures be taken to plant the finest tracts which are now being felled. In the Punjab, only two short lines are open,—one at Moulton, the other between Amritzur and Lahore,—and, with this small drain, firewood has risen 150 per cent in value. Timber has from time immemorial been felled in the most reckless manner; and only now are the forests beginning to receive the attention that they deserve. Madras and Bombay have for a time been doing something; but as yet no regular plan has been pursued in the North-West Provinces. Numbers of parties were allowed to fell timber, and did so recklessly; so much so that first-class timber of Sal (*Shorea robusta*)—a timber admirably fitted for railway purposes—had, in many of the fine forests at the base of the Himalayas, become scarce, and hence the outcry of the engineers. But there are many other timber trees, admirably fitted for railway purposes, which, through sheer ignorance, have been passed over, such as the Sar (*Pentaptera tomentosa*), Backha (*Anogeissus latifolius*), Dhowlah (*Lagerstrœmia parviflora*), Huldou (*Nauclea cordifolia*), &c. In the Kohistan of the North-West Provinces and Punjab there is no chance of coal being found, the formation being altogether wanting.

I have now established the Cinchona plant in two localities in the Himalayas, in Gurhwal, and the west of Mussouree, at altitudes of from 4800 to 6000 feet. The following species have thus been introduced:—*Cinchona Condaminea*, *C. succirubra*, *C. peruviana*, *C. nitida*, and *C. micrantha*.

XI. Mr M'NAB's Report on Plants in Flower in the Botanic Garden.

To give some idea of the mildness of the present season, I beg to lay before the Society dried specimens of 220 species of plants in flower, collected from the open air in the Royal Botanic Garden since the 1st day of December; the largest proportion being the summer and autumn annual and perennial plants, the others chiefly composed of trees, shrubs, and spring flowering plants in the following proportions:—

Annual plants (summer and autumn),	34 species.
Perennial plants. do.	118 ...
Trees and shrubs, ...	38 ...
Ferns, ...	8 ...
Spring flowering plants, ...	22 ...

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The 220 species are spread over 50 natural orders in the following proportions:—

Natural Orders.	No. of Species.	Natural Orders.	No. of Species.
Ranunculacæ, ...	9	Caryophyllacæ, ...	11
Berberidacæ	2	Hypericacæ, ...	1
Fumariacæ, ...	1	Geraniacæ, ...	3
Crucifera, ...	17	Rutacæ, ...	1
Resedacæ, ...	2	Rhamnacæ, ...	1
Violacæ, ...	4	Leguminosæ, ...	7
Polygalacæ, ...	1	Rosacæ, ...	17

Natural Orders.	No. of Species.	Natural Orders.	No. of Species.
Myrtaceæ, ...	2	Labiatae, ...	13
Onagraceæ	1	Verbenaceæ, ...	1
Portulacaceæ, ...	1	Primulaceæ, ...	4
Umbelliferae, ...	4	Plantaginaceæ, ...	2
Araliaceæ, ...	1	Polygonaceæ, ...	2
Loranthaceæ	1	Thymelæaceæ, ...	1
Caprifoliaceæ, ...	2	Euphorbiaceæ, ...	2
Valerianaceæ, ...	1	Urticaceæ, ...	2
Dipsacaceæ, ...	3	Corylaceæ, ...	2
Compositæ, ...	26	Garryaceæ, ...	1
Campanulaceæ, ...	3	Coniferae, ...	4
Ericaceæ, ...	13	Iridaceæ, ...	2
Aquifoliaceæ, ...	1	Liliaceæ, ...	2
Jasminaceæ, ...	1	Juncaginaceæ, ...	1
Apocynaceæ, ...	3	Cyperaceæ, ...	1
Gentianaceæ, ...	1	Gramineæ, ...	8
Polemoniaceæ, ...	3	Filices, ...	9
Boraginaceæ, ...	4		
Scrophulariaceæ, ...	16	Total, ...	220

SCIENTIFIC INTELLIGENCE.

BOTANY.

The Progress of Tea Cultivation in Northern India.—As the Russian war gave an immense impetus to the growth of fibrous and oil-giving plants by the natives on the plains of Northern India, so the mutiny has been followed by a still more remarkable extension of the cultivation of tea, by English settlers and native landowners, along the belt of the Himalayas, between the altitudes of 2000 and 5000 feet for 1500 miles from Suddya to Peshawur. Official reports enable us to learn exactly the extent of that development up to so recent a period as May 1863. To ascertain the number of planters, extent of grants, and amount of produce at the close of the present year, we may add one-half to all the figures we are about to give. A glance at the "Calcutta Gazette" will show the enormous extent of tea land advertised as applied for by capitalists in Assam. Our share-list, which does not represent private owners, almost every week contains the name of a new tea company. There are several young plantations, which annually double their produce; and Dr Jameson's reports of the Western Himalayan Gardens abound in remarks, to the effect that the out-turn of tea a short time hence will be immensely increased. To the capitalist, the recommendation of a tea plantation is the annually increasing returns it gives from the third to the seventh year, when the plant attains perfection. Nine-tenths of the gardens now in existence are not four years old.

We shall begin our survey at the border line which separates China from Assam, and proceed westward. Chinese tradition points to India as the original home of the tea plant; and the connection between the two countries was so intimate, as proved by Buddhism, that we accept the fact on which the tradition is based. We had hardly obtained possession of Assam, when in 1825 Mr Bruce, still an uncovenanted officer at Tezpoore, discovered the indigenous tea-plant. For some time Govern-

ment nursed the experiment of cultivation, till in 1839 their gardens became the property of the long mismanaged but now most prosperous Assam Tea Company. Two years after this a few plants, grown from China seed, were introduced from Kumaon into Darjeeling, but no tea was made there till 1846, when an Assam planter visited the bright spot. No plantation was formed till 1856 at Kursiong. In 1855 a common labourer discovered the indigenous plant in the Cachar valley, and thus gradually was begun that cultivation in the three great tea districts of Bengal proper, to which Lord Canning's land policy gave such an impetus and stability in 1861. While in the plains, the sepoys strove to wrest from us our empire; on the hills white settlers were laying the foundation of a trade which will yet enrich the land; missionaries were building new stations; engineers were surveying Central Asia; and political officers were upholding our honour in the midst of Mussulman fanatics, who clamoured to be led to victory over the infidel.

In the entire province of Assam there were in May last 246 tea estates, of which 76 belonged to companies, and 170 to private owners. Of these 96 had been acquired during the year. The area of the whole was 122,770 acres, of which 20,144 were under cultivation, or an increase of 4144 during the year. These acres yielded 2,150,068 lbs., or 358,979 lbs. more than in the previous year; taking each pound at 1s. 9d., the whole produce may be estimated at L.190,000. Allowing for long indifference to the plant, this may be considered the result of ten years' labour since the Assam Company revived. But in the six years since 1856 no less than 177 grants of land, covering 558,078 acres, had been applied for in Cachar, or almost every available foot of tea land in that rich valley. On 78 of these grants containing 146,218 acres, 17,594 acres were cultivated, and of these 9426 had been cleared during last year. The tea manufactured, with seed sold, is estimated at L.47,614, and in the current year the value will be double. In six years planters here drew from the treasury of a district previously uninhabited no less than L.173,058. Where there was hardly a human being before, there are now 150 English planters, employing 15,317 coolies, and the number is increasing every month. At Darjeeling there was last year 12,366 acres cleared, of which 9102 were cultivated by 7447 coolies. The out-turn was 40,446 lbs. of tea and 3280 of coffee; and the official estimate for this year is three times this amount of tea, which is likely to drive coffee out of cultivation altogether, as it has done at Hazareebaugh in South Bengal, of which we can give no statistics. Tea cultivation is said to have been successfully attempted on the Kymore hills of Shahabad.

As Bhotan intervenes between the tea districts of Assam and Darjeeling, so Nepal absorbs a large extent of the tea-bearing area. Starting from the Kali river on its western border, we are at once in the great tea tract of the North-Western and Punjab Provinces, which covers 35,000 square miles away to the Huzara hills, into which, near Peshawur, the plant has just been introduced. Dr Jameson estimates the produce of this tract, when in full bearing, on the moderate scale of 100 lbs. per acre, at 93 millions of lbs, or the whole quantity now exported by China. Going still westward, in Kumaon there are 11 plantations, of which two belong to Government; in East Gurwhal 5, and in Dehra Doon 21, of which one belongs to Government and eight to Hindoos. Thus, in the North-Western Provinces there are 38,556 acres of tea grants, of which 4596 were under cultivation by 3080 labourers, and produced 33,960 lbs. last year. Passing on westward by the hill road from Dehra and Mussoorie, we enter the Punjab, where there are 9518 acres of tea plantations held by 23 owners or companies, of whom five are Sikhs and one is

Government: 5 are in the Simla district, 2 in the Kooloo and 2 in the Mundee territories, and 14 in the Kangra valley. Government is introducing the cultivation into the Huzara hills, and has given notice that it will sell in fee-simple its Holta plantation, as well as, probably, the four North-Western gardens, to the highest bidder. The experiment in the Punjab dates from 1851, the year of the Great Exhibition. We shall combine all these figures into one comparative table:—

PROVINCE.	Date of beginning.	Acres granted up to 1862-63.	Acres planted in 1862-62.	Number of Estates.	Number of Planters.	Number of Labourers.	Out-turn in 1862-63.
BENGAL.							
Assam, . . .	1826	122,770	20,144	246	[250]	[20,000]	2,150,068
Cachar, . . .	1856	558,078	17,594	177	155	15,317	827,670
Darjeeling, .	1856	12,366	9,102	[40]	[50]	7,447	40,446
Hazareebagh, .	1859		...	...	...		
N. W. PROVINCES.							
Kumaon, . .	1948	9,900	1,500	11	[20]	11,260	80,850
Gurwhal, . .	...	9,900	544	5	[8]	696	15,500
Dehra Doon, .	...	18,787	2,572	21	[40]	1,254	56,540
PUNJAB.							
Simla, Kooloo, and Mundee,	1860	2,400	[500]	9	[12]	[1,000]	[500]
Kangra, . . .	1847	7,118	[1500]	14	[25]	[3,000]	[2,500]
Huzara, . . .	1863		...	...	...		
		74,819	53,456	523	560	49,974	2,623,074

The three great tea districts vary in several particulars. Labour has to be imported into Bengal; it abounds in the North-Western and Punjab Provinces. On the other hand, Bengal enjoys cheap and easy transit, while in the last two this is the chief obstruction, which the railways will do much to remove. The tea of Assam and Cachar is from indigenous seed, and is stronger than, if not so fine as, that of the other provinces which use China seed. It is preferred by the home dealers to mix with inferior China tea. It will be observed, that in Bengal about two and a-half millions of lbs. were produced last year. From all these circumstances the Bengal tea is chiefly exported, while that of the Western Himalayas is consumed on the spot. The tea districts of the Eastern and Central Himalayas are long likely to be in the hands of English producers alone, while in the Punjab Himalayas, where the hill peasants are so enterprising, the cultivation promises to be conducted on the China method, in small patches by every village, and sold to brokers for manufacture. The eagerness of all castes and classes of our native subjects for tea is well known, and the coarser the flavour the better they consider it. At present tea is sold at a rupee a pound. When it becomes a shilling, and even sixpence for the coarsest kinds, what a trade will arise! All India with

its hundred and fifty, and Central Asia with its fifty, millions, will then be the market. And if the Western Himalayas alone, from the Kali river to Peshawur, can yield 393 millions of lbs., what will not the Eastern half, with all Assam, Sylhet, Tipperah, and Manipore, produce? Nay, without extravagance, we may assert that the whole of the hills between Suddya and the Yangtse-kiang, the Chittagong and Burmese hills, and the Yoma range to the valley of the Irrawaddy on the east, and Negrais on the south-west, are tea-bearing tracts, within easy reach of the Bay of Bengal.

In 1862 the import of tea into Great Britain amounted to 114½ millions of lbs., or an increase of more than 25 millions over 1860. Of the 96½ millions imported in 1861, China sent 92,145,365, Japan 1,348,911, and India, Singapore, and Ceylon 1,983,785. But while the average price of the China and Japan tea was 1s. 5d. per lb., that of India was 1s. 8½d, or the highest on the list. Since then Mr Gladstone has reduced the duty by one-third; and who can estimate the increase of consumption this will cause? If, though new to the manufacture, Indian tea-planters can obtain nearly fourpence a pound more for their tea than China, they ought, by careful preparation, and by strict honesty on their own part and that of their London brokers, to make their tea still more eagerly sought after every year. The most cautious will admit that there is practically no limit to the future of the tea trade of Northern India.—*Friend of India*, Sept. 17, 1863.

Tinder used in the Punjab.—Dr Cleghorn states that the tinder of the Hill shepherds, "Kuphi," is furnished by the woolly *tomentum* on the surface of a composite plant, *Onoseris tomentosa*, figured in Royle's "Illustrations" as *Chaptalia gossypina*. The plant is found every where at 7000 or 8000 feet, and the Kuphi taken to the plains. It is mentioned by Royle and Jameson that an inferior cloth is manufactured from the woolly down of the leaves. Other composite plants at high elevations are furnished with a somewhat similar downy substance.

Sissoo Tree (*Dalbergia Sissoo*).—Dr Hugh Cleghorn writes from Simla, 5th August 1863, to the Agri-Horticultural Society of the Punjab as follows:—"I enclose a photograph of a Sissoo avenue at Mozuffurgurh, executed by William Coldstream, Esq., C.S. The picture shows the remarkable growth which the Sissoo attains under favourable circumstances of soil and situation, and gives confidence in extending the culture of a timber tree which is so much valued in the Punjab. The measurements of three of the largest specimens are as follow:—

	Ft.	In.
1. Girth 4 feet above ground, ...	11	0
Girth 11 feet above ground, ...	9	5
Height between 50 and 60 feet.		
Cubic contents from base to 11 feet above ground (approximate), ...	91	0
2. Girth 4 feet above ground, ...	11	7
Girth 11 feet above ground, ...	11	4
Cubic contents from base to 11 feet above ground (approximate), ...	114	0

Both trees have an unbranched trunk for 11 feet, and then throw out two branches more than half the diameter of the trunk. On the road to Shereeshah, 1½ mile from Mozuffurgurh, is another Sissoo, girth four feet above ground 12 feet 3 inches. These trees were planted by Mozuffer Khan, who built the town and made the garden in which Nos. 1 and 2 are standing. The age is about seventy years." I send the above extract from Mr Coldstream's letter accompanying the photograph, as the syste-

matic collection of observations showing the rate of growth of different kinds of trees is needed in India.

Botanic Garden, Calcutta.—Dr Anderson's Report of the Royal Botanic Gardens Calcutta, during the past official year, appears in the *Calcutta Gazette*. The planting out of the groups of natural orders was commenced last rainy season. Two specimens of all the species of a considerable number of orders were placed in their proper sites. The botanical name and native country of the plant, painted on a large zinc label screwed down on an iron rod of about two feet high, were placed near the best developed specimen of each species. 2500 seedlings of mahogany were reared for planting along the portion of the Ganges and Darjeeling Road between Titalyah and the foot of the Himalayas. Three hundred seedlings of *Polyalthia longifolia* were prepared for forming an avenue along the Circular Road in Calcutta. Six hundred and eighty plants of *Diospyros embryopteris* were supplied for planting along the banks of the Circular Canal. Of the fifteen Wardian cases, each with 150 Cinchona plants obtained from Ootacamund, four were sent to Dr Jameson at Saharunpore, and eleven were obtained by private individuals for experimental cultivation on tea estates. Four hundred and twenty plants were added to the stock in the Darjeeling Nursery. Planters should remember that the only time the introduction can be attempted with any hope of success, is from 15th November to 15th February. The tedious duty of arranging the Herbarium, commenced by Dr Thomson in 1856, is about half completed; and, as two European botanists have been appointed for the work, it will be finished in four years. Dr Thomson's botanical library was purchased for the gardens for L.300.

MISCELLANEOUS.

Letter from Mr ROBERT BROWN, Botanist and Collector to the British Columbia Association of Edinburgh, dated Victoria, Vancouver's Island, July 24, 1863.—When I had last the honour of communicating with you, I was about to sail for Alberni. After being delayed for several days both in Victoria and Barclay Sound, I arrived there on the 28th May, and received a very warm reception from the little community, and excellent accommodation in a large house belonging to Messrs Anderson and Company. On the 3d June I started in a little schooner, the Codfish, manned by two men, bound on a trading expedition with the Indians along the coast, and which I considered a good opportunity of visiting various places which otherwise I would have no opportunity of botanising over. We cruised cautiously along the coast, visiting and trading with the savages; while I pursued my researches on shore for nearly a fortnight. In this way we visited or traded with members of the Opishessats, Shesats, Ouchleclous-ets, Oh-i-ats, Uclul-u-ets, Toquats, Clay-o-quots, Ah-ous-ats, Mam-ous-ats, Kel-simats, Ash-quots, Nuchlachlets, Eh-ut-usets, Kaiquots—all little tribes, mutually hating, and frequently at war with each other. But the most famous tribe and locality which I visited was the Nootkas, in Nootka Sound—a tribe which had been traded with by no white man since the murder of Captain Sta last January. I was particularly anxious to visit Nootka, as this was the locality visited by Cook, La Perouse, Meares, Vancouver, and also Menzies, who collected here the few plants he had from this part. Though it is not my intention to describe particularly the botany of the localities I visited, as the absence of books and specimens for reference must make an account most imperfect and uncertain,

I may mention that I obtained *Pinus ponderosa*, *Rubus nutkanus*, *Cupressus nutkatensis*, &c., all species interesting from the locality, and worthy of the seed being sent home. As this account of my travels must, for various reasons, be merely a very short outline, I cannot enlarge on the various sights and adventures I saw and mingled in on a journey which people tried to persuade me was dangerous in the extreme, and which only a sense of my duty to the expedition led me to undertake. In every case the Indians treated us well, though this might perhaps be due to the close watch which was kept on them, and the precaution used to avoid treachery. Circumstances have since shown that this was not unnecessary. We returned on the 20th June with all the *éclat* of a successful expedition. The weather, however, during the whole of our trip, was very wet, the rain pouring from morning to night, for sometimes a week at one stretch. I landed, however, on every opportunity, going as far as the dense bush would allow me, and returning regularly every afternoon drenched to the skin (for the Indians had stolen my waterproof coat), with no place to dry my clothes, for a temporary fire was only lit in a grate on deck to cook our food (twice a day), and the rain soon extinguished it. In the little "pigeon-house," called a cabin, we had barely room to squat about, surrounded by trading articles—blankets, beads, tobacco, brass wire, paint, calico, &c.; and often, when turning into our "bunks" at night, we found that the rain had penetrated and soaked our beds. As you may well suppose, our paper got but an imperfect drying. On one occasion, in despair I had to take it under my blanket at night. Of course, all these inconveniences were counterbalanced by many other pleasant occurrences—such as seeing strange scenes, wild men, new places. On the whole, with all the anxieties and dangers we were surrounded by, I do not know whether I would not have preferred it to an excursion in Switzerland or Clova—at all events, I do not regret it now, as it taught me, what no amount of book-learning or teaching could ever train me to—namely, a familiarity with danger, and with the savage tribes through whose country I must travel solitary for a long time yet to come. I now understand the method of treating Indians, their customs, and the Chinook jargon—the ordinary language of communication with the whites—tolerably well. I can now strike bargains, engage men, and travel alone without any assistance from the whites. I need scarcely tell you how valuable this proves to me in such a very rough roadless country, only peopled by a civilised population in a few places in the vicinity of the mines, &c., and where nothing can be procured except at enormous prices.

On the 24th June I started on another excursion to explore the botany of the interior lakes, hitherto almost unknown. As the sawmills at Alberni were stopped at that time to undergo some repairs, four men accompanied me in my excursion. We proceeded up the Somass River to a lake, on the lower arm of which Anderson and Co. have established a "logging camp." After exploring a small arm six miles long, and sleeping for a night among the moss, and having the dubious pleasure of hearing "the wolf's long howl from Unalaska's shore" (by the way, we were only a few days' sail from Unalaska), owing to a storm arising on the lake we were prevented from returning, as we had expected, to the backwoods camp. We returned at daybreak perfectly ravenous. That same morning we started in a canoe, with an Indian guide (Quasson, third chief of the Opischesats), up a long arm of the lake, which no one knew the termination of. We paddled along all day, landing here and there along the wooded banks to cook our dinner, or to "prospect" on an island. I found a *Thuja* allied to *Thuja gigantea* (Craigana, Balf.),

but, I think, distinct; and once I picked the cones of *Abies grandis*, and one very like *Pinus contorta*, Dougl. I "blazed" the tree, and will return in the autumn to it, if I do not find it elsewhere. Just as the sun was setting, we came to the termination of the lake, which I had named "Sproat's Lake," after the resident partner in the firm of Messrs Anderson and Co., my friend and countryman Gilbert Malcolm Sproat. It is about 23 miles in length in its longest axis. Here we found a river flowing in. We ascended this in our canoe for a mile, when the navigation becoming difficult we camped, and enjoyed a sound sleep by a blazing fire, notwithstanding the mosquito and various other pests, of which, to those who have camped "*sub Jove frigido*" in North-West America, there requires no explanation. Next morning, after discussing our pork and biscuit, we took to the woods, ascending the banks of the river through a fine open valley, comparatively thinly covered with fine timber, and free of undergrowth—a great rarity for Vancouver's woods. If the wood was clear of undergrowth, we found a pest which incommoded us considerably—*Panax horridum*, well deserving its name, with its long waving stem, crowned with a pecten-like head of leaves, and a raceme of white flowers. In many places the ground was covered with snow a foot and a-half deep, through which some bulbous plants were protruding their flowers. In the course of the day I made a very agreeable discovery in the examination of what I thought the *Abies Canadensis* of the Atlantic slope; but which I found represented by a species which may be little known in England. It has only been recently described by my friend Dr Albert Kellogg, secretary of the Californian Academy, in their Transactions, vol. ii. p. 8, under the name of *Abies Bridgeri*. Though I will take care to send you plenty of it later in the season, I subjoin a short description of it:—

Leaves.—Evergreen, solitary, linear cuneiform obtuse, somewhat flattened, fleshy slightly, grooved above, ridged beneath, very minutely scabrous, serrate petiolate, somewhat two-ranked. *Cones*.—Numerous, solitary, terminal, pendent, elliptic-ovoid, about twice the length of the leaves. *Scales*.—About thirty or more oblong, roundish, concave, margin entire, thin, translucent, finely corrugate striate on the back; base abrupt, subauricled, stoutly attached to the ligneous axis. *Bracts*.—Three-lobed ciliate, villous ($\frac{1}{2}$ in. long.) *Seeds* (including wing) scarcely less than the scale, wings oblong oblique, broader at the base, somewhat suddenly narrowed above, obtuse, laterally warped or carinated, seeds proper, ovate, light-brown or drab colour, uniformly marked by three minute ovate glands on the side looking towards the base of the cone. A tree 80 to 100 feet in height, of dark verdure and graceful appearance. The branchlets are very villous, slender, and drooping. The timber is said to be firmer, finer, and straighter grained than the Canadian hemlock spruce, which it represents on the Pacific coast. It is certainly closely allied to *A. Canadensis*, but I believe, with Kellogg, that it is distinct. Even the Canadian woodsmen, who are very apt to forget that similarity is not identity, and apply "old country" names to anything at all resembling what they are familiar with in Canada or Maine, recognise this.

About three o'clock p.m. we called a halt to take counsel, after having tracked along the banks for about twelve miles, the river dashing over rocks or flowing calmly over gravelly spit, through wooded meadows or high trap banks, backed by snow-peaked hills, from which the melting snow was leaping down in cataracts, now hid among the dark pines, and again bounding over some rock, until it fell in a sheet over the bank of the river. On starting, we had only taken a few biscuits, expecting to find the river merely a freshet from the mountain, and that we

could proceed only a short way; but where the river was as broad as when we first ascended, it became prudent, as there was no appearance of game, to hold a survey upon our provisions, when it was discovered, to our dismay, that we only had two biscuits; so I ordered a retreat.

We reached our camp just before dark, very tired and very hungry, and sweetly did we that night enjoy our bed of pine branches, prefaced by a most substantial supper of salt pork, hard biscuit, and tea—our provisions on all such journeys in this country. While cutting the branches for my bed, I found *Taxus brevifolia* (? *Lindleyana* Murr.), but without fruit. Our fire was of *Abies Douglasii*. *Abies Menziesii* was not uncommon, random cut, but the commonest was the former; also *Abies Bridgei* and *Pinus Strobus*, with *Rubus spectabilis*, *R. nutkanus* and *R. leucodermis*, with *Ribes* (two species). I did not see *Ribes sanguineum*, so common in our English shrubberies, but it is excessively common on the southern part of the island, particularly the Indian Reserve at Victoria. I observed also *Vaccinium ovatum*, and occasionally the Indian Salsul (*Gualtheria Shallon*), a very pretty shrub. Next morning, looking about the neighbourhood, we re-entered our canoe, hollowed out of *Cupressus nutkatensis*—the mats we sat upon being made of the *liber* of the same tree, ropes of the same material, and occasionally of *Thuja plicata*. This river I named after one of our party, Mr Taylor, an Edinburgh man, who had formerly accompanied me on my first journey on the island, when we were lost in the Nausaimo Mountains, wet and half starved, for two days. Some of the mountains round I dedicated to some of our Edinburgh friends, and which will afterwards appear, when I send home to the Geographical Society, when I have leisure for it in the winter, the chart of my journey. The Indians told us that it flowed out of a lake near Clay-o-quots Sound, and that the Clay-o-quots sometimes descend it. We reached the camp (logging) for supper, and slept among some hay that night sorely harassed by aphaniptera. Next day we started again in our canoe down the south arm of "Sproat's Lake," and landed three miles off in the wood. We now started off, each man with his blankets on his back, over an Indian trail. After travelling for about two miles through dark shady woods, over a tolerably good trail marked by knotted twigs, we were startled by the appearance of a man. It was Kan-ash, second chief of the Opischesats. On learning from "Quasson" that I was "Hyass-ty-hee" (great chief), he was exceedingly friendly, but parted "uyon silex" (very sulky) because I would not give him an "as-pop" (gift) for passing over his land, and for the future use of some supposed "chapatz" (canoe) which was on the "Uyass-aw-ak" (the Great Lake), to which Quasson was conducting us.

About two o'clock, the Great Central Lake, forty-five miles long, burst upon our view, the landing-places scattered round with bones of bears and deer, the remains of Indians who had formerly camped here. We found a canoe hid in the bush, but of such a miserable character as to be quite unfit for our purposes, only holding three at a time, and then rapidly filling with water; so we were very unwillingly forced to abandon our project of ascending the lake to its head. We camped that night near the lake, and returned next day to the mouth of the Somass River, where, after a good deal of trouble at the Opischesat village, at one time threatening to be serious, I obtained a canoe, and swept down the river, and thence into the Alberni Canal, all safe and in sound health and appetite, after our novel journey. Such is a short account of one out of many similar journeys. I have gone more into detail concerning it than some others equally interesting. But as I cannot enter into anything like particulars regarding all my travels, you may accept this as a specimen. Some were

better; many a great deal worse. Of course, on such excursions a botanist can only collect one, or at most two specimens of each plant he meets with; and frequently, so limited are his means of conveyance in an almost trackless country, he can only take such as he has not seen in other places, noting, however, the occurrence of all others, in addition to all such observations as ordinary travellers make. So many mishaps come under one's precious load before it gets to a place of safety, that sometimes I have been mortified to find, after a laborious excursion, the tangible results, notwithstanding all my care, consisted only of a few blackened, indifferently dried specimens. I was preparing to cross the island by the Indian trail to Quallicom, and thence by Nauaimo and the valley of Corvitchen to Victoria, a distance of about 100 miles, when the rain began to fall in torrents, and so fell for ten days in a continuous pour, until, losing all hope of being able to effect this journey for the present, I came down to Victoria in the Thames, where I arrived after twenty hours' steaming.

I here take the opportunity of recording my thanks to Messrs Anderson and Co. for the great kindness and assistance they rendered me through Mr Gilbert M. Sproat, the resident partner, and without which it would have been utterly impossible to have undertaken anything at all approaching to such a lengthened exploration. On my arrival in Victoria, I found that my visit to the Nootkas had got noised abroad, and all the newspapers were anxious to have the first account. By request of his Excellency the Governor, I called upon him next day, when he was pleased to express great satisfaction at my safe return, and at the mode in which I had conducted the exploration. Among other favours, he offered me letters to the magistrates and district officers in British Columbia. Governor Douglas, some people say, is about to resign; and if so, his loss, I am sure, will be a deep blow to the success of the colony, for, notwithstanding many differences of opinion from interested or disappointed people, no man will ever be found with such a thorough knowledge of the wants of the country and all that concerns it.

Finding many plants in flower round Victoria which I had not obtained elsewhere, I remained here until the above date, wandering far and near, and frequently passing the night in some surveyor's tent away in some wild part of the district. During this period I was occupied part of the time in making arrangements for my more important future journey, and had the pleasure of gaining the friendship of many most pleasant people—so pleasant, indeed, that one feels almost sorry to have to leave them, and take to a vagabond life again in the wilds, where he may pass long days without looking on a human face.

I propose to start on Thursday the 30th on another and longer journey, and one which will be most profitable, not only because undertaken during the seed season, but also over a more varied and extensive tract of country. From Victoria, I go to Port Angelos, W.T., thence to Port Townsend, and *via* Olympia, Fort Nisqually, Seattle, and Whatcom, over the Cascade Mountains to the Fraser River, along the Fraser to Fort Alexandria, and down either the Bentinck arm trail, or that by Bute Inlet. This will, I expect, occupy me until the end of October, and will, I hope, yield something to reward the outlay of the Association, though I know I will be devoured by mosquitoes. You will perceive that part of it lies through American territory, but I do not suppose that the Association is so fastidious as to object to seeds gathered over what our Government was compliant enough to assign as the boundary line of 1846; and, indeed, the country is not so rich as to allow of picking and choosing. I have obtained the Government credentials necessary for the journey.

Spontaneous Generation.—This subject has occupied the attention of the French Academy for some time, and various communications have been read on the subject. The experiment of M. Pasteur on this subject are conclusive. He has shown by a series of admirable and carefully executed experiments, that no organisms appeared in solutions, unless the germs were introduced from without. M. Flourens states, that he considers M. Pasteur's experiment as decisive; and that there is no such thing as spontaneous generation. Germs and sporules are so abundant in the atmosphere everywhere, that there is no excluding them from fluids exposed to the air. M. Pasteur shows that even the mercury trough was the receptacle of a multitude of germs, and that these entered the tubes during manipulation. The experiments, therefore, required the utmost caution in order to be free from all sources of fallacy.

Call to participate in presenting a Testimonial to the distinguished Botanist, Dr Carl Friedrich Philipp von Martius of Munich.—In March of the coming year 1864, Dr von Martius will reach the fiftieth anniversary of his graduation in medicine. During his labours as a teacher for so many years, he has gained the highest esteem of his numerous colleagues, friends, and pupils, and has rendered the greatest services to science. All who have been privileged to live with him, or who at times only may have been inspired by his presence, will seize with pleasure an opportunity like the present, to give expression to their feelings of esteem, friendship, and gratitude. The recognition, from his contemporaries, which may fall to the lot of the man of action is in truth his highest reward, and there is much bitterness in life which only thus is sweetened and sunk in forgetfulness. Here, arriving at an epoch in his life, we see a man who is honourably known not only in his own country, but far beyond its limits; who, indeed, enjoys in foreign countries a reputation such as falls to the lot of few in his department of science. He has earned this reputation by his unexampled activity; and his works, which may serve as models alike of profound investigation and of beautiful delineation, are so numerous, and several are so magnificent in their plan, that they cannot but excite the greatest wonder. It is true that any mark of recognition in such a case is only an imperfect expression of the feelings which suggest it. To express these, and, as far as possible, to embody them in an appropriate form, the undersigned have deemed it most suitable to commemorate this jubilee occasion by presenting Dr von Martius on the day of honour (the 30th of March) with a medal struck in gold. In order to procure for this end the necessary and not inconsiderable means, co-operation from all quarters is necessary. Considering the great number of friends, and the multitude of his former pupils, now for the most part in the position of medical practitioners or apothecaries, we hope we do not deceive ourselves in reckoning upon numerous contributors. We beg that every contributor should give at least two thalers cons., and hope that such a contribution may make it possible to prepare for every contributor a copy of the medal in bronze which will be accompanied by a list of the contributors.—*Erlangen, November 1863.* (Signed) Dr LUDW. RADLKOFER, U.-Professor, Munich; Dr AUG. SCHENK, U.-Professor, Würzburg; Dr ADALB. SCHNIZLEIN, U.-Professor, Erlangen.

Geographical Discovery in New Zealand.—Dr James Hector, the Government geologist now engaged in the survey of New Zealand, has discovered a navigable river flowing into Martin's Bay, on the west coast, near Milford Haven. This river descended and entered a lake ten or twelve miles long, and from one to two miles broad. Passing through this lake, he found another river falling into it at the eastern end. This river he also entered and ascended for a considerable distance. He then left his vessel.

and proceeded through a country fit for settlement. After only forty-six hours' march, he found himself at Queenstown, on Lake Wakitepu. Martin's Bay will now be the nearest port to Melbourne and Sydney. The future town in Martin's Bay may entirely eclipse Dunedin.—*Times' Correspondent.*

OBITUARIES.

*The late Rev. Stephen Hislop of Nagpore.**—The newspapers of the day have recently recorded the death, in melancholy circumstances, of this gentleman, whose geological researches in Central India will doubtless render an obituary notice of him interesting to many.

Stephen Hislop was born at Dunse, in Berwickshire, on the 8th of September 1817. He received his education first in the schools of his native village, and subsequently at the universities of Glasgow and Edinburgh. On the "Disruption" of the Scottish Establishment in 1843, Mr Hislop, then a student, cast in his lot with the seceding party, and was sent out next year to found a mission at Nagpore, in Central India. Schools were in process of time established at the three stations of Nagpore, Seetabuldee, and Kamptee. These ultimately acquired great influence, being attended by no fewer than 700 pupils. During extensive tours, undertaken for missionary purposes, Mr Hislop paid keen attention to the physical character of the districts traversed, and various geological discoveries of a remarkable character were the result. Several officers, after a time, joined in the inquiry, and rendered effective assistance. A brief notice of the Nagpore discoveries was sent by Mr Hislop, in April 1853, to the Bombay Branch of the Royal Asiatic Society; and a more detailed paper, in our joint names, was read before the Geological Society of London on the 21st July 1854, and printed in their "Quarterly Journal" for August 1855. Various supplemental papers were subsequently drawn out by Mr Hislop, which also appeared in the journal. When afterwards at home on sick leave, he obtained aid from the British Museum and other sources, and described the fossil shells from Central India, a large number of which were new to science. Professor Owen had already named and pointed out the characters of one remarkable fossil, a new labyrinthodont reptile. Professor T. Rupert Jones, from whose kind assistance and sympathy much advantage had all along been derived, added a memoir on the Cyprides. Mr A. Murray, F.R.S.E., took up the subject of the insect remains. Sir Charles Bunbury appended a paper on the more antique series of the fossil plants. The more modern series of plant-relics, chiefly beautiful fruits, believed to be of Eocene age, has not yet been figured.

Though geology was the chief, yet it was not the only subject of inquiry at Nagpore; and it was in connection with another department of research that the lamented missionary met his death. In December 1847, as Mr Hislop, with his colleague, was passing the village of Takulghat, twenty miles south of Nagpore, he observed a circle of large unhewn stones. Further examination revealed, that there were no fewer than ninety such circles, some single, others double—all close together, and spreading over an area of about four square miles. Permission was subsequently sought and obtained from the late Rajah of Nagpore to make excavations among the circles; and in the centre of one of them, at the depth of three feet from the surface, was found an iron vessel like a frying-pan, with a handle on either side, which had rusted off and was now lying detached. The bot-

* This obituary is from the pen of the Rev. Robert Hunter, late of Nagpore, and is inserted in "The Reader."

tom of the vessel was covered with little pieces of earthenware, neatly fitted to each other like mosaic work, possibly designed to protect human ashes, of which, however, there were only doubtful traces beneath. The hostility of one of the petty native officials at Takulghat prevented the excavations from being as complete as had been intended, and it was all along felt that they should be resumed when a favourable opportunity presented itself. A couple of months ago, Mr Temple, the chief commissioner at Nagpore, who had succeeded to the authority of the deceased Rajah, was encamped four miles from Takulghat, and, feeling that the time had come for paying renewed attention to the stone circles, wrote requesting Mr Hislop to join him and superintend a new series of excavations. The missionary in consequence set off for Takulghat on the 3d September, and aided Mr Temple during that day and the next. As the night of the 4th approached, Mr Temple left first for his camp, and Mr Hislop was to follow when he had gathered up the trophies and examined a school in the neighbouring village. It was the depth of the monsoon, and, though no rain had fallen at Takulghat, it must have done so very heavily somewhere in the vicinity. In consequence, Mr Temple, on reaching the backwater of a river, which had been quite shallow in the morning, found it 10 feet deep. He left behind him a native to warn Mr Hislop of his danger, and conduct him to a ford some distance higher up the stream. When night fell the native left his post, and went back nominally to meet the missionary. Two horsemen left at Takulghat, as an escort were also missing when the hour of departure came. Mr Hislop, thus unwarned, rode up to the fatal post, unattended, at a canter, and was immersed before he suspected danger was near. There were indications to show that he must have been thrown from his horse, as the animal was plunging in the water. Alone, and under the cloud of night, he struggled hard with his destiny. But all his efforts were unavailing to avert the fatal result. When, two hours later, his horse appeared in Mr Temple's camp without a rider, alarm was excited, and parties having been sent out to scour the jungle, the body of the missing missionary was found in comparatively shallow water, still holding with a death-grasp handfuls of grass, showing that he had succeeded at one period in reaching the bank, but been too feeble to raise himself from the water.

Mr Hislop was tall, wiry, and able to endure astonishing fatigue. His natural and moral courage were heroic. None could long associate with him without being constrained to admire his fortitude and decision of character. His powers of observation were of the first order. His mind was unimaginative, but logical and painstaking in no ordinary degree. His general knowledge was extensive. He could address a native audience in Mahratta with much effect, and had preached in that tongue through a great part of Central India, enlivening the tedium of the journeys (mostly performed on foot) from village to village, by geologising as he went along. His personal piety was deep and sincere, and he possessed much influence over the Europeans, as well as the natives of Central India. He leaves a wife and four children. An obituary notice of him, from the pen of Professor T. Rupert Jones, in the November number of "*The Geologist*," concludes with these remarkable words:—"Taken away suddenly from his family, his friends, and his native church and schools, he will live in our memory as a beloved man, just and good, and as an acute observer, cautious and conscientious, not courting praise, nor even notice, but delighting in work and truth, as a loving student of nature, and a faithful servant of God."

Mr P. A. Munch, the Historian of Norway.—The following notice

has been transmitted by Mr Christopher Holst, Secretary of the Royal University of Norway, Christiania:—The Royal University of Norway has the honour of informing you of the loss which it has just sustained in the person of Mr P. A. Munch. Mr Munch, whom the voice of the people has proclaimed the national historian of Norway, was born at Christiania on the 15th December 1810. His father, Edward Munch, a protestant minister, made him take his first classical studies at the school of Skien, and sent him at a later period to study law at the University of Christiania. Young Munch soon drew public attention towards him by a remarkable intelligence, a lively imagination, and a wonderful memory. He abandoned law for historical studies, and was appointed Professor of History at the age of thirty-one, 16th October 1841. He devoted himself from that time exclusively to the history of his country, whose monuments are found almost as numerous out of Norway as within its actual limits. His researches led him successively into Sweden, England, Ireland, and Normandy, where he sojourned at different times. Little by little his name acquired a European celebrity, and he was elected member of several learned foreign societies. In 1857, the Storthing having given him a grant, in order to enable him to make researches at Rome into the ancient history of the Scandinavians, he devoted several years to the fulfilment of this mission in the archives of the Vatican. He gave himself up to these studies, which would have deterred a less persevering energy, with a sagacity which is only equalled by the strange boldness of his conclusions. Centuries did not succeed in hiding from him any of their secrets, and his penetration into the obscurity of past times enabled him to illuminate with glowing hypotheses the chaos of our early history. Is it not hypothesis alone which could open to history the tracks which the critic will clear at a later time? Mr Munch is the author of a number of historical, geographical, philological, and political writings. But his great work, his special title to glory, and at the same time to the eternal gratitude of his country, is his *History of Norway* (*Det norske Folks Historie*), a remarkable scientific and critical work, by which he has made known to the Norwegians their national origin. Unhappily this monument—raised by a choice spirit to the honour of a people who were formerly powerful, and whose annals were associated in the Middle Ages with those of the most of the great European nations—remains uncompleted. Death has stopped this work at the date of the union of Calmar (1397). Mr Munch was suddenly carried off at Rome on the 25th of March last, in the fifty-second year of his age. He leaves a sorrowing widow, one son, and four daughters.

PUBLICATIONS RECEIVED.

1. *Air-Breathers of the Coal Period; a Descriptive Account of the Remains of Land Animals found in the Coal Formation of Nova Scotia.* By J. W. Dawson, LL.D., Principal of McGill University.—*From the Author.*
2. *An Inquiry into the Nature of Heat, &c.* By Zerah Colburn. London, 1863.—*From the Author.*
3. *Canadian Naturalist and Geologist*, for February, April, June, and August, 1863.—*From the Editors.*

4. Bulletin de l'Académie Royale des Sciences, des Lettres, et des Beaux Arts de Belgique, Nos. 6-10, 1863.—*From the Academy.*
5. Journal of the Chemical Society, for September, October, November, and December 1863.—*From the Society.*
6. Journal of the Asiatic Society of Bengal, No. 2, for 1863.—*From the Society.*
7. Transactions of the Tyneside Naturalists' Field Club, Vol. VI, Part 1.—*From the Club.*
8. Catalogue of the Army Medical Museum, Washington, 1863.—*From the Surgeon-General of the United States Army.*
9. Report of Lieut.-Colonel J. D. Graham, United States Topographical Engineers, on Mason and Dixon's Line. Chicago, 1862.—*From the Author.*
10. Transactions of the Academy of Science of St Louis. Vol. II. No. 1, 1863.—*From the Academy.*
11. Annual Report of the Board of Regents of the Smithsonian Institution for 1861.—*From the Institution.*
12. Report of the Superintendent of the United States Coast Survey, for 1859 and 1860.—*From the United States Government.*
13. Dr Daubeny on Climate. An Inquiry into the Causes of its Difference, and into its Influence on Vegetable Life. 1863.—*From the Author.*
14. American Journal of Science and Arts for September 1863.—*From the Editors.*
15. Quadrature du Cercle. Par un Membre de l'Association Britannique pour l'avancement de la Science (James Smith); traduit par Armand Granges, Bordeaux, 1863.—*From the Translator.*
16. Essays on Digestion. By the late James Carson, M.D.—*From the Publisher.*
17. First Principles of Natural Philosophy. By William Thynne Lynn, of the Royal Observatory, Greenwich.—*From the Author.*
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Notes on the Mummied Bodies of the Ibis and other Birds, found in Egypt. By A. LEITH ADAMS, A.M., &c., Surgeon 22d Regiment.

I procured many mummied specimens of the *Ibis religiosa* (Cuvier), from Thebes and Lower Egypt, and found the following discrepancies in the measurements. It must be understood that the specimens were procured from different localities, and not two from the same tomb.

From the occipital protuberance to the tip of the bill the following were the dimensions in seven—viz., (1.) $8\frac{1}{8}$ in.; (2.) $8\frac{1}{8}$ in.; (3.) 9 in.; (4.) $7\frac{3}{8}$ in.; (5.) $7\frac{1}{8}$ in.; (6.) $9\frac{1}{8}$ in.; (7.) $7\frac{1}{8}$ in. The bills of these measured from the gape to tip—(1.) $6\frac{1}{8}$ in.; (2.) 7 in.; (3.) $8\frac{1}{8}$ in.; (4.) $5\frac{1}{8}$ in.; (5.) $5\frac{1}{8}$ in.; (6.) $7\frac{1}{8}$ in.; (7.) $5\frac{1}{8}$ in. Thus the total length of the skull varied from $7\frac{3}{8}$ inches to 9 inches, and the bills from $5\frac{1}{8}$ to $8\frac{1}{8}$ inches.

The length of the *humeri* in the above varied from $4\frac{1}{8}$ to $5\frac{1}{8}$ inches.

The *ulnae* could only be measured in four of the number, and ranged from $5\frac{1}{8}$ to $6\frac{1}{8}$ inches.

The *tibiae* in five of these specimens varied from $5\frac{1}{8}$ to 7 inches.

The *tarsi* in the seven varied from $4\frac{1}{8}$ to $5\frac{1}{8}$ inches.

The middle toe in six of the specimens varied from $2\frac{1}{8}$ to $3\frac{1}{8}$ inches.

The largest claw on one specimen measured $\frac{1}{8}$ inch.

There can be no question that all I examined belonged to one species; and of many more noticed in various parts of Egypt, I did not discover one that, in the dimensions of its bill, would agree with either the black Ibis* or the glossy Ibis. I think that the discrepancies just shown may be fairly attributed to the effects of long domestication as well as sex. The manner the sacred Ibis was permitted to range over the country, especially in towns and villages, but perhaps more in and about the temples, would, in the vast ages embraced by Egyptian history, be sufficient to produce not only a considerable difference in the size of the bird, but probably also in its colouring; add to this, the filthy narrow streets among the mire and refuse of large mud-built cities, like Thebes and Memphis, where, under conditions akin to what is noticed with poultry, it is by no means likely that the pure white of the plumage was at all preserved. Hence, Herodotus† may have fastened on a variety and considered it another species. His description of the white Ibis is clear and distinctive, but that of the other "all black, with legs like a crane," is not so evident; certainly it is not applicable to the glossy Ibis, and, except in the colouring, the black Ibis (*Ibis sacra* of Temminck‡) will not agree; moreover, as far as I can discover, the last named species is not a native of any portion of North Africa. Cuvier§ states, that his specimens from Lower Egypt had larger bills than those found at Thebes. The largest billed specimen in my collection was from the Ibis pits near Sacarah. It is likely, however, that the bird did not thrive so well in the north country; indeed we know from Roman historians, that when introduced into the temple of Isis at Rome, after the conquest of Egypt, it soon pined away and died. Savigny states, that the *Ibis religiosa* is to be met with at the present day on Lake Menzaleh, near the Damietta mouth of the Nile; but all inquiries I have made during and since my excursion to Egypt failed to substantiate his assertion. With Vierthaller,|| I am inclined to agree that the sacred

* *Falcinellus igneus*, Gould, B. E., plate 47.

† Euterpi, l. xxvi.

‡ Temm. Man. d'Ornith., 2d Ed., 2, 596.

§ Ossements fossiles.

|| Naumannia, 1852, p. 58.

Ibis is not now a native of Egypt or Nubia. He fixes its northern limit on the Nile at 14° or 15° of north latitude; and states, that it migrates to Chartum in July, and breeds there on the shores of the White Nile. Heuglin records it in his list of birds collected on the Red Sea.* Bruce gives a faithful description of the bird.† Between the *Sacred Ibis* and the *Ibis bengalis* there seems a very close alliance. The earlier naturalists, such as Buffon, Belon, Perrault, &c., either never saw the bird, or were constantly confounding it with the *Tantalus ibis* and the storks. Belon, in his description of an ibis in the menagerie at Versailles, is evidently noting the characters of the black stork;‡ moreover, the Egyptian vulture has been frequently mistaken by travellers for our bird, and the common buff-backed heron§ at the present day pays the penalty of death from many Nile voyagers, in order to be preserved as the sacred Ibis, on account of its white colour, for in no other respect is there any resemblance. A difficulty to be accounted for with reference to the presence of the Ibis in Egypt during the existence of the ancient race is,—How did they obtain the bird? and that we can only conjecture by its having been gradually introduced, and having there propagated itself. Eggs have been found along with mummied birds; and in the Antiquarian Society of Edinburgh there is a collection of eggs found at Thebes by my late lamented companion Mr Rhind,|| which it would be well to compare with those of the existent *Ibis sacra*. From the enormous numbers of mummied bodies found both at Thebes and between the pyramids of Sacarah and Gizah, there cannot be a doubt but that the birds were very numerous. To have regularly imported young or old from the upper country would scarcely have been possible, and we have no proof whatever of the bird having ever been indigenous in Egypt or Nubia; besides, there is no reason why it should not have bred freely in a domesticated state, especially as long as it held its position among the sacred birds, which Herodotus tells us were

* Ibis, vol. i. p. 34.

† Ibis, vol. vii., App. p. 271.

‡ Obs. de Belon, Paris, 1555.

§ *Ardea bubulcus* (Savigny).

|| See that author's late work, "Thebes, its Tombs and their Tenants," p. 52.

preserved by a penalty of death to whomsoever killed one, either by "accident or design."* Its extirpation may have been gradual, but the Romans at first do not appear to have been the cause, whatever may have taken place subsequently. The temple of Isis was honoured by its presence, as the drawings in Pompeii and ancient writers fully prove. It is likely, however, as the religion of the ancient race began to suffer from the inroads of the early Christians, that as the latter increased so did the Ibis decline; and to a bird which had received so much attention, neglect was likely to have soon brought about a complete extinction of the species.

In comparing the past with the existent race, there are two points to be considered: *first*, the age of the mummied birds. Sir Gardner Wilkinson writes me—"It is difficult to ascertain the date of an ibis, or other bird mummy, because they have seldom hieroglyphics with king's names." He believes, however, that many at Thebes are of the time of the nineteenth dynasty, *i.e.* B. C.

In the *second* place, the modifications in the dimensions of the skeleton are without doubt owing to its having been domesticated and subjected to artificial influences, such as affect our tame animals.

There is a fact in favour of there having been only one species. Among all the coloured drawings on the tombs of Beni Hassan, Thebes, &c., one species is only represented, and that is clearly the white Ibis. The perfection, nay I may say beauty and brilliancy, of many of the paintings and delineations of the Ibis, are unrivalled by anything of the sort now-a-days. Moreover, it is to be expected that as its figure had to be used in the hieroglyphic writings more than that of any other bird, by dint of constant practice, and in spite of the conventional style of the Egyptians, it is likely they would, after so long practice, arrive at a high point of excellence in tracing its outline. From the countries now frequented by the Ibis, there is every reason to believe that at least it did not migrate to Egypt or Nubia in winter; moreover, it is demonstrably a tropical species, and

* Euterpi, i. 15.

does not venture beyond the confines of the tropics. I doubt, moreover, if it has any well authenticated claim whatever to a place in European lists.

The following analysis of the stomachs of several opened by me testify to its varied diet. With reference to reptiles forming its chief food, or that the species shows any particular predilection for these animals, is, I think, at best doubtful. In common with its allies, such as the glossy and black Ibis, &c., it no doubt devoured frogs, lizards, and small snakes, along with fish, shells, and coleopterous insects, and even carrion; in fact, the species was a sort of public scavenger, just as the Egyptian vulture and carrion crow are to the present race, and the former, the adjutant, and ground kite, are to the natives of Hindustan.

*Examination of the Gizzards of the Mummied Ibis, from Tombs at Thebes, and near Memphis.**

Specimen a. Contained numerous portions of small beetles, with the legs of larger species, also small objects like the naked seeds of wheat or barley, and several angular fragments of stone. The legs of this specimen were in a perfect state of preservation, owing to a solution of bitumen having been painted on them, which has the appearance of japanning.

Spec. b. The lower extremities, from not having been preserved in the way just described, and merely covered with a bituminous bandage, were very friable. The claws were much elongated, overgrown and twisted; the outer claw and toe so much deformed as in all probability to have greatly impeded the bird's movements. The skin on the forehead was much shrivelled, and the bill and tarsi were of a reddish-brown colour. The gizzard contained the back-bone and ribs of a fish of small size. The plumes on the back were distinctly traced in this specimen; but excepting the centre portions of the wing quills, the original colouring of the feathers was completely destroyed by the bitumen.

Spec. c. Wherever the bitumen had been sparingly applied to the naked parts of the lower extremities or the

* See note at conclusion of this paper.

bill, both appear of a reddish colour. The bones of this specimen were much smaller and more friable than many of the others—probably a young bird. The gizzard contained a mass of black peat-like substance, with portions of the elytra of small beetles.

Spec. d and e. The gizzards of these two were empty. One was the largest specimen of all I examined.

Spec. f. The gizzard contained several large quartz pebbles, interspersed with numerous univalve spiral shells, evidently of the family Paludinæ.* Along with the aforementioned pebbles, a small oblong bead was found, with a hole through it, the same as now found on the wrists and forming necklaces on the human mummies.

Spec. g. A mass of small beetles of different species; a few with brilliant green elytra;† also the cast of what may have been the shell of an *Helix*.

Spec. h, i, j, contained the usual amount of undeterminable peat-like substance, interspersed with abundance of elytra and parts of beetles of divers sorts.

Examination of a Mummied Hawk from the Necropolis of Thebes.

From the small portions capable of removal, and the size and measurements of the bones, I take this to be a female of *Circus pallidus*, which is at present one of the common rapacious birds of Egypt and Nubia.

Skull much fractured. The upper mandible, $1\frac{1}{8}$ in.; humerus, $3\frac{2}{8}$ in.; ulna, $4\frac{2}{8}$ in.; femur, $2\frac{1}{8}$ in.; tibia, $3\frac{2}{8}$ in.; claw of middle toe, $\frac{1}{8}$ in.

The right radius and ulna appear to have been fractured at the time the bird was buried, showing that the bird may have been killed by accident or design.

Examination of the Body of a Mummied Eagle, found at Thebes.

This is in all probability the common spotted eagle (*Aquila nævia*), which affects the river valley for a long way, even above the first cataract. The following mea-

* See note at conclusion.

† Several identical with the species common everywhere on the banks of the river.

surements correspond with a ♀ of *A. naevia* in my possession, killed at Thebes.

Head, as taken in the Ibis's, $4\frac{1}{8}$ in. ; cere to tip, $1\frac{3}{8}$ in. ; humerus, $6\frac{1}{8}$ in. ; ulna, $7\frac{7}{8}$ in. ; femur, 4 in. ; tibia, $5\frac{7}{8}$ in. ; tarsus, $4\frac{1}{8}$ in. ; great toe, $2\frac{1}{8}$ in. ; hind toe and claw, $2\frac{1}{8}$ in. ; hind claw, $1\frac{3}{8}$ in.

Sternum, antero-posterior measurement, $3\frac{1}{8}$ inches. Gizzard contained the feathers and tarsi of a small bird, with the usual amount of peat-like carbonaceous substance.

Examination of the Body of a Mummied Hawk, evidently a Kestrel (Falco tinnunculus, L.)

Head, $2\frac{1}{8}$ in. ; bill from cere to tip, $1\frac{7}{8}$ in. ; humerus, $2\frac{1}{8}$ in. ; ulna, $2\frac{1}{8}$ in. ; femur, $1\frac{1}{8}$ in. ; tibia, $2\frac{1}{8}$ in. ; tarsus, $1\frac{1}{8}$ in. ; great toe, $1\frac{1}{8}$ in. ; claw of do., $1\frac{1}{8}$ in.

The above dimensions are somewhat short for the ♀, but agree with ♂ of *F. tinnunculus*. Stomach was empty.

The birds found in a mummy state were evidently subjected to the process by injecting the bituminous substance into the trunk by a wound in the abdomen. In none of those examined had the brain been removed or disturbed. After freely bedaubing the outer surface, the tips of the wings and tail were more or less twisted together, and the legs either bent at the tibia-tarsal joint, and placed on the front of the breast by the sides of the wings, or stretched out at full length, as was usually the case with short-legged birds, as the Kestrel, Eagle, &c. Long-necked birds had the head brought down and placed on the belly, whilst hawks, &c. were preserved in the natural position. There seems, however, to have been no rule as to position of the head and extremities, the object being to so form the mummy that it might be easily placed in the jar, after which the mouth was sealed up, and the whole deposited in tombs and pits among others of the same description. It appears that the latter was the case, more especially in Lower Egypt, whereas at Thebes the Ibis and other birds have been found with merely the usual thick walls of bandage around them.

I have often unrolled a large mass of bandage, and

found only a leg or wing of an Ibis, from which I conjecture that these may have been portions of mutilated carcasses, possibly half-destroyed by dogs, &c.; and as the bird was so highly venerated, every part of it, wherever found, was preserved with the greatest care.

From the evidence of historians, and what can be inferred from a study of the mummied Ibis, I think we may fairly conclude, that the bird represented on the monuments, and preserved in pits, was identical with the *Ibis religiosa* of Cuvier, *Ibis aethiopica* of Bonaparte, the *Tantalus aethiopicus* of Latham, &c. We can also show that it was domesticated, and, in all probability, bred freely in Egypt, roaming over the cultivated tracts in and about certain towns, villages and temples, at least as late as the first and second centuries of the Christian era. I believe it disappeared with the religion in which it figured so conspicuously, and as the Christians increased, so the Ibis decreased. One may contemplate a few survivors among the ruins of Karnak, or on the battered walls of Thebes and Memphis for a few years after their overthrow, just as if the Hindu religion was to be overturned and a few sacred bulls were to linger on the scenes of their former majesty.

The vast numbers of the mummied Ibis met with, especially about ancient Memphis and Thebes, and the scarcity in other places, lead me to suppose that the bird was not universally distributed over Egypt; indeed, like the other sacred animals, it had its patron cities, Hermopolis being the chief, as is stated by historians; the site, however, of this city has not been clearly defined, and by some it is conjectured to have been one of the many names for Memphis; at all events, the bird was excessively common in and about the Pyramids.

With reference to other birds, it appears that many of the more common species were mummied,* the Kestrel, in particular, which, however, does not seem to have been at all so plentiful in comparison with the last. There can be no doubt, however, that hawks were often kept in cages in and about the temples. The Kestrel, the bird of Re, Horus,

* See author's "Notes and Observations on the Birds of Egypt and Nubia," in the "Ibis" for 1863.

and a host of other deities, must have enjoyed unbounded freedom and protection ; and it is a curious circumstance now-a-days, with reference to this species, that as it is one of the most common rapacious birds of Egypt, so is it far tamer in that country than anywhere else I have noticed. Can the feeling of security which pervaded the old race be still lingering on ? It is not evident why other than sacred birds should have been preserved ; but as the process of embalming was almost exclusively confined to the priesthood, who seem to have followed out whatever practices their own fancies suggested, they most probably gave directions that all dead animals should be brought to their temples, without reference to individual species, which, among the hawks especially, is not always very easily determined.

The circumstance that, even on ceasing to occupy its position as a sacred bird, not one Ibis remained in the country—which ought not to have been the case had the climate, &c., been suited to its habits and constitution—surely goes some length to show that the bird was a foreigner, and, when left to its own resources, soon pined away and died ; possibly the cold of winter tried it most, when it had been accustomed to withdraw more from under the ample shelter of the temples and among the dwellings of the natives. Besides, the artificial habits acquired by a long domestic condition had rendered the species in many respects almost akin to poultry ; although, as far as the mummied specimens go, there is every appearance in the development of the bones and muscles of the wings to lead to the belief that the bird could make good use of these organs.

Note.—The following notes on the beetles found in the gizzards of the specimens of Ibis unrolled by Dr Adams have been supplied by the kindness of Andrew Murray, Esq., and will be read with considerable interest :—

" The contents of the gizzards submitted to me consisted of small lumps of bituminous matter containing numerous fragments of insects. The numbers of individuals which these represented must have been very considerable. There were ten heads of a large species of *Calosoma*, besides a corresponding quantity of fragments of other parts of the body. There was a tibia of *Atauchus sacer* (a large insect), several legs of large *Pimelias* and *Blaps*, and a multitude of debris of smaller insects.

"In none have I been able to see any material difference between them and the individuals of the present day. The species which I have been able to identify are the following, and I mention the portions I have found in order to indicate the probable value of my opinion, viz. :—

"*CALOSOMA RUGOSA*, Sch., Dej. Spec. des Coleopt. ii. 202.

"Heads, thoraces, elytra, abdomen, legs, and tarsi—almost every part of the insect, but all separate.

"This species is not (as far as I am aware) now to be found in Egypt. It is found in the Cape of Good Hope, and at least as far north as Natal. Dejean quotes the Cape of Good Hope as the locality of his specimens. Boheman gives Natal. But it is not quoted in any of the Mediterranean lists (which of course include the records of Egyptian species), and I am not acquainted with any instance of specimens having been found anywhere else than the Cape. It is represented on the west coast by a somewhat smaller but very similar species, *C. imbricatum*. Its representative in Europe is *C. inquisitor*. Although I do not see anything to warrant the fragments not being referred to *C. rugosum*, they are not absolutely the same. They are rather smoother, perhaps smaller. The thorax has the edging of its margins not so much raised, and the rugose punctuation finer.

"*Sphodrus*, sp.

"The termination of an elytron. There is an Egyptian species named *piceicornis* by Klug, to which this may perhaps belong. I have not seen it.

"*Hyphedrus senegalensis*, Aube, Dej. Spec. des Coleopt. vi. 458.

"The entire body, except the head and legs.

"This specimen corresponds with Aube's description of *H. senegalensis*, but I have not seen an authentic type of Aube's species. As the name implies, the species comes from Senegal. There is no species recorded as being found in Egypt which at all comes near the mummified fragment. I have therefore the less hesitation in referring it to *senegalensis*.

"*Ateuchus sacer*, Linn. Syst. Nat., t. 1, part 2, f. 545, 18.

"A single broken tibia.

"Although the fragment is small it is well marked, and shows that it belonged to the true Egyptian *sacer*, and not to the other Mediterranean variety *piceis*, or any other of the varieties of that species.

"*Scorus tritii*, Fab., Sol. Ann. Soc. Ent. Fr., vii. 165.

"Fragment of elytra.

"*Scorus striatus*, Fab., Sol. Ann. Soc. Ent. Fr., vii. 165.

"A head.

"*Ademia* (perhaps) *microcephala*, Sol. Ann. Soc. Ent. Fr., iii.

"Fragments of elytra.

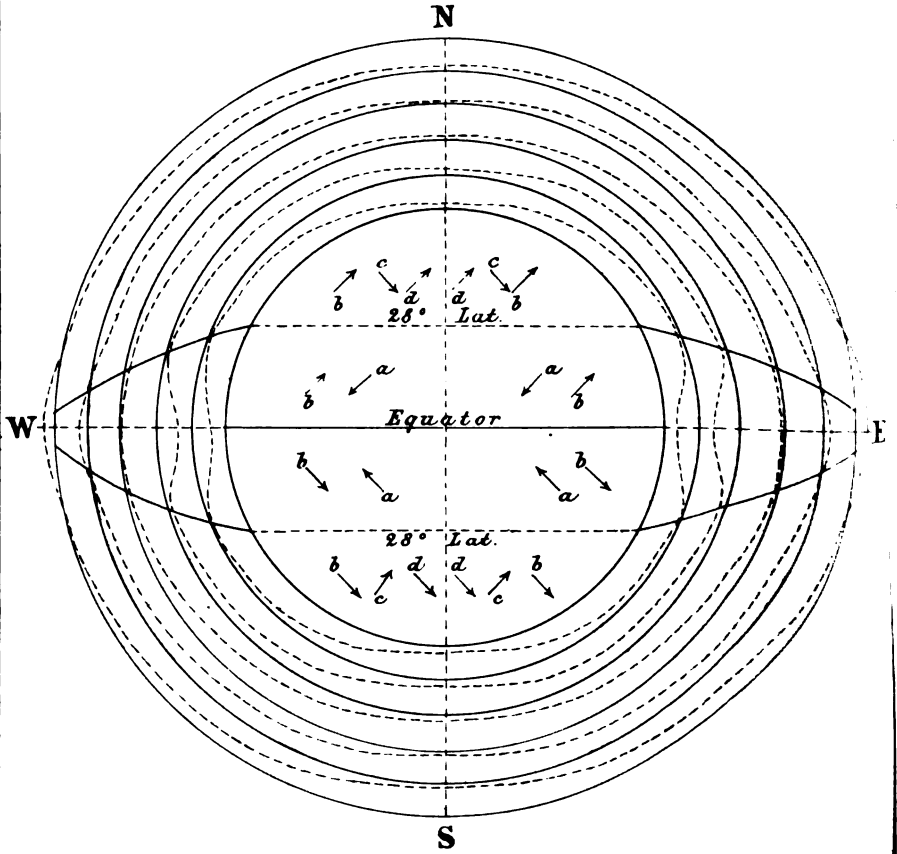
"*Ocnera* (Fisch.), (*Trachyderma*) *hispidula*, Fab. Klug Symb. Phys. ii. pl. 12, f. 8.

"Two fragments of the thorax and part of elytra.

"*Pimelia*, sp., Dej.

"A number of legs belonging to one or other of the large Egyptian species of *Pimelia*, such as *P. coriacea*, Dej., *P. barbara*, Sol., *P. cribripennis*, Sol., &c.





" *Opatrum subsulcatum*, Dej.

" Numerous elytra, and parts of the body and legs.

" *Sclerum*, sp.

" Several elytra and abdomen. Query *S. lucasi* near *S. foveolatum*, but larger and more distinctly marked.

Some of the gizzards contained remains of land or fresh-water shells. The only one sufficiently perfect to ascertain was submitted to Dr Baird of the British Museum, who writes to me—'Your shell appears to be identical in shape and size with the *Paludina bulimoides* of the Nile. This species varies much in colour and markings, but not in form.' The colours in this case were destroyed by the process the birds had undergone.—W. J.

On the Circulation of the Atmospheres of the Earth and the Sun. By JOSEPH JOHN MURPHY, Esq. (Plate I.)

Were the atmosphere not acted on by heat, it would be everywhere at rest, and every level surface, at whatever height, would be an isobarometric surface, or surface of equal barometric pressure. The earth's rotation cannot produce currents, but it modifies them when they are produced by the action of heat.

The greater heat of the equatorial regions expands the air, and thus causes the upper surface of the atmosphere to stand at a higher level there than in the polar regions. This difference of level produces an outflow in the upper strata of the air towards the poles; this outflow causes a partial vacuum in the lower strata, and an inflow of air towards the equator at the earth's surface. This inflow constitutes the trade-winds. The east component of the motion of those winds is due to the fact that they come from a higher latitude, where the earth's rotation is less rapid; they carry their less velocity with them, and thus have a relative motion from the east, or against the earth's rotation.

The upper currents, on the contrary, coming from a lower latitude, where the earth's rotatory velocity is greater, carry their greater velocity with them; they consequently move more rapidly than the earth itself in the latitudes to which they are impelled, and become south-west winds in the northern hemisphere, and north-west in the southern.

The upper and lower currents exercise friction on each other, and so tend to destroy each other's momentum, and the eastward momentum lost by the one must exactly equal the westward momentum lost by the other. But in addition to this the lower current must lose momentum by friction against the earth's surface. Consequently, the west component of the momentum of the upper current is much greater than the east component of the momentum of the lower one, and this preponderance of force causes the upper currents to communicate their own westerly motion to the lower ones. At the equator the easterly motion of the trade-winds must still prevail in a slight degree at all heights in the atmosphere. At a very little way towards the poles, the westerly motion begins in the upper stratum, thence the upper stratum of westerly motion deepens, and the lower one of easterly motion thins out, until about lat. 28° (taking the mean of both hemispheres) the former appears at the earth's surface. From thence to the poles, the air, in both its upper and its lower strata, constantly circulates round the globe from west to east, constituting what Maury calls the counter-trades. Every east wind in higher latitudes is either merely a local phenomenon, or a polar extension of the trade-winds.

Professor Coffin, in one of the earlier volumes of the Smithsonian Transactions, maintains, on the authority of certain registers, that the prevalent direction of the wind in very high latitudes is from the east. I do not understand the reasoning by which he endeavours to account for this, and I suspect it is a merely local or perhaps temporary phenomenon. Sir James Ross met nothing like it in high southern latitudes, and, as we shall see further on, observations bearing on the great atmospheric currents are of more importance when made in the southern than in the northern hemisphere. We have every reason to believe that in the northern hemisphere, during the summer half of the year at least, the pole of greatest cold does not coincide with that of rotation, and this would produce very complex and quite incalculable motions.

The principle of reaction makes it impossible that the winds can have any effect in either accelerating or retard-

ing the earth's rotation. A west wind moves round the earth's axis more rapidly than the earth, and tends, by its friction to accelerate the earth's rotation. An east wind, for the opposite reason, tends to retard it; and the two sets of forces exactly neutralise each other. The friction of a wind is approximately as the square of its velocity, and the unbalanced effect of any wind on the earth's rotation = the east or west component of its force $\times$ the area it covers $\times$ the radius of the parallel of latitude. The last factor gives *leverage*.

Were the whole equatorial region occupied by the trade-winds, and the whole of both circumpolar regions by the counter-trades, and were the east and west components of the force everywhere the same, the dividing lines, in order to produce the above-mentioned compensation, would be at $20^{\circ} 19' 20''$ north and south nearly,* but they actually are at about 28° , showing that, in order to produce the compensation, the force of the west winds must be greater than that of the east ones. The greater force of the west winds is a necessary consequence of the law of the conservation of areas, in virtue of which, if friction were absent, the air at any latitude would be moving round the earth's axis with an absolute velocity inversely as the radius of the circle of latitude. The excess or deficiency of the absolute velocity of a mass of air, as compared with the earth's velocity of rotation at the same latitude, is the velocity, west or east, of the wind. A mass of air in moving towards the pole will consequently gain absolute velocity, and increase in relative velocity as a west wind; in moving towards the equator, on the contrary, it will lose absolute velocity, and increase in relative velocity as an east wind. But the utmost increase towards the equator will be finite; towards the pole, on the contrary, friction apart, it would be infinite; the velocity at the pole would be infinite, in consequence of the radius of the circle of latitude there being nothing, which is physically interpreted by saying, that in the absence of friction no air would reach the pole—being kept away by centrifugal force.

Friction prevents the centrifugal force of these aerial

* My friend, Mr Harlin, Fellow of St Peter's, Cambridge, has calculated this for me. It is identical with the parallel that bisects the solid hemisphere.

vortexes from having so great an effect as this; but their centrifugal force does produce a sensible effect in keeping the air away from their centres, and heaping it up at their margins. The barometer stands at a maximum at about lat. 28° , from which it falls towards each pole. This depression is much greater in the southern hemisphere than in the northern; in the highest explored latitudes of the south, the barometer stands at least an inch below its mean level elsewhere. The reason I assign for this difference is, that the vortex is much more perfectly formed in the southern hemisphere than in the northern, owing to the unequal heating of the continents and oceans in the latter, which produces cross currents. Of course, the centrifugal force is chiefly due to the velocity of the upper strata, as that of the lower is reduced by friction against the earth's surface.

The excess of barometric pressure at lat. 28° over that at the poles, and the comparative absence of centrifugal force at the earth's surface, determine a motion of the air from lat. 30° towards each pole, and thus are produced the south-west winds of the middle latitudes of the northern hemisphere, and the north-west winds of the southern.* But these can occupy only a comparatively thin stratum. In the highest strata of every latitude, polar as well as equatorial, there must be a flow of air from the hotter to the colder regions, from the equator to the poles, and a return current underneath it, in the contrary direction. In the circumpolar vortex, consequently, there is a motion from the equator above and below, and a motion from the pole

* The cause of the polar depression of the barometer was first, I believe, pointed out by me in a paper read at the Belfast Natural History Society in the winter of 1855-6. The whole theory of atmospheric circulation in extra-tropical latitudes was first cleared up by Professor James Thomson, in a paper on the "Grand Currents of Atmospheric Circulation," read at the British Association in 1857, of which an abstract is published in the Transactions for that year. His discussion of the subject is also published with some fuller particulars in the "Proceedings of the Belfast Natural History Society" for 6th April 1859. I never published my paper, as I afterwards became convinced that it contained serious errors, but Professor James Thomson, in the last-mentioned paper, has referred to me as having first explained the cause of the polar depression of the barometer.

at an intermediate level. The motion from the poles is in the direction of the centrifugal force, that from the equator is against it.

There are two regions of barometric maxima, lat. about 28° or 30° , and three of minima, at the equator and at the poles. From the maxima, air flows at the surface of the earth to the minima, appearing in the tropical regions as the trade-winds, in the circumpolar as the counter-trades. The polar minima are produced by centrifugal force, and a barometer placed at any height above the sea-level, in the region of the polar minimum, will consequently stand below the normal level for that height; for centrifugal force acts at all depths in a vortex. But the equatorial minimum is produced in a totally different way, namely, by the ascent of rarefied air and outflow above. In that region, consequently, a barometer placed in the lower strata, where the currents are flowing inwards towards the barometric minimum, or comparative vacuum at the equator, stands below its normal level; but if placed in the upper strata, where there is an outflow of air towards the poles, it will stand above its normal level for the height. This is because an outflow can only be the effect of increased pressure and an inflow of diminished pressure.

In Plate I. the inner circle represents the earth, and the circles concentric with it represent level surfaces in the atmosphere. The dotted lines represent isobarometric surfaces. It will be seen that at all elevations they fall towards the earth's surface in nearing the poles. At the lower heights, they fall towards the earth's surface in nearing the equator, but at the greater heights they rise higher in nearing the equator.

Within the triangular spaces enclosed by the lines drawn from west and east to the earth's surface, the wind is from the east; outside of these, it is from the west.

The arrows marked *a*, indicate the trade-winds; *b*, the upper return trade-winds, which extend to the poles; *c*, the winds at middle height in the higher latitudes, which blow towards the equator; *d*, the winds at the earth's surface in the higher latitudes, which blow towards the poles.

It is obvious that in any other planet which rotates on

its axis, and is hotter at the equator than at the poles, the system of atmospheric circulation must be essentially the same as that of the earth.

The sun is such a planet. Its rotation has long been known, and Secchi of Rome has ascertained that its equatorial regions are sensibly hotter than the polar. No cause, I believe, has hitherto been assigned for this difference.

Mayer, Mr Waterston, and Professor William Thomson, have brought forward very strong reasons for believing that the sun is receiving a constant supply of heat by the fall of meteors from external space into his atmosphere; and Mr Carrington and another observer have simultaneously observed two meteor-like bodies of intense brightness suddenly appear on the sun's disc, and rapidly move across it from west to east. If, as is all but certain, meteors are small planet-like bodies, it can scarcely be doubted that the meteors, which supply the sun with heat, move round the sun from west to east like the entire solar system, and, like it, exist in a space of the form of a very oblate spheroid, having its greatest diameter nearly in the plane of the sun's equator. Consequently, the largest proportion of meteors must fall on the sun's equatorial regions, making them hotter than the poles.

It can scarcely be doubted that the meteors must enter the sun's atmosphere with a tangential velocity not much short of that of a planet revolving at that distance. We know that the sun's rotatory motion is incomparably less than this, and, consequently, the meteors, moving from west to east, ought to make the sun's atmosphere move round his body in the same direction, and with greatest velocity in the equatorial regions, as most meteors will fall in there. At the same time, the difference of temperature between the sun's equator and his poles, combined with his rotation on his axis, will tend to produce a system of circulation similar to that of the earth's atmosphere, and the actual circulation will be the resultant of this and of the motion from west to east, produced by the infalling meteors. Mr Carrington's comparison of the motions of the solar spots at different latitudes,* affords proof that such a circulation

* Proceedings of the Royal Astronomical Society, 18th April 1860. Mr

is what really exists. Assuming the sun's period of rotation to be 25·38 days, he has computed the mean daily drift of the spots, in longitude and latitude, to be as follows, the + sign indicating pole-ward motion in latitude and eastward in longitude.†

At 50° north — 64' in longitude + 11' in latitude.

30	„	— 25	„	+	5	„
18	„	— 14	„	+	1	„
8	„	+ 8	„	—	5	„
11	south	+ 10	„	—	3	„
19	„	— 10	„	+	1	„
29	„	— 21	„	+	4	„
45	„	— 85	„	—	2	(uncertain).

We thus see a regular decrease in eastward motion from the lowest to the highest latitudes in which spots are observed, being what I have inferred from the meteoric theory; but the exact opposite of that which is observed in the earth's atmosphere, and which exist in any atmosphere which is acted on, like the earth's, only by greater heat in lower than higher latitudes, combined with the planet's rotation; for in any such planet the motion of the whole atmosphere must be westward in the equatorial, and eastward in the middle and higher latitudes.

In order to explain the motion of the spots in latitude, it is necessary for me to assume that they are formed, and float in the lowest stratum of the sun's atmosphere.

Were the sun's atmosphere acted on only by the mechanical force of the in-falling meteors, the centrifugal force would heap up the air at the equator, and barometric pressure would be greatest there; and this excess of pressure would produce currents from the equator to the poles at the sur-

Carrington's facts, which I quote, are most valuable; but I confess I do not understand the reasoning by which he tries to account for them.

† It is true that the absolute motions in longitude assigned by Mr Carrington are quite untrustworthy, as the true period of the sun's rotation is not yet determined. But what we have to do with is the *differences* in the motions at different latitudes. If it is true that the sun's atmosphere is impelled round his body, it follows that the rotation of his body must be slower than has been inferred from observations of the spots. Mr Carrington thinks, on the contrary, that it is more rapid than he has assumed, in order to construct the table.

face of the sun, where the centrifugal force would be diminished by friction; just as we have seen that a similar cause produces currents in the earth's atmosphere from Lat. 30° to the poles.

But this is not what we observe. Mr Carrington's table shows that if lines are drawn round the sun at about Lat. 15° north and south, the currents in which the spots drift, flow on the polar side of those lines towards the poles, and on the equatorial side towards the equator. We may infer that a parallel of latitude from which currents flow on both sides must be a place of barometric maximum. In the earth's atmosphere, as we have seen, there are two such barometric maxima, but they are at Lat. 28° , about 13° nearer the pole than those of the sun; and in the earth's atmosphere they nearly coincide with the boundaries between the westward trade-winds and the eastward counter-trades. It is, I think, safe to assume that such coincidence must take place in the sun's atmosphere as well as in the earth's; for there must be an outflow of air at the surface of the earth from both sides of a zone of barometric maximum; and the effect of the planet's rotation on a wind flowing to a different latitude, will be to give an eastward direction to one towards the pole, and a westward direction towards the equator. I have shown that were the east and west component of the velocity everywhere the same, the boundary of the east and west wind regions would be at Lat. $20^{\circ} 19' 20''$, in order to have no effect on the planet's rotation. But I have further shown, that the force of the eastward winds of the higher latitudes, must of necessity be greater than the force of the westward winds of the lower latitudes; so that in order to effect the above-mentioned compensation, the boundary must be on the polar side of the parallel of $20^{\circ} 19' 20''$, and with it, if I am right, the zone of barometric maximum.* But in the sun, as we see, that zone is on the equatorial side of $20^{\circ} 19' 28''$.

If my reasonings are correct, were the sun's atmosphere acted on only by the meteors, the barometric maximum

* The frictional force of a wind is a function of its velocity and the nature of the surface it passes over; but we have reason to believe that the sun's surface is everywhere alike, being everywhere liquid from the intense heat.

would be at the equator; were it acted upon only by the forces that act on the earth's atmosphere, the barometric maxima would be on the polar side of Lat. $20^{\circ} 19' 20''$ (in the earth's atmosphere they are about 28°); but they are intermediate between the two, about Lat. 15° , I infer from this, in addition to other facts, that the sun's atmosphere is acted on by both sets of forces, and that the observations tabulated by Mr Carrington show a resultant effect from the two.

The solar spots are most numerous in the zones north and south of the equator, and never appear near the poles; they are seldom seen on the equator itself. It is very probable that they are cyclones, and we know that cyclones cannot be formed on a planet's equator, though they may drift on to it. But this will not account for their absence near the poles; on the contrary, were all other things equal (which, however, is not the case in the earth's atmosphere), the tendency to the formation of cyclones would be greatest at the poles; as it is, there the rotation of any planet is most rapid in relation to an axis drawn perpendicular to its surface. The production of spots in the lower latitudes is probably due to the greater number of meteors that fall in there, causing greater mechanical disturbance, as well as by the greater heat of those latitudes, which must give rise to a more energetic vertical circulation of the atmosphere. Such vertical circulation is certainly proved to exist by the phenomena of the sun's atmosphere, especially by the "rose-coloured protuberances" seen during solar eclipses, which are in all probability cumulus clouds.

The following short *resumé* of the most novel and important points of this paper was read as a communication from me in Section A of the British Association at Newcastle, 1863.

"Secchi of Rome has ascertained that the sun's equator is sensibly hotter than his poles. That this should be the case follows from the meteoric theory of solar heat. The asteroids which revolve round the sun and fall into its atmosphere as meteors, probably occupy, like the entire solar system, a lenticular space having its greatest diameter nearly coincident with the sun's equator, and if so, a greater num-

ber of meteors must fall on the equatorial than on the polar regions of the sun, making the former the hottest. The meteoric theory will also account for the currents in the sun's atmosphere observed by Mr Carrington. He finds that the spots in the lowest latitudes drift most rapidly from W. to E. Were the sun's atmosphere, like the earth's, acted on by no other motive-power than the unequal heating at different latitudes, the relative direction of the currents would be the reverse of this, in virtue of the well-known principles of the trade-winds and "counter-trades," and this would be true at all depths in the sun's atmosphere. But if meteors are constantly falling into the sun's atmosphere, moving from west to east with a velocity scarcely less than that of a planet at the sun's surface, and in greatest number in its equatorial regions, there is a motive power which is adequate to drive its atmosphere round it from west to east, and with greatest velocity at the equator. The intensely bright meteor-like bodies, which Mr Carrington and another observer simultaneously saw traverse the sun's disc, moved from west to east, and they were almost certainly asteroids falling into the sun."

Remarks on the Sexuality of the Higher Cryptogams, with a Notice of a Hybrid Selaginella. By JOHN SCOTT, Royal Botanic Garden, Edinburgh.*

Modern researches, on the reproductive phenomena of Cryptogams, have induced a number of botanists to accept the doctrine of their sexuality, this function being attributed to the organs known as the Antheridia and Pistillidia. Amongst those botanists who deny the sexual hypothesis, as applied to Cryptogams, a difference of opinion exists; one class attributing a sexual function to the above organs as occurring in the genera *Pilularia*, *Marsilea*, *Salvinia*, and *Isoetes*, but strangely arguing, that such an import cannot possibly be attributed to these organs in the other orders; while another class,—with a more consistent scepticism,—

* Read before the Botanical Society of Edinburgh, 10th March 1864.

refuse to attribute a sexual import to these organs in any order of the class, and regard all as strictly agamic.

It would be mere surplusage, on my part, to give to the Society even the briefest *resumé* of the nature of the evidence on which the sexuality of Cryptogams is based, inasmuch as the writings of Henfrey, Berkeley, Suminski, Hofmeister, &c., have rendered it sufficiently familiar to all, and must satisfy all who have accepted the doctrine that nothing short of hybrids, artificially produced between distinct species of Cryptogams, will induce a universal acceptance of the hypothesis of sexuality as applied to these plants.

Several supposed instances of hybridity have been recorded by authors, but these not being results of direct experimentation, do not by any means place the question beyond the reach of doubt. For example, Hofmeister, in his work "On the Higher Cryptogams," p. 181, states that Bayrhoffer "suggested certain mosses, found by him growing wild, were hybrids between *Gymnostomum pyriforme* and *G. fasciculare* on the one side, and *Funaria hygrometrica* on the other side." Hofmeister, however, remarks that he "has not yet succeeded in producing such hybrids experimentally, although he brought together antheridial plants of *Gymnostomum pyriforme* and plants of *Funaria hygrometrica*, with their antheridial shoots cut off. The mutilated plants of *F. hygrometrica* always perished."

In the case of ferns, it has been asserted that true hybrids exist in the genus *Gymnogramma*. Braun, in his "Plantarum novarum et minus cognitarum adumbrationes," notices several supposed hybrids belonging to the above genus which have appeared in gardens; and similar notices have from time to time appeared in the "Gardeners' Chronicle." That now well known segregative individualising power of the fern-spore—if I may term that subordination of the specific formative tendencies in that organ to those casual variations of the segments or pinnæ upon which it originates—ought to make us extremely cautious in ascribing a hybrid origin to any forms that may appear amongst these plants. Furthermore, the hermaphrodite nature of the prothalli, and the juxtaposition of the antheridial and archegonial cells,

render the occurrence of hybrids, in the *true* ferns, much less probable, I believe, than in any other order of Cryptogams. The Botrychiums and Ophioglossums, as shown by Hofmeister and Mettenius, afford much higher facilities for successful casual hybridization than occurs in the true ferns. Inasmuch as in the former the antheridial and archegonial cells occur on opposite sides of the prothalli, so that an equal, or even higher facility, is thus afforded for the conjunction of distinct individuals than the pure hermaphrodite conjunctions. In the latter—or true ferns—on the other hand, where the antheridial and archegonial cells are produced upon the same side of the prothallus, and this being the under, an examination of the individual relations of the prothalli in a single pot will, I think, suffice to show that the crossing of distinct individuals must here be a most exceptional occurrence; unless, indeed—as so generally occurs in the higher plants—nature has provided certain external agents.

In the Selaginellas, the only genus of the Lycopodiaceæ whose reproductive phenomena are known,* the greatest possible facilities are afforded for hybridization by the unisexual characteristics of their spores, and their production in distinct organs; one kind of spore—microspore—producing spermatozoa; the other—macrospore—producing the archegonial cells. From these relations of the reproductive organs, it might be supposed that hybrids would be easily raised experimentally between different species. The only points to be studied being a slight regard to systematic affinities, and the relative time required for the development of the

* Hofmeister has the following remarks on the above point:—"The reproduction of those Lycopodiaceæ which bear powdery spores of one kind only, is still a mystery. Repeated sowings of the spores of *Lycopodium clavatum*, *inundatum*, and *Selago*, have yielded me no results; but I have lately often observed, that in spores of *Lycopodium Selago*, which had been sown for from three to five months, numerous small spherical cells had been formed, similar to the mother-cells of the spermatozoa of *Selaginella helvetica*. I have not yet found spermatozoa inside these vesicles. De Bary has lately discovered that the spores of *Lycopodium inundatum* produce a body composed of a few cells, whose structure is not unlike that of the archegonium of a fern. It is probable, from these observations, that the similarly formed spores of *Lycopodium*, *Psilotum*, &c., are of different sexes, and, as in *Equisetum arvense*, produce partly *archegonia* and partly *spermatozoa*."—"On the Higher Cryptogams," p. 398.

spermatozoa and archegonial cells.* I have found, however, that this is far from being the case; for, after numerous experiments, the subject of the following remarks is the only one to which I can with certainty assign a cross origin. The history of this plant may be thus briefly told:—I placed *thirty macrospores* of *Selaginella Danielsiana* on the surface of a pot of moist sand; over these I strewed thickly the *microspores* of *Selaginella Martensii*, and then closely covered all with a small bell-glass. In case of differences in the time required for the perfect development of the male and female organs of the respective species, for some time after the first sowing, I frequently added fresh microspores of the latter species, *S. Martensii*. Ultimately *one* of the macrospores produced a *germ-plant*, *all the others proving abortive*. The gradual development of this germ-plant I have watched with interest, and I have now the pleasure, through the kindness of Mr M'Nab, of placing it and its parent forms upon the table for the examination of this Society.

Previous to my noticing the individual and relative characteristics of hybrid and parents, there are one or two other points on which I beg to make a few remarks, by way of obviating certain objections which may be advanced against the hybrid nature of my seedling; they are as follows:—A. Braun ("Plantarum novarum et minus cognitarum adumbrationes," 1857, Appendix, p. 16) considers that *Selaginella Martensii* and *S. Danielsiana* are conspecific; and taking the former for the normal or typical form of the species, calls it *S. Martensii normale*; the latter *S. M. compacta*. Three other forms, considered by some as distinct species, have also been referred by Braun to *S. Martensii* under the following names:—*S. M. flaccida*, *divaricata*, and *congesta*.

* A single illustration will show the necessity for attending to the period required for the development of the spermatozoa and archegonia in the species tried. Thus, in the closely allied *Selaginella denticulata* and *S. helvetica*, the spermatozoa and archegonial cells are developed in the former species about six weeks after sowing; whereas, in the latter species, according to Hofmeister, the *microspores* lie *five months*, and the *macrospores* between *six and seven months*, before they produce their respective spermatozoa and archegonia. We thus see that here, as elsewhere, in the vegetable kingdom, other points than recognised systematic affinities must be attended to in hybridizing.

Mr Moore informs me by letter that he is inclined to agree with Braun in uniting under *S. Martensii* the forms he has so placed; and furthermore, considers that the seedling form which I have raised goes to confirm this view, by showing that varying forms are capable of being produced from the spores; that, in fact, so far as he could judge from the pressed specimen (which I sent him for examination), I had merely produced *S. M. normale* from the *S. M. compacta*. Mr Moore continues, however, that in plants so peculiar as these Lycopods, a good deal of their natural appearance is lost under pressure. In the present instance, the Society will observe, by a comparison of the hybrid and parent plants with the pressed specimens upon the table, the truth of Mr Moore's remarks, as respects the affinities in judging from the dried specimens alone, and, moreover, the need for that express reservation added to the above view, inasmuch as it is at once obvious by a comparison of the living plants, that though nearer *S. Martensii* in the characters of the leaves, it—the hybrid—has much more affinity with *S. Danielsiana* in its general habit.

In consequence, then, of this view of Braun and Moore, respecting the conspecificness of the parent forms, I can only give a provisional significance to the hybridity of my seedling; satisfied, however, that even by an ultimate agreement amongst systematists as to the genetic affinities of the parent forms, it will simply cause a substitution of the term "mongrel," for that of "hybrid," at present given. And thus, that in either case, it will afford a stronger argument in support of the sexuality of the higher cryptogams than any, so far as I am aware, which has yet been recorded.*

On the supposition, however, that Braun has rightly regarded the parent forms of my seedling as conspecific, it may

* Mr Moore, in answer to a query as to the occurrence of undoubted hybrids amongst the above plants, writes me as follows:—"I am not aware of any well authenticated instances of hybridization among Cryptogams. I have always regarded the varieties of *Gymnogrammas* (which do sometimes present an appearance intermediate between two known sorts) as sports—chiefly, however, from the want of any direct evidence of hybridity."

be argued that as the *S. Danielsiana*—*S. M. compacta*—is the more incipient form of the two experimented upon, it may yet have a *tendency* to produce, by a *truly parthenogenetic process*, a varying offspring from its spores. To this I can answer only as follows, but the answer, I think is satisfactory. *First*, When the *macrospores* of the *S. Danielsiana* and *S. Martensii* are sown *alone, neither*—and I speak from an extensive series of experiments—*will produce a SINGLE plant*; clearly demonstrating, as I think, a sexual reproduction dependent on the mutual action of both kinds of spores. That consequently *parthenogenesis*, in so far as my experience goes, *does not occur in either of these forms*; nor indeed in any of the species of *Selaginella* which I have tried, if sufficient care be taken to exclude the *microspores*. Again, *secondly*, When the *microspores* and *macrospores* of the *S. Danielsiana* and *Martensii* are each purely commixed and respectively sown in distinct pots, they reproduce themselves perfectly, as I have in several instances proved by experiments. That the Society may be enabled to judge as to the truth of this statement, I have placed upon the table seedling plants of both forms, all of which betray at once their respective parents. Conjoining, then, the latter with the foregoing evidence, *i.e.*, the non-development of the *macrospores* when sown alone, and the facility with which both forms reproduce themselves when *the two kinds of spores* are mixed; and comparing them with the previously given history of the presumed hybrid, we are thus, as I am inclined to think, afforded, *firstly*, most conclusive evidence of the existence of true sexual organs in these plants; and, *secondly*, indubitable proofs of the mixed origin of the seedling plant.

Let us now see in how far this view of the mixed origin of the seedling plant is supported by an individual and comparative examination of the morphological characteristics of the latter and its parent forms. First, for the individual characteristics:—

1. *Selaginella Martensii*.—*Spike* sessile, linear, somewhat attenuated, from 8 to 10 millimetres long. *Bracteas* ovate, acuminate, denticulate. *Microsporangia* ovate, subtruncate, tumid, $\frac{1}{2}$ of a millimetre. *Microspores* reddish-orange, $\frac{1}{16}$ of a millimetre, somewhat wrinkled and granulated.

Macrospores, greyish-white, $\frac{1}{2}$ of a millimetre, reticulated. *Stem* ascending, flexuose at the extremity, branches spreading. *Leaves* oblong-ovate, oblique, falcate, somewhat blunt, 3 to 4 millimetres long; anterior base sub-dilated, margin ciliated, posterior base rounded, margin denticulated. *Stipuliform leaves* oblong or oblong-ovate, acuminate, denticulate, carinate, recurved, 2 millimetres long; exterior base auricled, and bordered with a few long hairs; interior rounded, and nearly entire.

2. *Selaginella Danielsiana*.—*Spike* sessile, short and thick, 6 to 7 millimetres long. *Bracteas* ovate, acute, denticulate. *Microsporangia* oblong, tumid, $\frac{3}{4}$ of a millimetre. *Microspores* brownish-grey, wrinkled and granulated as in *S. Martensii*, $\frac{1}{8}$ of a millimetre. *Macrospores* white $\frac{2}{3}$ of a millimetre, reticulated. *Stem* ascending, branches short, rigid, erect, sub-fastigiate. *Leaves* ovately-oblong, 4 to 5 millimetres long; anterior base dilated, and sparingly fringed with long hairs; posterior base sub-truncate, margin entire. *Stipuliform leaves* ovate, acuminate, carinate, recurved, 3 millimetres long; exterior base auriculate, margin sparingly ciliated; interior base rounded, margin entire.

3. *Selaginella Danielsiana-Martensii*.—*Spike* sessile, short and thick, 3 to 4 millimetres long. *Bracteas* ovate-triangular, shortly mucronate, denticulate. *Microsporangia* ovate-oblong, half a millimetre long, tumid. *Microspores* brownish-grey, finely granulated, size very variable, from the 38th to 30th of a millimetre; a high percentage apparently imperfectly developed. *Macrospores* white, $\frac{1}{2}$ of a millimetre, obscurely reticulated. *Stem* ascending, branches numerous, short, rigid, erect, and sub-fastigiate. *Leaves* oblong-ovate, bluntish, slightly oblique, 2 to 3 millimetres long; posterior margin denticulated; anterior margin entire. *Stipuliform leaves* lanceolate-ovate, shortly mucronate, carinate, 1 to 2 millimetres long.

Again, secondly, by a relative comparison of the hybrid and parent forms, we have something like the following results:—*First*, in the short, erect, rigid, and somewhat fastigiate branches (destitute of any principal or leading shoots) of the hybrid plant we have a marked characteristic of the female parent, the *S. Danielsiana*. As in the latter species, the right and left forks of the terminal bud are in

general imbued with an equal degree of the vegetative force, so that both forks being developed alike, the plants thereby assume a dwarf, compact, bushy habit. In the male parent—*S. Martensii*—on the other hand, the right and left forks of the terminal bud are alternately more vigorously developed, so as to give rise to an apparently principal axis, or leading shoot, with a right and left series of branches, and a lax, somewhat spreading habit to the plants. *Secondly*, In the form of the leaves, and their somewhat lax rachidal disposition, the hybrid exhibits more affinity with the male than the female parent, the only difference being a decreased size. In the denser cellular structure of these organs, however, and likewise in the deep lustrous green, with the brownish-tinted stems, the hybrid again approaches the female parent. In the form of the stipuliform leaves and bracteas, it differs from either parent, and here approaches another of the forms which Braun has referred to the *S. Martensii*, viz., *S. M. congesta*. *Thirdly*, In respect to the characteristics of the organs of fructification, there is a great similarity in the three forms, those of the hybrid being the smallest. There is one point, however, in connection with them, worthy of a passing notice, namely, the relatively great variability in the sizes of the microspores of the hybrid—a high percentage of which are badly developed—as compared with those of the parents; while the macrospores, though smaller than those of the latter, present in general very trifling relative differences, and so far as I can judge, until I have time to test their germinative capabilities, perfectly developed. We have here a curious and interesting—real or apparent—analogy, with that which occurs in the phenomena of sterilisation in the hybridisation of the higher plants. Hybridists have shown, that in the latter class of plants, the pollen is more susceptible to the sterilising action than the ovules, and that in general, perhaps invariably, as has been maintained, we find that if the anther-cases contain a few grains of perfectly developed pollen, the ovaries also will contain a higher percentage of ovules capable of fertilisation.*

* I believe an exception, of which I will satisfy myself at the approaching

On the Chemical and Natural History of Lupuline. By M. J. PERSONNE. Translated by GEORGE LAWSON, LL.D.,* Professor of Chemistry in Dalhousie College, Halifax, Nova Scotia. (Plate II.)

Note by Translator.—Considering the great importance of the hop in an economical point of view, we might expect our scientific and manufacturing works to contain a somewhat satisfactory statement of the chemical products of the hop, and of the nature and development of the remarkable organ by which these products are secreted. This, however, is far from being the case; and intelligent brewers in Canada, puzzled by the contradictory statements that have been put forth, have frequently applied to me for information on this as on other scientific points connected with their art. I have therefore thought that a translation of M. Personne's Memoir, published some years ago in the "*Annales des Sciences Naturelles*," might not be without its use. In some of its bearings, the subject is of much interest in a strictly scientific point of view. It is obvious, likewise, that an acquaintance with the chemical properties of Lupuline is important, not only to the brewer, but to the hop-grower, the exporter, the manufacturer of hop-extract, and, indeed, to every one who has to handle an article so prone to change its character, and, consequently, its commercial value, from apparently trifling causes. The Canadian brewers having a favourable grain-market, and an unlimited supply of excellent water in the great lakes, almost entirely devoid of organic matter, have the means of manufacturing excellent beer. But much of the hops used requires to be imported from England. Canadian hops are grown to some slight extent at Kingston, more abundantly about Picton, and Belleville, C. W., and especially farther to the westward; but the best qualities of hops are always imported. The Canadian hop gives greater

flowering period, to the above law, occurs in the bigeneric hybrid of the *Rhododendron Chamæcistus*, and the *Menziesia empetrifolia*—the *Bryanthus erectus* (Graham), inasmuch as I have found apparently well-developed pollen grains in the anther-cases, yet I have repeatedly failed in fertilising this plant with its own pollen, or that of either parent.

* Read before the Botanical Society of Edinburgh, 10th March 1864.

bitterness, but is deficient in delicacy of aroma. Were pains taken (and I have reason to believe that hitherto they have not been taken) to select suitable varieties from the Kentish hop-gardens, and to ascertain, more precisely than we as yet know, what are the special influences of certain soils and climates, no one can doubt but that a great improvement would result in the character of Canadian hops. All attempts in this direction must proceed upon a correct knowledge of the nature of the substances which give the hop its economical value; and although M. Personne's memoir is more complete and satisfactory than any other that has been published, yet it is to be hoped that by again calling attention to the subject, additional information may be obtained on points that are still imperfectly made out.

The cones of the hop (*Humulus Lupulus*) employed in therapeutics, and especially in the manufacture of beer, owe their properties to a multitude of yellow corpuscles, resinous and odorous, which are separated very freely in bruising the ripe and dry cones. These small bodies have been successively called by the names of Lupulin, Lupuline, and Lupulite. It is to these that the hop owes its bitter and aromatic flavour; for if the scales and the fruit are deprived of this yellow powder, the cones lose those properties on account of which they are sought after.

The importance of this substance has been known for a sufficiently long time. In 1821, Dr Ives of New York attempted to determine its principal constituents, and endeavoured to introduce it into therapeutics under the name of Lupulin. In France, almost about the same time, Planche likewise concluded that it was a proximate substance, and named it Lupuline, because, said he, "This substance is to the hop what quinine is to cinchona or strychnine to nux-vomica."

In 1822, MM. Payen and Chevallier made the most complete chemical analysis which we have of this substance. They thereby demonstrated the complex nature of lupuline, and, consequently, the error of Planche; but the small quantity of substance upon which these chemists worked,

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did not permit them to study sufficiently well the bodies which they had obtained from it.

Lastly, in 1827, M. Raspail published, on the organisation of lupuline, the unique work which exists on this subject. That author sought to demonstrate the analogy of this body with the pollen, as much by the investigation of its structure as by that of the action which the various solvents and chemical reagents exercised upon it. He designated it under the name of *pollen of the foliaceous organs*, "because its office," said he, "is to fecundate the bud, just as that of the pollen of flowers is to fecundate the ovary." I review farther on the observations of M. Raspail.

Structure and Development of Lupuline.

The lupuline obtained from cones that have arrived at maturity presents itself in the form of a yellow powder, whose tint varies according to the length of time which has elapsed since it was gathered. In the fresh state, it has a greenish-yellow colour, which afterwards passes into a golden yellow, deepening more and more the longer it is kept, especially when exposed to contact with air. The form of the lupuline, when it has arrived at its complete development, may be compared to that of an acorn with its cup. Just as some acorns are more or less lengthened at the base, so also some of the grains of lupuline are more or less elongated. The length of these grains varies between $\frac{1}{8}$ ths and $\frac{3}{8}$ ths of a millimetre, and their thickness between $\frac{1}{8}$ ths and $\frac{1}{4}$ ths; but in general the two parts of the lupuline, the superior and inferior, are strictly proportional. We shall later see the reason.

In comparing the lupuline with an acorn, I do not mean to say that it is, like it, composed of two solid parts, one of which encloses the base of the other. The comparison can only be applied to the external form, for they differ in all other respects. In fact, the surface of the two parts, superior and inferior, of the lupuline, is perfectly continuous, only the superior, at its point of insertion on the inferior, is bent a little inwards towards the centre, and it is the slight curve which it makes that gives it the acorn form.

These two parts present on the exterior, even under a magnifying power of from 200 to 300 diameters, a structure apparently similar. Both appear to be composed of cellules more or less irregular, which, however, are frequently disposed with a certain regularity from the centre to the circumference; they are sometimes ranged in radiating series from the summit of the superior part, and from the base of the inferior to the circumference or median line, which unites them. The cells, therefore, increase in size from the two extreme points to the (median line) point of junction. But as I said just now, this structure is only *apparent* in the upper half; because if we succeed in making a longitudinal section in the direction of the axis of the grain of lupuline, and adjust the same, when placed under the microscope, in such a manner that the plane parallel with its axis shall be in the focus of the instrument, it will be seen that the lower half of the grain is a sort of cupule, composed of a single layer of cells. It is by the base of this cupule that the grain is attached to the epidermis of the bracts, calycine leaves, &c. It is observed, besides, that the upper half consists only of a very thin continuous membrane, and that the cells, which are depicted upon its surface, are nothing more than the imprints of utricles, the origin of which we give further on in describing the formation of this organ, this singular gland. The space embraced between this membrane and the interior of the cupule is occupied by a yellow liquid, the nature of which we shall examine fully farther on. The cellules which compose the cupule are also filled; it is these that secrete it, as we shall presently see.

One sees already that this description of lupuline differs essentially from that given by M. Raspail in his "New System of Organic Chemistry," 1833, page 175. Here, in effect, is what he says:—"Examined by the microscope, this yellow powder (the lupuline) is seen to be composed of vesicular organs, rich in cellules, varying in size about the $\frac{1}{3}$ th of a millimetre, and of about the form of that represented in figure 6 of plate v. (of his work.) Each of these grains, when dried, is of a beautiful golden yellow, somewhat diaphanous, flattened, presenting on some

part of one of its two surfaces the mark of its point of attachment, by which the grain has been originally attached to the organ which produced it, which mark I usually designate by the name of hilum. . . . When these grains are examined, as recently obtained from the still living female cone, they are found to be pyriform, with a peduncle terminated by a hilum," &c.

And farther on, § 387, pp. 176, 177, M. Raspail attempts to prove that the grains of lupuline emit pollen tubes, and that these are produced in contact with water. The conclusion of this paper will show the cause of the error of this observer.

Let us now study the origin of lupuline.

It commences like a hair, by one cellule *l* (fig. 3, Plate II.), which is developed between cells of the epidermis *e*. This cellule, projecting to the exterior, is divided by a transverse partition at the level of the external surface of the epidermis. The utricle *a*, ovoid or elliptical, which results from this division, is in its turn divided transversely (fig. 4, *a*). The two new utricles enlarge; the superior *a* (fig. 5) is more dilated than the other, and is filled with somewhat granular matter; the inferior *p* forms a short pedicel, which unites the former to the epidermis *e*, by means of the primitive cell *l*. Thus far the multiplication goes on by transverse division; it now proceeds vertically. The terminal cellule *a* divides longitudinally into two, as shown by figure 6 at *a*. Each of the two utricles which thus originate produces in its turn, either one after the other (figs. 7 and 9), or simultaneously (figs. 8 and 10), two cellules, so that by this time the pedicel *p* is terminated by three cells (fig. 7), or by four, as in figure 8. The figures 11 and 12 show more advanced stages of this subdivision. There now appear some new utricular elements in the interior of the terminal cells. Figure 13 presents a degree of multiplication still more advanced; in it may be clearly observed, in *a a a a*, the four terminal cells of figure 8, and that they have divided in a radial manner and parallel to the circumference. In figure 14, which indicates a later phase, may also be observed the four original divisions; but the cells of each of these are

still more numerous than in the preceding figure. It not unfrequently happens that the utricular multiplication parallel to the rays is more marked than that which occurs in the other direction, in which case the section appears as in figure 15. It is at this stage of development of the lupuline that its edges become raised; then from the discoid state it becomes cup-shaped. Figure 16 represents some of these cupules which are almost arrived at the perfect state. They have longitudinal striæ, interiorly and exteriorly; that is to say, in the direction of the utricular multiplication parallel to the rays. These elegant cupules appear sessile in consequence of the pedicel not being elongated.

When the enlargement of the cups has ceased, other phenomena take place in the interior of their tissues. Each cupule consists, at this time, of a layer of cells, which is covered with a cuticle on its two faces, the interior and exterior; then commences the secretion of the yellow liquid before mentioned. It is poured out on the whole internal surface of the cupule between the secreting cells and the cuticle which covers them. The latter, detached from the cells by this flowing, is gradually raised completely from the whole extent of the internal surface (fig. 17 *d*), and finally pushed up like the finger of a glove; it is now that the lupuline takes the form of an acorn (figs. 18 and 19), to which I have compared it; it is then arrived at its most perfect stage of development.

It is curious to observe under the microscope the rising of the cuticle. It may be caused artificially, by placing the cupules in water slightly alkalised, which penetrates their walls better than pure water. They may be seen successively passing through all the intermediate stages between the form *l* of figure 16 and that of figure 18.

If we examine the fresh but perfectly developed lupuline in water, it is seen to swell gradually, becoming turgid by endosmose, then all the cells of the cupule appear as a perfect network, and it is then evident that the imprints marked upon the cuticle disappear almost completely. The enlargement increases to the point of bursting of the grain, and it then emits a perfect cloud, formed by a multitude of



small globules of essential oil ; it frequently happens that these globules, by uniting, form a globule somewhat large, which is very well seen on the summit of the grain in front of the rent.

This rent is generally made at the junction of the cuticle with the edge of the cupule. The cuticle is raised, as a cover, and, as the cupules open, the cuticle is detached, and swims away in the surrounding liquid. Occasionally during this action it occurs as much in the wall of the cuticle as in that of the cupule, according to their greater or less resistance.

An alkaline solution and alcohol act more rapidly than water, because, by dissolving more readily the resinous matter which impregnates the walls of the grains, they render the penetration more easy.

It has never been possible for me to observe the pretended pollen tubes seen by M. Raspail, in examining the fresh lupuline. But if we examine lupuline that has been kept for some time, we observe a very few grains which are with difficulty impregnated with liquid in this or that place, and which, breaking a long time after most of the others, permit the exudation of a viscid matter. This matter, moulding itself in the aperture which gives it passage, slightly resembles, to a certain extent, a pollen tube, and it was this most probably that was seen by M. Raspail; but it requires only a slight examination to account for the appearance, which is most certainly due to the interior matter of the grains having been dried dissolving with difficulty.

The lupuline is produced on the ovaries, on the inferior surface of the bracts and on that of the leaves. It is equally met with on the stem and on the stipules ; but it is only on the ovary and on the scales of the cone that the lupuline arrives at its complete development. On the leaves, on the stipules, and on the stem, it is never met with except in the state of cupules more or less advanced, or all simply of discs, which readily wither up and are shed.

The lupuline is then a gland, which contains a complex liquid, of which we now proceed to investigate the nature.

Chemical History of Lupuline.

The matter contained in the gland, which I designate by the name of *Lupuline*, has a very complex composition ; its constituent principles may be classed into two groups : the one embracing those that are volatile, and are obtained by distillation with water ; the other those that are fixed, or at least not volatile with steam.

Examination of the Volatile Principles.

The product of distillation consisted of a solution decidedly acid, which reddened tournesol paper, and upon which floated an essential oil, coloured occasionally of a most beautiful green.

The proportion between the quantity of essential oil and of acid of the liquor distilled varied according to the quality of lupuline employed in the operation. Besides, the lupuline when as fresh as possible, furnished at once a less acid liquor and a greater quantity of essential oil than the older lupuline, which gave, on the contrary, more acid and less essential oil ; the latter is likewise drier and more resinous than that obtained with the freshest lupuline.

The quantities of essential oil which I obtained with the lupulines of different ages, have given me the following proportions :—With recent lupuline I obtained as much as 1 from 100 of the essence, while with older lupuline I have had not more than 0·61 from 100, that is near my proportion.

Volatile Acid of Lupuline.

If we next separate the essential oil from the acid liquid obtained, as I have described, by distillation of lupuline with water, and saturate the liquid with some carbonate of soda, and then evaporate to dryness, it yields as residue a mass of a soapy nature, which liquefies by heat and becomes very solid on cooling, it is with difficulty permeable by water, which, however, ultimately dissolves it completely ; it, in short, comports itself like the compounds of fatty acids with alkalies.

This mass, dissolved in a small quantity of water and

then treated with sulphuric acid diluted with its weight of water or with gelatinous phosphoric acid, yields some sulphate or phosphate of soda which remains in solution in the aqueous liquid, to the surface of which is seen to float a brown oily liquid, diffusing a strong and disagreeable odour of butyric and valerianic acids.

Subjected to distillation this liquid furnished, by many successive rectifications, a product which boiled at $+ 175$ degrees ($= 347^{\circ}$ Fahr.), and distilled without alteration at this temperature; the first portions carried over water in excess, which was thus separated with sufficient ease.

This acid, obtained in a state of purity, is a liquid, slightly oleaginous, very fluid, colourless, with a strong and persistent odour of valerianic acid; its flavour is acid and piquant; it produces a white stain on the tongue in the manner of energetic fatty acids; it is not solidified by a cold of -16 degrees ($= +3^{\circ} 2$ Fahr.), and remains perfectly limpid; it burns readily with a smoky flame. The specific gravity of this acid is found to be 0.9403 at $+15$ degrees ($= 59^{\circ}$ Fahr.). It corresponds to that of valerianic acid, which has been found to be 0.937 at $+16.5$ ($= 61^{\circ} 7$ Fahr.)

I omit here the description of all the analyses which I have made for ascertaining the composition of this acid. All lead to the formula of valerianic acid. I have purposely multiplied its combinations with oxide of copper, oxide of silver and baryta, in order to be well satisfied of its true constitution. But the odour alone of lupuline, especially of that which has been kept for some time, does not admit of doubt of the existence of this acid among the bodies which this substance contains.

Volatile Oil of Lupuline.

This crude essential oil—that is to say, such as has been given by distillation of lupuline with water—is an oleaginous liquid, more or less fluid according to the state of the lupuline which furnished it, and of a specific gravity less than that of water. It has at the same time a somewhat intense colour of yellowish green, more frequently of a beautiful green; its odour recalls slightly that of the hop;

but this odour does not resemble that of valerianic acid when the oil has not undergone oxidation or contact with air.

Subjected to distillation, it enters into ebullition at +140 degrees (=284° Fahr.), and distils for some time at +150° (=302° Fahr.) to 160 degrees (=320° Fahr.), but the temperature rises gradually, and when the process is finished, is +300 degrees (=572° Fahr.)

The portion of this essence obtained between 150° (=302° Fahr.) and 160° (=320° Fahr.) is a sufficiently thin liquid, slightly amber-coloured, of an odour which does not resemble that of the hop, and of a specific gravity of 0.8887. It has not an acid reaction, but, on exposure to air, it acidifies and becomes resinous; it is slightly soluble in water, to which it communicates its odour, and the solution exposed to the air acidifies rather readily; it is soluble in alcohol and in ether. With a cold of -17 degrees (+1°.4 Fahr.) it lost a little of its fluidity, but its transparency was not altered, even after four or five hours' exposure to that temperature. It deviates to the right the rays of polarized light. Its rotatory power (Dextrogyrate) has been found by the red glass to be +2.7° for the length of 0^m.080; it is then of

$$\frac{+2.7}{80 \times D}$$

Nitric acid gives at first a beautiful purple colour; afterwards, if heated a little, the reaction becomes more lively, and the products furnished are a resinous matter and valerianic acid.

Potash in solution fails to attack it at a boiling temperature; but if we form an emulsion with a concentrated solution of potash, and expose the mixture for some time to contact with air, we find that there are produced valerianate of potash and a resinous matter.

Fused potassa transforms it into carbonate and valerianate of potassa, with disengagement of hydrogen and of a hydrocarbon liquid.

This reaction of potassa is very important, because, after some useless trials, and a great number of analyses, it rendered clear the true nature of this essence, placing it by the side of the essential oil of valerian.

In fact, the composition obtained by analysis of the crude essence, may be represented by the formula $C_{56}H_{44}O_6$; that of the essence distilled between $+150$ and 160 degrees (302° and 320° Fahr.) by the formula $C_{22}H_{18}O_2$.

In submitting these essences to the action of fused potassa, there were obtained products in which the quantity of carbon and of hydrogen increased each time that they were submitted to a renewed action of potassa, while the proportion of oxygen decreased. Finally, after many successive treatments, we finished by having a perfectly pure hydro-carbon.

This hydro-carbon is a colourless liquid, which boils at $+160$ degrees (320° Fahr.) It does not acidify by contact with air; it is as difficult to be altered by contact, for a score of days, with pure oxygen. Its composition, deduced from analysis, is represented by the formula $C_{10}H_8$; it is consequently the same as that of the oil of turpentine and of *bornéène*, which M. Gerhardt has found in the essential oil of Valerian. But this body, although possessing the composition of oil of turpentine and of *bornéène*, is not the same, but isomeric with these last; for I have not transformed them into solid camphor of Borneo, neither by the action of nitric acid nor by that of potassa. Kept for some time on a solution of potassa, it acquired the odour of thyme, sufficiently to show an approach to thymol.

We see that the action of fused potassa upon the essential oil of hop, consists in setting free a hydro-carbon liquid $C_{10}H_8$, and in retaining an oxygenated body, which it transforms into valerianic acid and carbonic acid; results absolutely similar to what M. Gerhardt has obtained with the essence of valerian.

It is not easy to separate the oxygenised principle of this essential oil, because it is found to be retained in the thickish resinous matter, which does not permit the separation without great difficulty.

The essential oil of lupuline is clearly, then, to be considered as a complex oil, constituted by a hydro-carbon $C_{10}H_8$, and a body containing oxygen of the formula $C_{12}H_{10}O_2$ analogous to *valerol* of the essential oil of valerian. The formula of the crude oil $C_{56}H_{44}O_6$, may be represented by

3 ($C_{12}H_{10}O_2$) + 2 ($C_{10}H_8$); that of the oil rectified between +150 and 160 degrees (302° and 320° Fahr.) by $C_{12}H_{10}O_2 + C_{10}H_8 = C_{22}H_{18}O_2$.

The process by which it may be obtained as free as possible from extraneous matter, consists in preparing a tincture of lupuline with alcohol, of 36 degrees; to treat this liquid with an alcoholic solution of tartaric acid, which forms a precipitate somewhat abundant, of bitartrate of ammonia. The liquid separated from the precipitate, is added to a little water, and submitted to gentle heat in a capsule exposed to the air; the alcohol, in evaporating, leaves separate, at the end of two or three days, the resinous matter of the solution, acid and bitter. This bitter liquid is then deprived of the excess of tartaric acid which it contains, and then made to digest with some carbonate of lead recently precipitated; the mass, evaporated at the lowest possible temperature, is treated by boiling alcohol, which dissolves only the bitter matter.

Resinous Matter.

The resinous matter is very abundant in lupuline; it forms itself alone nearly two-thirds of its weight; it retains always a certain quantity of the volatile oily products, which gives to it a variable consistence, and preserves at the same time the peculiar odour of lupuline. It is oxidized by contact with air, especially in presence of water, and its colour then passes from a golden yellow to a deep brown tint, at the same time that it hardens. It is largely soluble in water, to which it communicates the property of lather by agitation. This solution presents an acid reaction, and is completely altered by evaporation in contact with air.

The alkalies dissolve it in the cold, and separate an insoluble part. This resin, insoluble in the alkalies and in water, is soluble in alcohol; it is dry, friable and inodorous. The alkaline solution, saturated by an acid, sets free the resinous matter with its original properties, and retaining some valerianic acid which is got by distillation. Lastly, nitric acid with heat attacks this resin with energy, but without producing special reaction which would serve to characterise it.

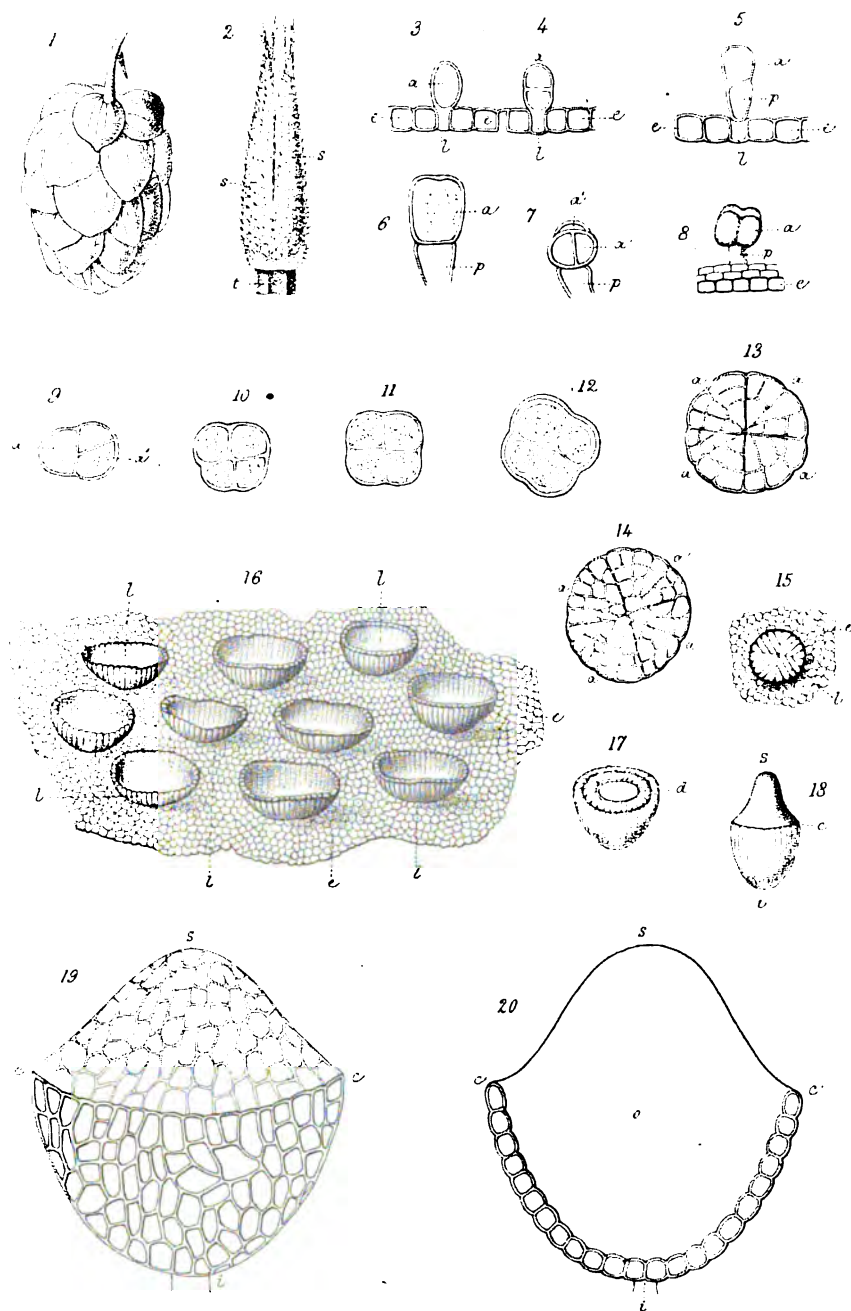
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To obtain this resin as pure as possible, the lupuline must be exhausted by long boiling in water, which drives off the volatile products, and dissolves the bitter matter. The insoluble residue, composed of resin and of disintegrated tissue of the lupuline grains, well washed and dried, is then treated by boiling alcohol, which sets free, when cooled, a certain quantity of *waxy matter*; the alcoholic liquor, filtered after cooling, furnishes the resin by evaporation of the alcohol.

The wax is contained in the cells which compose the cupule of the lupuline grain; it exists also in the scales which constitute the cone of the hop, and by treating these scales with boiling alcohol, it is procured in sufficient quantity. It is dry and pulverulent, inodorous and tasteless; it begins to soften at +80 degrees (176° Fahr.), and is fully melted at +100 degrees (212° Fahr.) Strongly heated, it gives two volatile products, which diffuse an odour of wax; it burns without residue, producing a white shining flame; this matter resembles, as we see by its properties, the wax of the sugar cane.

Explanation of Plate II.

- Fig. 1. Cone of Hop.
- Fig. 2. Terminal bud enveloped by the stipules, *s s*, on which are marked the granulations, which represent the cupules and the discs indicated by the figures 14, 15, 16, &c.
- Fig. 3. Lupuline originating; *s s*, epidermis; *l*, primordial cellule of lupuline, by which it is attached to the epidermis; *a*, cellule produced by the preceding, and which gives rise to the following modifications:—
- Fig. 4. *a*, epidermis; *l*, primordial cellule; *a*, cellule divided transversely into two; the inferior division constitutes the pedicel of the lupuline, the superior forms the gland of the same.
- Fig. 5. *s s*, epidermis; *l*, primordial cellule; *p*, pedicel; *a*, cellule containing grey matter with granules.
- Fig. 6. *p*, pedicel; *a*, cellule divided into two longitudinally.
- Fig. 7. *p*, pedicel; *a*, represents one of the two cells of the preceding figure, subdivided longitudinally into two; *a'*, is another cellule, not so parted.
- Fig. 8. *s*, epidermis; *p*, pedicel; *a*, gland formed of four cellules.
- Fig. 9. Gland, represented in figure 7, front view; *a*, is the cell not divided; *a'*, the cell which is parted into two longitudinally.
- Fig. 10. Gland *a* of the figure 8, front view.
- Fig. 11. The same gland more advanced, in which are seen many cells originating by the intra-utricular mode of multiplication.



- Fig. 12. The same gland, seen on the face, and a little farther advanced.
 Fig. 13. Gland more advanced, in which the four cellules of figures 10, 11, and 12, are subdivided parallelly to the ray, and parallelly to the circumference; each of the cells is indicated by *a a a a*.
 Fig. 14. Gland in which the utricular multiplication is still more advanced. The four mother cells of fig. 10 are still visible, and indicated by *a a a a*.
 Fig. 15. Shows the aspect which the glands present when they have acquired a somewhat considerable size; *e*, epidermis; *l*, the gland.
 Fig. 16. Glands more advanced. The edges of the discoid glands, as seen in preceding figures, are here raised, forming cupules, *l, l*; *e*, epidermis.
 Fig. 17. Cupule from the internal (or upper) surface of which the cuticle *d* is detached, and elevated by the secretory products.
 Fig. 18. Lupuline, which has acquired its complete development; *e i*, secreting cupule or proper gland, surmounted by the cuticle *c s*, raised up by the products of secretion.
 Fig. 19. Grain of lupuline enlarged; *e i*, cupule or gland proper; *i*, point of attachment; *c s*, elevated cuticle. There is seen on this last the impression or trace of the cellules of the cupule, on the cavity of which this cuticle was applied.
 Fig. 20. Longitudinal section of a grain of lupuline; *e i*, cupule composed of a single layer, which secretes the contained liquid; *c s*, cuticle detached from the internal surface of the cupule by the secreted liquid.

The figures are from the pencil of M. Trecul.

Remarks on the Sexual Changes in the Inflorescence of
Zea Mays. By Mr JOHN SCOTT.*

The florets of the Indian corn, *Zea Mays*, as is well known, are unisexual, and so placed that the male florets form a terminal panicle, or raceme, and the females inferior lateral spikes. In the male panicle the spikelets are two-flowered; both florets perfect and characterised by two glumes, two squamulæ, and three stamens. In the female spike, the spikelets are also two-flowered, but in this case the inferior floret is neuter; two paleæ alone being developed, the superior fertile, and possessing two or three paleæ, an oblique, sessile ovary, and a long compressed style, bifid, and pubescent at the apex.

In the abnormal specimens which I now submit to the Society, the male and female florets, in place of being

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arranged as above, on distinct axes of the plant, occur more or less irregularly on a single axis. Thus, in specimen No. 1, we have a female monocious spike; *i. e.* a female spike, producing both male and female florets. In this case the basal portion of the spike is normal, presenting several circles of the perfect grain; the upper and major portion of the spike, on the other hand, has every floret converted into the male form; each spikelet, be it observed, producing *two perfect male florets*. In other cases, however, the upper portion of the spike retains its feminine character, while the basal portion assumes the male; or again, we may have an irregular intermixture of male and female florets over the whole spike.

The metamorphosis of the female into the male floret is not, however, always complete, and this is more especially so in such cases as the latter, where there is no definite arrangement of the male and female floret. From the special theoretical interest now attached to these imperfectly metamorphosed florets, in their association with others perfectly metamorphosed, I will here describe one or two of the most instructive which have come under my observation. *First*, In the *superior* floret of a female spikelet, the style was abortive, ovary rudimentary, squamulæ developed (though smaller than those of a normal *male* floret), glumes lanceolate-acuminate; in the *inferior* floret the stamens, squamulæ, and glumes were perfectly developed; so that the normally *neuter* floret of the female spikelet was in this case converted into a *perfect* male; whereas a very imperfect metamorphosis has been effected in the case of the fertile female floret. Again, *second*, in another spikelet, from a female spike, I found the *superior* floret presenting all the characteristics of the *normal male* floret; while the *inferior* (though still retaining its neutrality of function)—stamens and pistils being alike abortive—presented by a pair of lanceolate acuminate glumes, and two minute cuneate squamulæ, an evident tendency to assume the male form also.

If we now turn to an examination of the male panicles, we are at once struck with the rare occurrence of the monocious structure in them, as compared with the occurrence of such a structure in the female spikes. Somehow—and

it is difficult to understand why it should be so—the female organs in this instance, as indeed in most other unisexual plants, are much less prone to become developed in the male flowers than are the male organs in the female flowers.* On this account, then, I trust the Society will bear with me while I briefly attempt to describe the few male monocious panicles, which I have been fortunate enough to obtain; they are as follows:—

First, In specimen No. 2,—a terminal panicle,—the primary axis bears male and female florets; the florets of the former are perfect in the upper portion of the axis, which they exclusively cover, but in the lower portion, where they approach the female florets, the superior floret in the majority of the spikelets is alone perfect; while in the inferior floret the stamens occur in a more or less rudimentary condition. The female florets of the primary axis are *all imperfect*, the ovary existing in a rudimentary form, and the stamens utterly aborted in the *superior* florets; whereas, in the inferior florets of the spike-

* May we not regard this as probably indicative of those homological distinctions between the male and female organs of plants, insisted upon by Schleiden and Endlicher, at least as modified by Dr Dickson, in his interesting paper "On the Nature of the Cormophyte." (*Vide Society's Transactions*, vol. vi. p. 95.) Dr Dickson there states, that he is "inclined to believe that there exist in reality two modes of placentation, the one where the ovules are produced by a process of gemmation from the carpellary leaves (*parietal*); the other, where the ovules spring from the prolonged floral axis (*central*). In this modified sense, then, a strong argument against the Schleidonian theory of placentation is completely neutralised. I refer to the *inverse* convertibility of male and female organs in certain plants. For example, in the willows, we have some excellent illustrations in the Society's Transactions. Thus, in vol. i. page 118, the Rev. J. E. Leefe has illustrated the gradual modifications of the pistillary into staminal organs in the *Salix Caprea*; while Mr Lowe, vol. v. p. 118, has given us, *vice versa*, all the conceivable intermediate stages in the transformations of the staminal into the pistillary organs in *Salix Andersoniana*. Now, as these cases of the willows naturally come under the division assumed to possess a *parietal placentation*, their evidently disproving tendencies are utterly invalidated. And thus, even in view of such anomalous occurrences, we may justifiably reiterate the above suggestion, as to the difference in the *inverse* convertibility of the male and female organs, in at least the case of the maize, where the floral axis, as terminal shoot, undistinguishable in the cavity of the germen as a special organ, bears a single seed-bud (Schleiden's "Principles of Botany," p. 386), and is thus referrible to the division characterised by a central placentation.

lets, staminal and pistillary organs are similarly aborted. Again, in the arrangement of the male and female florets in the secondary axes, a similar plan is observed to that of the primary axis; the female florets occupying their basal portions, but in several instances *perfectly* developed, the upper portions being covered with *perfect male* spikelets.

Secondly, In specimen No. 3,—a terminal panicle,—we have a very irregular intermixture of perfect and imperfect male and female florets, along with several structurally *hermaphrodite florets*. Generally speaking, however, in this specimen, as in No. 2, the upper portions of both primary and secondary axes still retain their normal-male-sexual characteristics, and the basal portions assuming the female characters. By a careful examination, however, I have detected several structural peculiarities in certain florets of the latter part, to which I am inclined to attribute a highly important theoretical signification, as will be seen subsequently. The following are the most instructive:—*First*, In the *superior floret* of a spikelet, presenting the broad glumes and paleæ of the female florets, I found a rudimentary *hypogynous stamen*; while in the *inferior floret* the glumes were *ovately-lanceolate*, squamulæ as usual in normal male florets, stamens developed, but *destitute of pollen*. *Second* spikelet; glumes of *superior floret* *broadly-lanceolate*, squamulæ *cuneate*, obliquely truncate, larger than those characteristic of the male florets, ovary and style incipient, as in the above; but in this case I found *two rudimentary stamens*, one consisting of the filament alone, the other of the filament and a pellucid rudimentary anther, presenting the appearance of a glandular hair; the only modification the *inferior floret* of this spikelet had undergone from its normal male condition, was the non-development of the pollen, the anther-cases being quite empty. *Third* spikelet; glumes of *superior floret* *ovately-lanceolate*, squamulæ *cuneate*, minute, anthers destitute of pollen; *inferior floret* functionally a perfect male, glumes *broadly-lanceolate*, squamulæ as usual in male florets, anthers containing pollen.

These, then, are the more interesting peculiarities which I have observed in the structure of the florets in the above panicle; and there is just one other point in connection

with it to which I will here specially direct attention,—namely, the remarkable irregularity observed in the relative arrangement of the male and female florets. The most striking case is presented by one of the secondary axes; the florets in its basal portion are nearly all converted into more or less perfect *females*, whilst those above retain in like manner the male characteristics. Associated with the latter, however, and near the upper extremity of the axis, *two solitary female* florets are at once observable by their prominently developed grains. On examination of the spikelets bearing these, I find that the female morphogenesis is complete, the *superior* floret alone fertile, the *inferior* neuter. This individual isolation of the florets, occurring as they do in distinct parts of the axis, and surrounded by normal male florets, and perfect metamorphosis, excellently illustrates the occasional independence of such phenomena on mere physical conditions.*

Thirdly, In specimen No. 4—a terminal panicle—a somewhat different arrangement is observed to that which we have seen followed in specimens Nos. 2 and 3. In the latter two, the basal portions of the axes produce female florets, and the upper male; whereas in the former, No. 4 specimen, the opposite of this occurs, namely, the upper portions of primary and secondary axes converted into *compact spikes* of *female* florets, while the lower portions,

* Dr Lindley in treating on the changes of sex under the influence of external causes (*Introduction to Botany*, vol. ii. 4th ed. p. 80), states "that Mr Knight long ago showed that a high temperature favoured the development of male flowers, and a low one that of female;" furthermore, that this eminent horticulturist "entertained little doubt that the same fruit-stalks might be made to support either male or female flowers in obedience to external causes." Dr Lindley illustrates these conclusions by experiments on water-melons and cucumbers. From personal observations, however, on several monoicous plants, I cannot think that these influences are at all definite as to their influence on sex produced. The above laws are still less applicable to dioicous plants; and certainly, upon any theory of special creation; on a subjective consideration of the vegetable individual, I fail to see why they should not be equally potent in the one case as in the other. Such cases as those above noticed in the maize, in which collateral florets assume distinct sexual characteristics, induce me to believe that in general the influence of physical condition on the change of sex is subordinate to certain innate, specific, formative qualities; in short, an inherited tendency to produce the characters in question.

retaining their normal characters, continue to produce the racemose male spikelets. In the majority of the secondary axes, however, the basal spikelets are nearly all aborted; whereas this portion of the primary axis is covered with perfect male spikelets, which, as they extend upwards, are *abruptly* metamorphosed into a short terminal spike of female florets. These, like those on the secondary axes, are all very imperfect, the ovary and style existing in a more or less rudimentary condition, and occasionally presenting the rudiments of one or two hypogynous stamens.

Hitherto our remarks have been chiefly confined to a mere description of the sexual metamorphoses in the florets of maize, though I have more than once alluded to their possible connection with, and elucidation of, certain highly important points in theoretical natural science. For the sake of clearness in the exposition of the theoretical bearings of these metamorphoses, I will now give a brief *résumé* of the foregoing illustrations. First, then, we have stated that the inflorescence is normally unisexual—the female florets borne on inferior lateral spikes, the male on terminal racemes or panicles. Our illustrations, however, show that these structural arrangements undergo important modifications. Thus, we have first the female spikes assuming a monoicous structure, and this without any regard whatever to the relative axial arrangement of the male and female florets; the same part on distinct axes indifferently producing perfect and imperfect male or female florets, as well as collateral mixtures of both; showing us most conclusively, their morphogenetic independence of the mere external conditions of life. Again, individual florets of these female spikes present themselves with a structure intermediate between that of the perfect male and female; and then manifest a most interesting and instructive correlated order in the development of their organs. Thus the *superior* and normally *fertile* floret of a spikelet with abortive style and rudimentary ovary, had assumed the characteristic squamulæ and glumes of the *male* floret; while the normally *neuter* floret had assumed *in toto* the male characteristic.

In the terminal male panicles, with a similar series of

changes to those which we have noticed in the female spikes, there are also the important additional illustrations of *structurally hermaphrodite* florets.* Thus, in one of the spikelets we had a *superior* floret with incipient ovary and style, a *rudimentary stamen*, and the characteristic paleæ and glumes of a *normal* female floret; while the *inferior* floret differed from a normal male only in its *broad* and *shorter* glumes, and *barren* stamens. Again, in the *superior* floret of another male spikelet, with glumes somewhat intermediate between those of the normal male and female florets, we noticed an incipient ovary and style, and *two* rudimentary stamens; while the inferior floret of the same spikelet retained its *male* characteristics.

What now are we to say as to the cause of these changes? We see the unisexual florets of the maize not only undergoing inverse metamorphoses, *i. e.*, the male converted into female florets, and the female into male florets, but also assuming every conceivable intermediate stage between these and a structural hermaphroditism. Now, it is well known that similar sexual changes occur in—at least the female florets—many other monoicous and dioicous plants; *e. g.*, in the *Melandryum prætense*, and the *Lychnis dioica*, the female flowers occasionally become bisexual by the development of the stamens. I may also state that I have observed bisexual (female) flowers on the *Littorella lacustris*, *Bryonia dioica*, and *Ricinus communis*.†

Seeing, then, that unisexual flowers undergo such serial transformations in their sexual characteristics, we, on the ordinary theory of creation—*i. e.*, assuming species as the original units—might justly expect a similar series of changes in the characteristics of bisexual flowers. This, however, as is well known, is not the case; no instance can be

* I may state, that although I have failed in illustrating structural hermaphroditism in the female spikes, cases are already recorded. C. F. Gärtner, in his "Beiträge zur Kenntniss der Befruchtung," notices the occurrence of solitary stamens in the female florets of *Zea Mays*; he also states that he has observed in the conversions of male into female florets, a solitary stamen associated with the pistillary organ of the latter.

† I will not here enter on details as to the occurrence of the above, as I hope at some future time to lay them before the Society in a notice of my observations and experiments on the subject of *Vegetable Parthenogenesis*.

adduced of a bisexual species undergoing sexual metamorphoses similar to those above described in the unisexual maize. Moreover, supposing that the sexual characters of hermaphrodite plants had exhibited masquerading tendencies similar to those of unisexual plants, it is at once evident, that, upon any theory of special creation, the *cause* of such changes in either case is equally unintelligible. On the other hand, if, with Mr Darwin, we believe that species are the modified descendants of previously existing species, these phenomena are no longer enigmatical, but clearly the results of definite and well-known laws. I need only refer to Mr Darwin's interesting papers on the distinct sexual forms of the dimorphic species of *Primulas* and *Linums*, "*Jour. Linn. Soc.*," vol. vi. p. 77, and vol. vii. p. 69, by way of illustrating, as has been remarked, "the possibility of a plant becoming dioicous by slow degrees." Now, if we reflect on this dimorphism of the *Primulas* and *Linums*, those differences in the variability of the unisexual, relatively to the bisexual flowers, are, I believe, readily explicable on the supposition that the latter—i.e., the hermaphrodite structure, as Professor A. Gray has maintained—*vide* "*Sill. Amer. Jour.*," vol. xxxiv.—"is the *normal* or *primary* condition of flowers." In fine, then, in accordance with the theory of modification with descent, I, inferentially guided by that principle of reversion to type so much insisted upon by those opposed to derivative hypotheses, look confidently at such sexual changes as those above described, as retrogressive tracings of the graduated modifications by which an original hermaphrodite progenitor gave rise to a monoicous offspring.

New Researches on Hybridity in Plants. By M. CH. NAUDIN. Translated from the *Annales des Sciences Naturelles*, by GEORGE MAY LOWE, Esq.*

(1.) *On the Sterility and Fecundity of Hybrids.*

A century ago, Koelreuter demonstrated by proofs which no other observer has ever surpassed in exactitude, and which

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still retain all their value, the fact of the sterility of hybrids being absolute in some cases, but only partial in others. These two facts, since so frequently confirmed, cannot now be disputed. In a former paper I gave some examples which serve to illustrate them.

We have seen *Nicotiana-californico-rustica*, *N. glutinoso-macrophylla*, *N. glutinoso-angustifolio-macrophylla*, *Digitalis luteo-purpurea* and *Ribes Gordonianum*, sterile both by the stamens and ovary—the former being totally destitute of pollen well formed, and the latter incapable of impregnation by the pollen of the parent plants. But as the pistil does not in every case present any appreciable deformity, it is natural to seek in the ovule itself the true cause of this inaptitude to receive impregnation.

It has been fully proved by many cases of hybridity, in which, in the same ovary one portion of the ovules resists impregnation, whilst the other becomes converted into embryonic seeds capable of germinating—that this defectiveness exists in the ovule, and not in the more exterior parts of the pistil.

We have seen this in the three hybrid generations of *Luffa acutangulo-cylindrica*, also in *Luffa amaro-cylindrica*, *Cucumis Meloni-trigonus*, *Nicotiana rustico-paniculata*, and *paniculato-rustica*, &c. *Cucumis myriocarpus-Figarei* is a not less convincing proof, since among 100 fruits which were developed and ripened under the influence of pollen derived from the maternal species, 19 at least were destitute of seeds, and each fruit, among the small number which contained any, only yielded one seed. I might mention, in support of this fact, the example of *Mirabilis longiflora-Jalapa*, though in this case the ovary is uniovular. The stigmas of this hybrid were all equally developed, and in this respect not inferior to those of the parent species; yet eleven attempts to impregnate it with the pollen of *Mirabilis longiflora* were made without effect, and even ten were necessary with that of *M. Jalapa* to determine the increase of a single ovule. In the *Luffa* hybrids just mentioned, and also in the case of *Cucumis Meloni-trigonus*, however poor the pollen might have been which was employed to fertilise their ovaries, it is beyond doubt that the number of

good grains deposited on their stigmas far exceeded that of the ovules which were developed into seeds.

This, it is true, is only hypothetical, but it is extremely probable. It remains to be confirmed by the anatomical examination of the ovule, and it would be very interesting to discover in what part the defectiveness exists; but this is a peculiar kind of research, very difficult, very minute, often uncertain in its results, and which one cannot enter upon without being well accustomed to it, and provided with excellent instruments, two things in which I am deficient.

I therefore contented myself with verifying experimentally the fecundity or the sterility of the ovaries, which was more expeditious, and probably more conclusive; but it is not less a subject to be recommended to professed micrographers.

That the sterilising action of hybridisation exerts much more force on the pollen than on the ovules is a most indubitable fact, and one well known to all hybridologists. This need not surprise us, since the pollen is, of all parts of the plant, the most elaborated, the most animalised, if such an expression can be used. Frequent chemical analyses prove that it is in these granules that the phosphorised and azotised materials are more accumulated than elsewhere, and thus it may be conjectured that it is this high organisation which is injured in hybrids, where the whole vegetation suffers from the disturbance which results from the intermixture of two specific essences created to live separately. The hybrids of which I have given an account present several examples. We have seen that *Mirabilis longiflora-Jalapa* yields pollen unfit for fertilisation, whether it be applied upon the stigmas of the hybrid, or upon those of its two parents, whilst in twenty-one attempts to impregnate it with the pollen of these last (*M. longiflora* and *M. Jalapa*), there was only one which took effect, and enlarged the ovary. This result is quite in accordance with those which M. Lecoq ("Revue Horticole," 1853, pp. 185 et 207) announced that he obtained from the same hybrid, the pollen of which he always found useless, but he was able to fertilise it by that of *M. Jalapa*. The difference in the strength of the pollen and the ovules becomes still more

manifest in *Nicotiana glauco-angustifolia* (and it would undoubtedly have been the case with *N. glauco-macrophylla* if the experiment had been made on it), where the whole pollen mass is defective and inert, whilst the ovary becomes filled with seeds, when it is fertilised with the pollen of *N. Tabacum* and *N. macrophylla*.

All the hybrids I have observed, containing well-developed grains of pollen in their anthers, have been fertile, often to a high degree, by their ovaries. I have never seen, and I do not believe it possible to mention a single instance in which, the ovary being sterile, the stamens have been fertile, even in the least degree.

The deleterious influence which hybridisation exercises upon the fertilising apparatus shows itself in different forms.

The most common, or at least the most remarkable case, is the direct atrophy of the pollen in the anthers, more rarely the atrophy of the anthers themselves; but we have also seen it act on the entire flowers. It is so among all the hybrids produced by the agency of *Datura Stramonium*, the flowers in the lowest branches invariably fall without opening; also among all the individuals of *Luffa acutangulo-cylindrica* of the first generation,—all the primary male flowers perish entirely, and also some flowers which begin to open when the plants are more than full grown, and have lost part of their vigour. The same phenomenon is observed in *Mirabilis longiflora-Jalapa*, which loses three-fourths of its buds, in *Nicotiana rustico-paniculata* and *paniculato-rustica* of three consecutive generations, &c. In fine, another mode of sterilisation is that effected by the changing of monœcious male flowers into female, as we have seen in *Luffa* hybrids of the third generation.

I have every reason to believe, although I cannot positively affirm so, that the specimen of *Cucumis Figarei*, so remarkably large, and peculiar by the nearly total absence of male flowers, which I experimented on in 1856, and which yielded the results I have mentioned, owed both its great size and almost female unisexuality to hybridisation.

(2.) *On the Difference of the Fertility of Hybrids.*

Hybrids are self-fertile in all cases in which their anthers contain well-organised pollen ; but if the quantity is very small, it is well not to leave the impregnation to chance, but to aid artificially in fertilising the hybrid with its own pollen. I have done this in *Luffa acutangulo-cylindrica* of the first generation, which has but few male flowers and a small quantity of good pollen.

In the majority of cases microscopic inspection sufficiently shows the character of the pollen ; the difference in form, size, and transparency distinguishes the good and bad ; and it is easy to judge, at least approximatively, of the relative quantity. Yet there are some cases, though not very common, where this examination of the pollen is not sufficient to determine whether it is active or inert ; for it may happen that it has all the appearance of good pollen without having its qualities. Such was that of *Mirabilis longiflora-Jalapa*, whose grains, although unequal, were not deformed, and appeared full of fovilla, notwithstanding their inefficacy upon the stigmas of the two parent plants, as well as upon those of the hybrid. Perhaps the employment of chemical reagents would better determine their impotency.

There are various degrees of fertility in hybrids by means of pollen. We have seen *Luffa acutangulo-cylindrica* of the first generation extremely low in this respect, but in the third remarkably productive. It is the same, and nearly to the same degree, in *Luffa amaro-cylindrica*, *Nicotiana rustico-paniculata*, and *paniculato-rustica*, and in a great number of the toad-flax hybrids (*Linaria purpureo-violacea*) of the second, third, fourth, and fifth generations.

A greater richness of pollen is seen in *Primula officinalis-grandiflora* of the first, and especially second, generation, and in *Cucumis Meloni-trigonus*, &c. In fine, there are some hybrids where the pollen is little inferior, if at all, in perfection to the most legitimate species. This is the case in *Coccinia Schimpero-indica*, *Datura meteloido-Metel*, *D. Stramonio-Metel*, *D. Stramonio-lævis*, *Nicotiana angustifolia-macrophylla*, *N. texano-rustica*, *N. persico-Langsдорffii*, *Pe-*

tunia violaceo-nyctaginiflora, &c. ; and the same in many of the toad-flax hybrids of the third and fourth generations, already very close to *Linaria vulgaris*.

In a word, as I said at the commencement of this article, hybrids are found of all degrees of fertility, from the extreme case where the ovary only is fertile to that where all the pollen is as perfect as that of the best-established species.

(3.) *Is the Aptitude of Species to cross each other, and the Fertility of the Hybrids which result, proportional to the apparent Affinity of the Species ?*

In general this is the case ; but there are exceptions, and we have stated some. There are, indeed, some species, closely allied in exterior organisation and physiognomy, which are less disposed to mutual crossing than other species which are far distant in their outward appearance. Thus we have seen three species of eatable gourds, so closely resembling each other that most botanists fail to distinguish them, resist all attempts to cross them ; whilst the melon and *Cucumis trigonus*, so very different from one another, easily give origin to very fertile hybrids, though the pollen is a little defective. Such is the case with *Nicotiana glauca*, which, although very distant from *N. angustifolia* and *macrophylla*, yet gives hybrids with them, having very fertile ovaries ; whilst *N. glutinosa*, more difficult to cross with them, although belonging to the same section of the genus, only gives one sterile hybrid both by the pollen and ovary. I might also mention the crossing of *D. Stramonium* and *D. ceratocaula*, two species strangers to each other, from which there results a fertile hybrid, although attended by that peculiar kind of partial sterility which consists in the loss of the first flowers.

These exceptions, for which it is probably impossible to assign a cause, do not prevent the affinity of species, as revealed by the exterior organisation, from indicating generally the degree of aptitude to cross, and do not prevent us from forming a conjecture to a certain extent as to the fertility of the hybrids. We have seen the proof in *Datura Meteloido-Metel*, *D. Stramonio-Tatula* and *Tatulo-Stramonium*, *D. Stra-*

monio-lævis, *Nicotiana texano-rustica* and *rustico-texana*, *N. angustifolio-macrophylla*, &c., which hybrids, with the marked exception of those of *D. Stramonium*, have perfect fertility. The aptitude of species for mutual impregnation, and the degree of fertility of the hybrids which result, are therefore the true signs of their special affinity as regards generation; and in the great majority of cases this affinity is indicated by the exterior organisation—in other words, by the physiognomy of the species.

(4.) On the Physiognomy of Hybrids.

To give a just idea of the aspect which hybrids present, it is essential to distinguish between the first generation and those which follow.

I have always found in those hybrids which I have obtained myself, and whose origin has been well known to me, a great uniformity of aspect between individuals of the first generation, no matter how numerous, provided they proceed from the same crossing. This we have seen in *Petunia violaceo-nyctaginiiflora*, *Datura Tatulo-Stramonium*, and *D. Stramonio-Tatula*, *D. Meteloido-Metel*, *D. Stramonio-lævis*, *Nicotiana texano-rustica*, and *N. rustico-texana*, *N. persico-Langsдорffii*, &c.

I do not mean to say that all the individuals of the same crossing are absolutely counterparts of one another; there are sometimes slight variations between them, but not sufficient to alter the general uniformity in a sensible degree, and it does not appear to me that these differences are any greater than those which are frequently seen between the seeds of legitimate species of the same production. In short, it may be said that hybrids which proceed from the same crossing, resemble each other, in the first generation, as much as, or nearly as much as those which proceed from the same legitimate species.

Must it be admitted, as M. Klotzsch maintains, that mutual hybrids (those which proceed from the two possible crossings between the two species) are markedly different from each other; for example, the hybrid obtained from the species A fertilised by the species B, differs sensi-

bly from that which is obtained from the species B fertilised by the species A? I cannot deny this in an absolute manner; it would be necessary to see the hybrid which induced M. Klotzsch to make this statement; but I can assert, that all the mutual hybrids which I have obtained, as well between allied species as between distant ones, resembled one another as much as if they proceeded from the same crossing. I have already pointed this out when speaking of *Datura Stramonio-Tatula* and *Tatulo-Stramonium*, *Nicotiana paniculato-rustica* and *rustico-paniculata*, *N. angustifolio-macrophylla* and *macrophylo-angustifolia*, *N. texanorustica*, and *rustico-texana*, *N. persico-Langsдорffii*, &c.; without doubt it may not be always so, but if the fact is true, it must be rare, and considered more as the exception than the rule.

All hybridologists are agreed that hybrids (and it always applies to hybrids of the first generation), are mixed forms, intermediate between two parent species. And this is really what does take place in the great majority of cases; but it by no means follows that these intermediate forms are always at an equal distance between the two species. On the contrary, it is often observed that they are frequently much nearer one than the other. Besides, we may conceive, that the appreciation of these relations is always a little vague, and that it is the idea which determines it. We may also remark that hybrids resemble sometimes one of the two species in one character, whilst they resemble the other in another character. This is very true, and we have seen an example in *Mirabilis longiflora-Jalapa*, which is distinctly more like *M. longiflora* in the organs of vegetation, and *M. Jalapa* in the flowers. But I think it is wrong to refer this distribution to the part which the species have played as father or mother in the crossing whence the hybrid has arisen. At least I have not seen anything which confirms this opinion.

M. Regel asserts (*Die Pflanze und ihr Leben*, &c., p. 404, *et suiv.*), that when the hybrid proceeds from species of different genera, their flowers bear the essential characters of those of the father; but we have seen in the *Datura ceratocaulo-Stramonium*, proceeding from two nearly generically

different species, the flowers were absolutely like those of the mother (*D. Stramonium*) ; in *Nicotiana glauco-angustifolia*, and *glauco-macrophylla* obtained from very different species, they were remarkably more like those of the mother than those of the father ; whilst in *N. californico-rustica* and *glutinoso-macrophylla* they were very distinctly intermediate between the parent species.

The rule laid down by M. Regel seems to me therefore very hazardous, or at least founded upon insufficient data. For my own part, I believe that these inequalities in resemblance, sometimes very great between the hybrid and its parents, are maintained chiefly by the marked preponderance which many species exercise in their crossings, whatever may be the part which they act (whether as male or female).

This we have seen in the hybrids of *Petunia violacea* and *P. nyctaginiiflora* which have a greater resemblance to the first than the second ; in *Luffa acutangulo-cylindrica* of which the forms are far more like *Luffa cylindrica* than the conjoined species ; and especially in *Datura ceratocaulo-Stramonium* and *D. Stramonio-levis*, of which all the individuals are incomparably nearer *D. Stramonium* than the other species, although in one case *D. Stramonium* fulfils the function of the male, and in the other that of the female.

Commencing with the second generation, the physiognomy of hybrids is modified in a most remarkable manner. Very often the perfect uniformity of the first generation is succeeded by an extreme medley of forms, the one approaching the specific type of the father, the other that of the mother—sometimes returning suddenly and entirely into the one or the other. At other times this recurrence towards the generating types is performed by degrees and slowly, and sometimes the whole collection of hybrids is seen to incline to the same side.

I think it is now placed beyond dispute that this dissolution of hybrid forms commences, in the great majority of cases (it may be in all) in the second generation.

- (5.) *On the return of Hybrids to the specific forms of the producing species. What is the cause which determines this return?*

In every hybrid which I have examined, the second generation presented changes of aspect, and a manifest tendency to return to the forms of the producing species, and that under such conditions that it was impossible for the pollen of those species to have concurred in bringing them back. We have seen striking examples in *Primula officinali-grandiflora*, in all the hybrids of *Datura Stramonium*, *D. Meteloido-Metel*, the mutual hybrids of *Nicotiana angustifolia* and *macrophylla*, *N. persica*, and *Langsdorffii*, *Petunia violacea* and *nyctaginiflora*, in *Luffa acutangulocylindrica*, and still more in *Linaria purpureo-vulgaris*. Among many of these hybrids, from the second generation, a complete return to one or other, or even both, of the two parent species has been seen, and approaching them in different degrees; among many also we have observed forms continuing intermediate, whilst simultaneously other specimens of the very same production have effected the return of which I am about to speak. Further, we have stated in some cases (*Linaria purpureo-vulgaris*) that, in the third and fourth generation, true retrogression towards the hybrid form takes place; and sometimes even we have seen individuals of a plant to all appearance wholly returned to one of the two species, which seemed to revert almost entirely into the opposite species.

All these facts are naturally explained by *the disjunction of the two specific essences in the pollen and ovules* of the hybrid. A hybrid is an individual in which two different essences are found united, each having its particular mode of vegetation and finality, which are mutually opposed, and are constantly striving to disengage themselves from one another. Are these two essences intimately blended? Do they reciprocally penetrate every part, so that each particle of the hybrid plant, however minute or divided, contains equal portions of both?

It may be so in the embryo and first stages of the development of the hybrid; but it seems to me more probable that

this last, at least in the adult state, is an aggregation of particles, both homogeneous and unspecific when taken separately, but distributed more or less equally between the two species, and mixed in different proportions in the organs of the plant. The hybrid, according to this hypothesis, would be a living mosaic, the discordant elements of which, so long as they remained mixed, would be undistinguishable to the eye ; but if, in consequence of their affinities, the elements of the same species approached each other and agglomerated themselves in small masses, parts and sometimes entire organs, would then be visible, as we have seen in *Cytisus Adami*, and the bizarre group of the orange and citron hybrids, &c. It is this tendency of two specific essences to disengage themselves from their combination, which has induced some hybridologists to say, that hybrids resemble the mother by their leaves and the father by their flowers.

Although the facts may not be sufficiently numerous to conclude with certainty, it seems that the tendency of species to separate, or, so to speak, to localise themselves in various parts of the hybrid, increases with the age of the plant, and is more and more pronounced as the vegetation approaches its term. These disjunctions become more manifest in the highest organisms of hybrids, about the reproductive organs ; in *Cytisus Adami* disjunction shows itself in the flowering branches ; in the orange anomalies and *Datura Stramonio-lævis* in the fruit itself. In *Mirabilis longiflora-Jalapa* and *Linaria purpurea* the corolla manifests the phenomenon of disjunction, by the separation of the colour peculiar to the producing species. These facts authorise the idea that the pollen and ovules, but especially the former, are precisely the parts of the plant where disjunction goes on with most energy ; and what adds a greater degree of probability to this hypothesis is, that they are at the same time very elaborate and minute organs—a double reason for rendering the localisation of the two essences more perfect. This hypothesis being admitted, and I confess it seems to me extremely probable, all the changes which supervene in hybrids of the second and more advanced generations would explain themselves, as it were ;

but if, on the contrary, it be not admitted, they would be perfectly inexplicable.

Let us suppose in the Toadflax hybrid of the first generation, that disjunction takes place both in the anther and contents of the ovary; that some grains of pollen entirely belong to the paternal species, others to that of the mother; that in others disjunction has not, or, at least, only just commenced. Again, let us suppose that the ovules are, to the same degree, separated both in the direction of the male and female parent; what will result when the pollen tubes descend into the ovary to fertilise the ovules? If the tube of a pollen grain, which has returned to the male parent, meet an ovule separated in the same direction, a *perfectly legitimate* fecundation will be produced, from which will result a plant entirely returned to the paternal species. A similar combination effected between a pollen grain and ovule, both returned to the female parent, the product will return in the same manner to the species of this last; if, on the contrary, combination is effected between an ovule and pollen grain, separated in opposite directions, they will perform a true crossed fecundation, like that which gave origin to the hybrid itself; and there will again result a form intermediate between two specific types. The fertilisation of an ovule non-separated, by a pollen grain separated in either direction, would give a quadroon hybrid; and since disjunctions, as much in the pollen as in the ovules, can take place in all degrees, every sort of possible combination will result as chance may direct. We have seen these multitudinous forms produced in the Toadflax hybrids, and *Petunia* from the second generation.

The retrogression of a hybrid in its course of return to one of the parent species is also easily explained by this hypothesis. I mentioned several examples when speaking of the third generation of *Linaria purpureo-vulgaris*. Thus, for example, among eighty plants sprung from an individual of the second generation, which seemed to be entirely returned to *L. purpurea*, fresh hybrids appeared, which came back to the intermediate form of the first hybrid, and other individuals still more sensibly approached to *L. vulgaris*. The reason is, that the purple-flowered hybrid of

the second generation, notwithstanding appearances, still retained some essence of the yellow-flowered *Linaria*, and this strange particle was sufficient to bring some pollen grains and ovules back either to a mixed state, or altogether to *Linaria vulgaris*.

Similar actions are produced, though less marked, in the descent of hybrids of the second generation, which seem entirely returned to the type of *L. vulgaris*, and even to a certain extent in that of *Datura Stramonio-lævis*, where some individuals return to *lævis*, preserving up to the third generation the accessory characters which belong to that form of hybrids. All these facts show us that the separation of specific forms allied in hybrids, is not always completed so rapidly as one might be led to suppose, judging from physiognomy and external appearance.

The return of hybrids to the forms of the parent species is not always so sudden as that which we have observed in the Primroses, Petunias, *Linaria purpureo-vulgaris*, *D. Meteloido-Metel*, &c.; it is frequently completed by insensibly minute gradations continued through long series of generations. We have seen, for example, in *Luffa acutangulo-cylindrica*, even in the third generation, that among forty individuals only one was found which had wholly reassumed the external appearance of *L. cylindrica*.

Hybrids of *Nicotiana persica* and *Langsdorffii*, modify themselves slowly, and ten or even more generations may be insufficient to bring them back entirely to the specific forms.

It is remarkable in the latter case, that the hybrids do not present any appreciable mark of disjunction of the two specific essences, which appear intimately blended together in every part of the plant. Nevertheless the traits of one of the two species sensibly disappear from generation to generation, as if extinguished by degrees; but it not unfrequently happens that this extinction takes place with such rapidity as to be completed in the second generation.

En résumé, hybrids fertile and self-fertile return sooner or later to the specific types from which they were derived, and this return is effected either by the separation of the two mixed essences, or by the gradual extinction of one of

the two. In the latter case the hybrid posterity returns entirely and exclusively to one only of the two producing species.

(6.) *Are there any exceptions to the law of return of hybrids to the parent forms? Do certain hybrids become fixed and give rise to new species?*

I have not been long enough engaged in the study of hybrids to have formed any settled opinion on this question. Many botanists of good authority believe that some hybrids, if not all, *can* become fixed and pass to the state of constant varieties, that is to say, true species, intermediate between those of their parents; this is in particular the opinion of M. Regel, who regards it as probable that in the group of willows, roses, and many other genera rich in nearly allied forms, the nomenclature of which is very embarrassing to the botanist, there originally existed but a small number of species (two or three), the fertile crossings of which have given rise to equally fertile hybrids, which, in their turn, crossing between themselves and their parents, have produced, age after age, those multitudes of forms which exist at the present day.

Such may be the case, but it is without proof, and the hypothesis is entirely gratuitous. In my opinion the fact may be explained otherwise in a much more natural and probable manner, viz., by the inherent property of all organisms (at least vegetable) to modify themselves to a certain extent according to the influence of the surrounding medium, in other words, by the innate tendency of what we call *species* to subdivide into secondary species. How can it be admitted, for example, that roses, which are disseminated over the whole extent of the Old World, from Ireland to Kamschatka, from the Atlas and Himalayas to the glacial ocean, which cover all North America, which are often isolated in narrow spaces and different localities, can have met each other to give rise to hybrid forms?

It would be hardly possible to conceive such a fact. Have roses never been subjected to experiment to ascertain how far they can mutually hybridise, and if their hybrids

would be fertile or not? I can affirm this, that I have never obtained a hybrid which manifested the least tendency to form a specific stock.

At present I only know a single instance which might serve as a basis for the hypothesis of fixation of hybrids. Still this fact is doubtful; it is that of *Ægilops*, closely allied to wheat, which was cultivated at the Museum about ten years, during which the successive generations did not produce any appreciable modification.

It remains to be proved whether the *Ægilops* cultivated at the Museum (*Æ. speltæformis*, Jord.) is really a hybrid, and that it does not modify itself during a long series of generations: it would be an exception; but this very general rule would not be weakened, at least so long as the fact remained isolated.

(7.) *Is there any precise limit between Hybrids and Crosses?*

Most hybridologists insist on making a distinction between hybrids and crosses, and nothing could be more easy to understand; the hybrid results from the crossing of two distinct species, two true species, as M. Regel says—the crosses from that of two races or varieties.

Theoretically, nothing is more clear; in practice, nothing is more difficult than the application of these two words.

For example, ought the produce obtained by the crossing of the Cantaloup Melon and Netted Melon, that of the Netted Melon and Dudaim, that of Dudaim and *Cucumis Pancherianus*, or even that of *Datura Stramonium* and of *Datura Tatula*, &c., to be called hybrids or crosses? This question gives rise to another, that of the distinction of species, races, and varieties, an everlasting subject of dispute among naturalists, which too often ends in a war of words unworthy of the science; to settle which, it is necessary to turn to the examination of what is understood by the term *Species*, *Race*, and *Variety*.

(8.) *What, therefore, is a Species, Race, and Variety?*

Let us start at the very origin of the notion of species,

and not lose sight of the fact that all our ideas arise from the *contrast of things*.

The man blind from birth has no idea of darkness, because being deprived of the sensation of light he does not perceive the difference between the two ; even one possessed of sight would have no idea of the light which surrounds him if the whole world was luminous and that to the same degree. The notion of species does not escape the common law ; it is more complex, and is formed from more elements, as we shall attempt to elucidate.

If there existed in nature but *one vegetable form*, wheat for instance, always and everywhere alike, without any variation in the innumerable individuals which represent it, we might arrive at the idea of an *individual* and *vegetable*, but not *species* ; *wheat* and *vegetable* would be confounded in one's mind as one and the same thing.

Let us suppose also that nature had created an indeterminate number of different organisms, and each of them represented on the earth by only a single individual, incapable of multiplying itself, but indestructible and imperishable ; even here we could not arrive at the conception of a species, for each type of organisation would be isolated, and have no resembling individual.

To have a species it is necessary, therefore, *1st*, To have a *plurality* of similar individuals, that is to say, a group, a collection ; *2d*, That this group or collection of individuals *contrast in some degree* with other groups of individuals likewise resembling each other, and yet able to approach one another in some common points which render them comparable.

It follows that the idea of species is connected with that of kind or genus (I mean genus taken in a philosophical sense) ; that the one fact always supposes the other ; that, in a word, they are inseparable and unable to exist apart. And as, in the organic world, individuals have a transitory existence, reproducing themselves by generation, it is necessary, *3dly*, In order that species may have consistence and duration—that the *resemblance of individuals forming a specific collection shall continue in successive series of generations*.

Thus a plurality of similar individuals forming a group, and the contrast of groups among themselves by certain

characters common to different groups; and, *lastly*, The perpetuation of resemblances between the individuals of the same group constitute the elements of species. Species contain nothing more or less.

It is not, therefore, an ideal type, as certain abstract-loving naturalists have suggested; it is essentially a collection of similar individuals. The abstract ideal type of a common organisation is only, as it were, a tie, which in our mind collects similar individuals in the same bundle, and sums up the contrasts (or differences) which separate their group from every other.

It is necessary, then, to return to the pure and simple definition of Cuvier,—viz., *A species is a collection of individuals descended from one another, or from common parents, and from those which resemble them as much as they resemble themselves.*

Let us remark, in passing, that in thus defining species, Cuvier did not take *races* and *varieties* into consideration.

Everywhere where there is a group of similar individuals, contrasting in some measure with other groups, and preserving through a series of generations the physiognomy and organisation common to all the individuals,—*there is a species.*

It is by their contrast that species are distinguished from one another, and it is by comparison that their contrasts appear. Contrasts may be more or less great according to the objects compared. If they are very great and well marked, all the world acknowledge the specific distinction of the compared forms; if they are very weak, almost inappreciable, opinions are divided; one party separating the feebly contrasting forms into distinct specific groups, the other collecting them into one, and applying to them in the mean time the qualifications of *races* or *varieties*.

These collections and separations are purely optional, and they can have no other rule than scientific or economic advantage; in order to determine them it is necessary to be endowed with a certain tact which is ordinarily acquired by experience.

In short, there is no qualitative difference between species, races, and varieties; it is idle to seek one. These

three things are formed from one, and the terms which pretend to distinguish them only indicate degrees of contrast between compared forms.

It must be understood that here the question is not concerning simple individual variations, non-transmissible by way of generation, but only forms common to an indefinite number of individuals, and transmitting themselves faithfully and indefinitely by generation.

Contrasts between compared forms *are of all degrees*, from the strongest to the weakest, which simply means that following the comparisons which are established between groups of similar individuals, species are found of all degrees of strength and weakness; and if it was attempted to express these degrees in so many words, the whole vocabulary would be insufficient.

The delineation of species is therefore as I said before entirely optional; it makes them larger or smaller, according to the importance which is given to the resemblances and difference of various groups of individuals taken with respect to each other, and these appreciations vary according to men, times, and phases of science. How many modifications have certain great species of Linnæus and Jussieu undergone during fifty years!

The division of old species, their *pulverisation*, if I may use such a term, seems to have now reached its extreme limits, and many botanists are led by this tendency to complicate the descriptive part of the science in such a way as to threaten to involve the whole life of a man in its minutiae. Notwithstanding this, if those who have inaugurated these scientific refinements have not committed error by taking individual alterations, non-transmissible and not forming a group, that is to say simple variations, for forms common to an indefinite number of individuals, very constant and very faithfully transmissible in every consecutive generation, there is reason to believe that they have proceeded logically. The whole question is to know if it be advantageous to science to distinguish and enrol in its catalogues, these feebly contrasting species; but it is essentially necessary to be assured that the characters which are assigned them are really specific—that is to say, common to an unlimited num-



ber of individuals, and always faithfully reproduced in every generation.

But it is more than probable that in a multitude of cases (in the genus *Rubus*, for example) purely individual variations without persistence, have been taken for common characters, constant and transmissible.

Does it then follow that the terms *race* and *variety* ought to be banished from the science? Certainly not, for they are convenient to designate weak species that ought not to be enrolled among the official species, but it is proper to give them their true signification, which is absolutely the same as that of *species* properly so-called, and to see in forms designated by these terms some unity of a weak kind, which might be neglected without inconvenience to the science.

9. *Can Artificial Hybridisation furnish a mark to determine what it is proper to distinguish as Species?*

I have not the least doubt but that there are some cases where it would be of a slight assistance, and again a greater number where it would not be practicable. Here are some examples of its practical utility.

I have stated before, in speaking of the three species of eatable gourds, that they but slightly differ in outward appearance, and even by their intimate characters, for most botanists cannot clearly distinguish them; Linnæus himself confounded them in one. But these three plants refuse to give hybrids by mutual crossing; they are then three self-governed species perfectly distinct.

M. Dunal, in his Monograph of Solanaceæ, combines into one species *Datura Stramonium* and *D. Tatula*, considering them as simple varieties of the same species. But the produce of their crossing does not vegetate altogether like these two forms; it grows much larger and flowers less, inasmuch as it loses its flower-buds in the seven or eight first branches. This disturbance caused in the vegetation of the mixed produce, is an indubitable sign of a difference in the autonomy of the two parent forms; therefore these forms ought to be held as distinct species.

Datura Metel and *Meteloides* are at least as nearly allied to one another as the two preceding ; but, from the second generation, their hybrids cease to resemble them, and a certain number of individuals return to one or other of the two parent forms. Let us therefore conclude that these forms are specific, that they each have their autonomy and deserve, notwithstanding their affinity, to be distinguished from one another.

Nicotiana macrophylla and *N. angustifolia* combined in the "Prodrômus" of De Candolle with *N. Tabacum*, give hybrids which, after the second generation, manifest a very appreciable commencement of return towards the producing forms. These last have therefore also their manner of growth proper to each of them. Why do we not admit them as distinct in our botanical catalogues ?

But when the forms are so closely allied to one another that they are with difficulty distinguished, their hybrids must differ still less from one another than they differ between themselves. The data furnished by hybridisation, therefore, here lose their value ; but then it becomes a matter of indifference, whether to separate the two forms as distinct species, or to combine them, by the title of simple varieties, under a common specific denomination.

It follows from all we have said, that the application of the terms hybrid and cross is determined by the rank which may be assigned to the individuals from the crossing of which the mixed forms requiring to be named have been produced—that is to say, it is entirely left to the judgment and tact of the nomenclator.

On Diplostemonous Flowers ; with some Remarks upon the Position of the Carpels in the Malvaceæ. By ALEXANDER DICKSON, M.D., Edin. (Plate III.)*

It has long been known that in *Geranium* and its allies, the stamens superposed to the petals are external to those superposed to the sepals. That this is the case is very distinctly seen in the adult state, where the dilated bases of the filaments of the outer stamens overlap those of the inner.

* Read before the Botanical Society of Edinburgh, Feb. 11, 1864.

Before these plants were examined organogenically, the outer stamens were, not unnaturally, assumed to be the older; and, as this involved a want of due alternation of parts, it was imagined that a third and outermost whorl of stamens alternating with the petals must have aborted, the idea being held to be countenanced by the frequent occurrence, in these plants, of five glands outside the andrœcium and alternating with the petals.* When, however, the development of the parts was observed, the unsoundness of this theory became evident; for it was found that the outer stamens are the younger; and, moreover, that the glands do not appear until shortly before the time of blossoming.†

The fact of the younger whorl of stamens being external to the older one is remarkable, as being exactly the reverse of what one would, *a priori*, have expected. The question as to how stamens should be so arranged, is an interesting one, and derives great importance from the researches of Payer having shown that this arrangement, so far from being uncommon, obtains in the greater number of diplostemonous dicotyledons.

In attempting an explanation of this difficulty, I am fully aware of the delicacy of the questions involved; and I would offer the result of my consideration of the subject, more as a suggestion worthy of being kept in view by those who may examine diplostemonous flowers, organogenically or otherwise, than as a definite solution of the problem. In short, I would submit a *possible* solution, to be substantiated or negatived by more extended and comprehensive observation of the facts.

Of diplostemonous flowers, there are two principal forms which demand our attention:—

1st, That in which the younger staminal whorl is the more internal, and the carpels, when of the same number, alternate with the younger stamens. Examples—*Coriaria*,‡

* Maout, Atlas de Botanique, p. 60. Balfour, Class-Book of Botany, p. 788. fig. 1485, with description.

† Payer, Organogénie, p. 59; pls. 12 and 18: the development of the glands in *Erodium* is shewn in pl. 12, figs. 17 and 21.

‡ Ibid., p. 49; pl. 10. *Limnanthes*, in all probability, comes under the same head. Although Payer describes the younger stamens in *L. Douglasii* as the more external, his figure (pl. 10, fig. 21) does not seem to bear out

Agrostemma, *Cerastium* (e.g. *C. triviale*),* *Lasiopetalum* (e.g. *L. corylifolium*),† *Lilium*, &c. (Pl. III., fig. 2.) Comparatively few dicotyledons, but almost all diplostemonous monocotyledons, fall under this head.

2d, That in which the younger stamens are the more external, and the carpels, when of the same number, alternate with the older stamens. Examples—*Geranium*, *Erica*, *Malachium*, &c. (fig. 1.) As I have already mentioned, the greater number of diplostemonous dicotyledons fall under this head.

In the first of these two forms of diplostemony (that in which the younger stamens are the more internal), the arrangement requires no explanation, as it corresponds with the ordinary centripetal evolution of successive whorls upon an axis.

The case, however, is widely different where the younger stamens are the more external. Here we seem to have a *centrifugal* succession of parts upon an axis. Have we any analogies to guide us in explaining this apparent anomaly? What suggests itself most naturally in this regard is perhaps the case of polyadelphous flowers, where the members of each staminal group are usually developed in centrifugal

the statement, for there the circle of the younger stamens appears almost to coincide with that of the older ones, or, if anything, to be somewhat smaller than it. In the more advanced stages, the younger stamens are very distinctly the more internal. At all events, the plant requires re-investigation upon this point.

* I have examined, with great care, the development of the stamens in *Agrostemma Flos-Jovis* and *Cerastium triviale*, and in both of them I can confidently state the younger stamens to be the more internal. I have given figures of young flowers of these two plants (Plate III., figs. 12 and 18). Payer has, on the contrary, stated that the *older* stamens in *Cerastium* are the more internal; but his figures do not indicate this very satisfactorily. To judge from my own experience of *C. triviale*, I should imagine it to be very difficult to determine which staminal whorl is the more internal, when the flower is viewed so much from above as those represented by Payer (*Organogénie*, pl. 72, figs. 7 and 8). In all flowers like *Cerastium*, where the receptacle is very convex, it is very necessary to obtain a completely side view, so as to see the difference in *elevation* of the different parts. In flowers with flat receptacles, on the other hand, the view from above is the most advantageous, as the parts are no longer on higher and lower elevations, but on larger and smaller circles.

† *Organogénie*, pl. 9, fig. 4.—Though the fact is not mentioned in his text, Payer's figure leaves no doubt as to the younger stamens (staminodes) in this plant being on a smaller and more internal circle than the older.

succession upon a cushion-like body which precedes their appearance. Payer has, however, by the most convincing arguments, determined the staminal phalanges of polyadelphous flowers to be compound stamens, the parts of which, when centrifugally developed, correspond to the basipetally developed leaflets or lobes of ordinary leaves; so that in these flowers we have no real examples of centrifugal succession of parts *upon an axis*. A curious arrangement, however, is described by Payer as occurring in *Opuntia*, where vast numbers of stamens are developed in centrifugal succession, and apparently distributed uniformly round the receptacle. We may arrive at a comprehension of this remarkable form, if we direct our attention to certain cases which appear to connect it with more easily intelligible forms. In *Brathys* (*Hypericum*) *prolifera*, Payer has shown that the staminal cushions (which usually remain distinct in the Hypericaceæ) become fused together, at an early period, into a single nearly uniform annular cushion, upon which the stamens make their appearance centrifugally. Again, in *Cistus*, he has shown that, in the early condition, the centrifugally developed stamens exhibit distinct traces of grouping, although the annular cushion, on which they are developed, is always entire. From *Cistus* we pass at once to *Helianthemum*, where all trace of grouping in the stamens disappears,* presenting us with a condition quite analogous to that in *Opuntia*. Such a series of forms leaves us in no doubt that in *Opuntia*, as in *Helianthemum* and *Cistus*, we have merely an extreme case of the fusion of compound stamens, which differs from that in *Brathys* only in being congenital, while in that plant it does not commence until a little after the appearance of the staminal cushions.

In connection with the above, I must not omit allusion to Payer's own determination of the signification of the androecium in *Cistus*; and as the questions suggested by it have no unimportant bearing upon the subject of this paper, I may be excused the apparent digression of commenting upon it. In this plant he has found that the stamens

* *Organogénie*, p. 17, plate iii. fig. 25.

make their appearance in centrifugal succession upon an annular cushion surrounding the centre of the floral axis, and in this wise:—in the first place, a circle of five stamens, superposed to the sepals, makes its appearance on the upper part of the annular cushion; later, alternate with and below these, a second circle of five stamens is developed; still later and lower, ten stamens appear, one on either side of each of the stamens of the second circle; lastly, a great number of stamens continue the centrifugal succession till the annular cushion is completely covered. "From this mode of staminal development," he says, "may we not conclude that the andrœcium of *Cistus* is composed of only two whorls: the one superposed to the sepals, in which the stamens remain simple and are the more internal; the other superposed to the petals, in which the stamens are grouped in five bundles, the stamens in each bundle appearing from above downwards."* Here, I cannot but think, Payer has introduced an unnecessary complication into the subject. His interpretation involves at least one serious improbability,—that in the same flower there should be both simple and compound stamens. What induced him to adopt this opinion was no doubt the consideration that, if the five stamens of the first degree, which are superposed to the sepals, were assumed to be the apices of staminal groups, the stamens of the second degree, which are superposed to the petals, must occupy neutral territory between these groups. He seems not to have taken notice of the fact which his figure† plainly indicates, that the same difficulty occurs again lower down, where there are stamens (apparently of the fifth degree) superposed to the petals, and therefore also on neutral territory. The same thing appears still more strikingly in his figures of *Capparis* (the andrœcium of which he has recognised as being similar to that of *Cistus*), where it will be seen that in every second or third generation of stamens there are some occupying neutral territory. There seems, therefore, to be no more reason for considering the stamens of the second degree as the apices of staminal groups than for so viewing any of the other sta-

* *Op. cit.* pp. 16, 17.

† *Op. cit.* plate 3, fig. 13.

mens of inferior degree which may be superposed to the petals. It appears to me that in *Cistus* and *Capparis*, all the stamens superposed to the petals (including of course the stamens of the second degree) must be looked upon simply as neutral structures, resulting from the coalescence of parts at the points of fusion of the contiguous groups, just as "interpetiolar stipules" are neutral structures, resulting from the coalescence of the stipules of opposite leaves at the points of fusion of the leaf-bases. This analogy will at once be admitted as a legitimate one, when it is remembered that stipules are nothing but lobes of the leaf.

To sum up, I think it is sufficiently evident from the foregoing considerations, that all the instances of indefinite stamens exhibiting an apparent deviation from the law of centripetal succession of leaves upon an axis, may be resolved into cases of compound stamens with development of lobes from above downwards.

From this conclusion we are naturally led to inquire whether those diplostemonous forms, where the younger stamens are the more external, may not in like manner be found to be merely *apparent* deviations from the ordinarily recognised laws of leaf-succession? We are at least bound to show that the phenomenon is incapable of explanation by the action of known laws, before we admit it to be an example of centrifugal succession of leaves upon an axis.*

In the *Geraniaceæ*, which, as I have already mentioned, exhibit this form of diplostemony, the genus *Monsonia* presents the remarkable peculiarity of having *ten* stamens in the younger and outer circle, arranged in five pairs superposed to the petals.† Payer considered that these five

* It may be observed that I have here left out of view the centrifugal evolution of ovules upon central placentas. I have done so, because it is vain to discuss this subject until we have more definite notions than at present prevail on the morphological value of the ovule itself. If the ovule represents a modified leaf, the ôvular groups will probably fall under the same category with the staminal groups of polyadelphous plants, the placental elevations corresponding to the staminal bosses or cushions. If, on the other hand, the ovule is to be viewed as a bud or branch, analogies may be sought for among contracted centrifugal inflorescences.

† Similarly, in the *Zygophyllaceæ*, the five outer and younger stamens of *Zygophyllum*, &c., are replaced in *Peganum* by five pairs of stamens superposed to the petals.—Payer, *Organogénie*, p. 69; pl. 14; fig. 28.

pairs represented the five single stamens of the outer whorl in *Geranium* congenitally deduplicated.* At first sight, this view seems unexceptionable, since there is no doubt as to the parts being homologous. It appears to me, however, that if we were to invert Payer's statement, and say that in *Geranium* there is a congenital connation, in pairs, of the parts of the outer circle, which are distinct in *Monsonia*, we should thereby be enabled at once to explain the apparent anomaly of a younger whorl being external to an older one.

If, in fact, we adopt a line of argument analogous to that which Payer himself has employed in determining the signification of the epicalyx in the Potentillidæ, the whole difficulty, it seems to me, disappears. "In *Fragaria collina*," he says, "we always observe a calicule, composed sometimes of five leaflets alternate with the sepals, sometimes of ten leaflets grouped in pairs which alternate with the sepals; and, as this calicule appears always after the calyx, it cannot be doubted that it is formed by the stipules of the sepals." (*Op. cit.* p. 503.) Now, in the Geraniaceæ we have an outer staminal whorl, which consists sometimes (as in *Geranium*) of five stamens alternate with the parts of the inner whorl, and sometimes (as in *Monsonia*) of ten stamens grouped in pairs which alternate with the parts of the inner whorl. Moreover, these outer stamens appear after the inner ones. The parallelism of these two cases, as regards the number, position, and order of succession, of the parts, is quite complete; for we have in the Geraniaceæ an outer whorl whose relation to the inner one in these respects is exactly the same as that which the apparent outer calycine whorl of *Fragaria* bears to the inner one, or calyx proper.

The presumption, raised by this comparison, that the outer stamens in Geraniaceæ represent the lateral lobes of the inner or primary ones, distinct, or congenitally connate in pairs like interpetiolar stipules, amounts almost to a certainty, when we consider the mode in which the pentadelphous condition of *Monsonia* occurs, where each stamen of the inner whorl is connate with two of those of the outer

* *Op. cit.* p. 60.

—one on either side,—offering the closest analogy to a leaf with lateral lobes, or with adnate stipules. That the presumed interstaminal lobes—if I may so call them—in *Geranium* should so closely resemble, in all essentials, the primary stamens, need not surprise any one who bears in mind instances like *Galium cruciatum*, where the interpetiolar stipules differ in no respects from the leaves between which they are placed.*

It is evident that if the foregoing reasoning holds good as to the Geraniaceæ, we must extend its application to all the numerous cases where a similar diplostemonous arrangement occurs. All such plants, if my view be adopted, must be considered, strictly speaking, as isostemonous, the members of the outer staminal circle consisting merely of the lateral lobes of the primary stamens which form the inner circle.†

We may now proceed to examine what bearing the position of the carpels may have on this question. I have already stated that where the younger whorl of stamens is

* It may perhaps be thought that I am begging a question a little, in this allusion to the stipules of the Galiaceæ. Although the stipulary nature of these organs has been admitted by many very eminent botanists, yet I would not thus have assumed their opinion to be correct, had I not satisfied myself on the subject by examination of the course of development in *Galium aparine* where I can positively testify to the appearance, in the first place, of opposite leaves, followed afterwards by the development of intervening lobes, two or three on either side of the axis. I can hardly doubt that the leaf development in the Galiaceæ has been traced by others, but I have not succeeded in finding any references to it.

† It is perhaps worthy of remark that Payer has shown that the "staminodes" of *Linum* are not developed until after the fertile stamens are so far advanced as to indicate the distinction between anther and filament, and after the carpels have made their appearance. (See *Organogénie*, pl. 13, figs. 6 and 7; with description, p. 67.) Now, if these staminodes in *Linum* represent, as Payer suggests (*loc. cit.* p. 66), the staminodes in *Erodium*, and these last constitute a true whorl of sterile stamens, it is very difficult to understand their very late appearance. If, on the other hand, we adopt the view stated above, as to the younger and outer stamens being merely the lateral lobes of the primary ones, and analogous to leaf lobes or leaf stipules, this difficulty disappears; since there is nothing surprising in such structures appearing at a comparatively late period, and it is quite in accordance with what one observes in the case of staminal groups, where frequently the greater number of the stamens (lobes of the compound stamens) are developed *after* the appearance of the carpels.

the more internal (as in *Agrostemma*, &c.), the carpels, when of the same number, alternate with the younger stamens ; but that, where the younger whorl of stamens is the more external (as in *Geranium*, &c.), the carpels alternate with the older stamens.*

The position of the carpels in the first of these two forms requires no explanation, since it is manifestly in accordance with the usual rule of alternation of floral whorls.

In the second form of diplostemonous arrangement (that in *Geranium*, &c.), the case is apparently very different. Here the carpels alternate with the older stamens, and are thus superposed to the stamens developed next to them in order of time. If the outer and younger stamens in this form be regarded as forming a true staminal whorl, and as of equal value with the older whorl, we must admit a very extensive series of exceptions to the rule of alternation of whorls. On the other hand, if we view the younger stamens here as forming a merely adventitious whorl, the symmetry becomes at once intelligible, since the stamens with which the carpels alternate are then the only ones of primary importance. The fact of my interpretation of this staminal arrangement satisfactorily explaining away such a large number of apparent exceptions to the rule of alternation of whorls, is, I think, no small argument in favour of its being well founded.

It is further to be noted that, when, in a group of plants exhibiting this pseudo-diplostemonous arrangement, the outer and younger stamens disappear, the position of the

* Some may be inclined to think that the circumstance of the carpels occupying different positions in these two cases is not a point of much importance ; that the carpels are only growing out where they have most space for expansion. It is quite true, as matter of fact, that the carpels here do occupy the places where they have most room ; but it appears to me impossible to reflect at all upon the arrangement of the parts of flowers, and admit that this arrangement is primarily dependent upon any such simple law of packing, if I may so express it. That such a law cannot be viewed as the basis of the arrangement of floral parts, must, I think, be apparent, when the not unfrequent instances of superposition of successive whorls are considered ; for in these instances the parts are certainly not developed where there is most room. The fundamental conditions are more likely to be found in the modifications of a contracted spiral, than in the mere influence of surrounding parts upon nascent structures.

carpels is unaffected by such disappearance. This, of course, is only what might have been expected, if the outer stamens are viewed as merely accessory parts. Thus, in the *Ericaceæ*, we have in most of the species of *Ledum* the apparent diplostemony which is frequent in the family, while in *Ledum latifolium* the younger stamens disappear; and yet in this species the carpels are superposed to the petals just as in the others.* In *Epacris*, so nearly allied to the heaths, we have also an absence of the stamens superposed to the petals; and yet the carpels have the same position as in the *Ericaceæ*. Contrasted with this, it is striking to observe the consequence of the disappearance of the younger stamens in a group of plants exhibiting what I believe to be a true diplostemonous arrangement—one where the younger stamens are the more internal. I have already mentioned, as an example of this arrangement, a Büttneriaceous plant, *Lasiopetalum corylifolium*, the organogeny of which has been given by Payer. Here, the fertile stamens, which form the outer and older whorl, are superposed to the petals; the inner and younger whorl consists of staminodes alternate with the older stamens; and the carpels alternate with the staminodes, and are thus superposed to the petals.† In *Hermannia*, on the other hand, where only the fertile stamens superposed to the petals exist, the carpels are no longer superposed to the petals, but are now found superposed to the sepals, occupying, apparently, the place of the missing staminodes. In the *Dombeyæ*, Baillon has described in *Astrapæa* a single whorl of *staminal groups*, with the carpels similarly superposed to the sepals.‡

* I am indebted to a friend for the facts regarding *Ledum*.

† In the Büttnerææ, moreover, Baillon has shown (*Adansonia*, II. p. 168) that in *Büttneria*, &c., the fertile stamens superposed to the petals are, as in *Lasiopetalum corylifolium*, older than the staminodes which alternate with the petals, and that the carpels are also, as in that plant, superposed to the petals. He has not stated which of these whorls is the more internal; but I can scarcely doubt that further investigation will show that the staminodes are the more internal.

‡ Payer has observed (*Organogénie*, p. 45) that in *Melhania* the carpels are similarly superposed to the sepals, and I have been able to confirm his observation. I have also found that in *Pentapetes* the carpels occupy the same position. It is probable that this arrangement obtains in the *Dombeyæ* generally.

The genus *Melhania* exhibits the simplest form of androecium that occurs in

If my explanation of the apparent diplostemony in the Geraniaceæ be admitted, the analogy between such an arrangement and that of polyadelphous stamens will be at once allowed. In this regard, it is not unimportant to inquire whether these two forms may not sometimes pass into one another; and I believe that instances of a passage of this kind do really occur.

the Dombeyæ. In *Melhanis incana* there are five staminodes superposed to the petals, and five stamens which apparently alternate exactly with the staminodes. At first sight, the arrangement of parts in this plant seems very incomprehensible. Here are apparently two staminal whorls, and yet the carpels are superposed to the sepals, as in the isostemonous *Hermannia*. On further examination and reflection, however, I have come to the conclusion that the diplostemony here is only apparent, and that we have merely to do with a much reduced form of the staminal groups which are found in *Astrapeæ*. In *Melhanis incana*, the stamens and staminodes are connected below into a short tube or ring, which adheres to the petals at points corresponding to the bases of the staminodes. When, however, we detach the corolla from the flower, the staminal ring becomes broken into five parts, a stamen and a staminode coming away with each petal. In those flowers which I have examined, the stamen is always to the left side of the staminode, which, as I have already stated, is superposed to the petal, and adherent to it by its base. From the regularity with which the rupture of the staminal ring takes place, it seems reasonable to infer that the fertile stamens do not exactly occupy the indifferent or neutral position between the staminodes which we should expect, were this a case of two alternating whorls.

In *M. decanthera*, where, instead of one stamen, there are two in each interval between the staminodes, I find that of these two stamens there is always a longer and a shorter one, whose position to right or left as regards each other is constant in the same flower, although differing in different flowers. These facts seem to indicate that the pairs of stamens here have not an indifferent relation to the staminodes between which they are placed.

When we consider how easy the transition is from *Melhanis* (through *M. decanthera*) to *Dombeya*, which again is closely allied to *Astrapeæ* where Baillon has distinctly traced the origin of the stamens and staminodes to five groups superposed to the petals, we can scarcely doubt that the androecium of *Melhanis* is referable to a polyadelphous type, and thus the difficulty as to the position of its carpels disappears.

It will probably be very difficult, in the Dombeyæ, without organogenic examination, to apportion the fertile stamens to their proper groups, as they appear to vary very widely in their ultimate relation to the staminodes: thus, in *Dombeya viburniflora* (Bot. Mag. tab. 4568), the fifteen fertile stamens are collected into five bundles, which apparently alternate with the staminodes; while, in an opposite direction, an example may be found in *Trochetia grandiflora* (Bot. Beg. tab. 21), where the stamens and staminodes unite to form five phalanges, each phalanx consisting of a staminode from which four fertile filaments spring, two on either side.

In the *Aurantiaceæ*, we have, in *Citrus*, staminal groups, of which Payer has fully detailed the development. These groups alternate with the petals. In each group, the successive evolution of the stamens extends, in single line, laterally to right and left from a central oldest stamen superposed to a sepal; those stamens, therefore, being youngest, which are furthest removed from the central stamen or terminal lobe of the compound stamen.* Payer, moreover, describes the arrangement in *Tiphrasia trifoliata*, in the same family, as diplostemonous with the carpels superposed to the petals. The fact of the carpels here being superposed to the petals is important, as such an arrangement cannot fail to recall that in *Geranium*, *Erica*, &c., and of course suggests that the diplostemony in *Tiphrasia*, and many other *Aurantiaceæ*, is of the same spurious character as in these plants. Now, if this is the case, does not the andrœcium of *Citrus* bear to that of *Tiphrasia*, a relation exactly analogous to that which the whorl of apparent leaves of *Galium aparine*, consisting of opposite leaves, with a plurality of intervening lobes, does to that of *G. cruciatum*, where the intervening lobes are reduced to one on either side of the axis?

Again, in the *Philadelphaceæ*, there are, in *Philadelphus*, staminal groups, the development of which, as described by Payer, is strikingly like that in *Citrus*; while we have an apparent diplostemonous form occurring in *Deutzia*. But we seem further to have a form intermediate between *Philadelphus* and *Deutzia*, in *Decumaria*, which is described as having thrice as many stamens as petals, there being single stamens superposed to the sepals, and pairs of stamens superposed to the petals.† Thus, in the *Philadelphaceæ*, we have—1st, in *Philadelphus*, compound stamens with indefinite lobes; 2d, in *Decumaria*, a reduction in the number of the staminal lobes, resulting in a condition apparently analogous to that in *Monsonia*; and 3d, in *Deutzia*, an apparent diplostemony, probably analogous to that in *Geranium*, &c.‡

* Payer, *Organogénie*, p. 114, pl. 25.

† Endlicher, *Genera*, p. 1187.

‡ *Vismia*, in the *Ternstroemiaceæ*, is probably another example of a reduced polyadelphous form. In *V. mocanera*, Payer has shown that, of its 15 stamens,

Although we must avoid attaching undue weight to the foregoing facts as to the Aurantiaceæ and Philadelphaceæ—the organogenic evidence being far from complete—yet it may be allowed, I think, that such facts at least heighten the presumption in favour of the justness of my views as to the constitution of the androecium in *Geranium*, &c. If my conjectures are well founded, it is possible that the younger stamens in *Tiphrasia* and *Deutzia* may be found to appear on the same circle with the older ones, just as in *Citrus* and *Philadelphus* all the staminal lobes are on one circle, or nearly so.

Having stated my reasons for believing the diplostemony in *Geranium* and the like, to be merely apparent, I would now allude to certain objections which may be urged against that view.

It may be said that such diplostemony may occur in plants whose leaves are neither lobed nor stipulate. In *Malachium*, for example, the leaves are entire and ex-stipulate. Regarding such an objection, I would observe that, in the case of compound stamens, which I believe affords us the closest analogy with this form of diplostemony, there is not only no necessary coincidence between the lobed or compound condition of the stamens and a similar condition of the leaves in the same plant, but there is not even any neces-

5 are superposed to the sepals, and 10, in 5 pairs, to the petals. The stamens superposed to the sepals are the first developed, and appear simultaneously. In each of the pairs superposed to the petals, however, there is an older and a younger stamen. From Payer's figure (*Organogénie*, pl. 154, fig. 25), it would appear that the position of the older and younger stamen, in each pair, to right and left, as regards each other, is uniform; so that each of the primary stamens superposed to the sepals has an older stamen on the one side and a younger on the other. There is evidently here an alternate succession of secondary staminal lobes, analogous, so far as it goes, to what has been described by Payer in *Malva viscus*, and by Payer and Baillon in *Euphorbia*. The androecium of *Vinea* cannot fail to recall that of *Melhania decanthera*, described in a former note, and I have no doubt that the two cases are quite analogous, only, the apices of the reduced staminal groups are represented by staminodes in *Melhania decanthera*, the intervening unequal pairs of lobes being alone fertile.

Aristotelia, in the Tiliaceæ, is evidently also a reduced polyadelphous form, being described as having 5 inner stamens superposed to the sepals, and 10 outer in 5 pairs superposed to the petals (Payer, *Leçons sur les fam. nat.*, p. 278.)

sary correspondence between the mode of succession of the staminal and foliar lobes, when both stamens and leaves, in the same plant, happen to be lobed. Thus, in the *Hypericaceæ*, *Myrtaceæ*, &c., we have examples of families characterised by their compound stamens, and yet remarkable for the simplicity of their leaves; and in *Cajophora* (*Loasa*) *lateritia*, where the stamens are developed in succession from above downwards, or basipetally, upon the staminal cushions,* I find the pinnæ of the leaf to be developed from below upwards, or basifugally. From such considerations it may be inferred that we need not expect any necessary association of lobed or stipulate leaves with pseudo-diplostemonous flowers.† Should any one be inclined to imagine that the facts I have just been stating at all invalidate Payer's determination of staminal groups as compound stamens, I would have it borne in mind that it is no more surprising that there should be entire leaves and compound stamens in the *Myrtaceæ*, &c., than that in many other plants there should be lobed leaves and simple stamens; or, again, that in *Cajophora* there should be basipetal development of staminal lobes with basipetal development of leaf-lobes, than that these two modes of development should often occur together in the same leaf, as they do in the so-called mixed leaf-formation.‡

* Payer, *Organogénie*, p. 891, pl. 84.

† I may observe, however, that in some plants with pseudo-diplostemonous flowers, the leaves are not only stipulate, but exhibit a tendency to the formation of interpetiolar stipules. A. P. de Candolle has remarked that "several *Geraniaceæ* present this peculiarity [fusion of stipules] in a very evident manner." (*Organographie Végétale*; Paris, 1827; tome i. p. 389.)

In some *Geraniaceæ* I find a very remarkable condition, which, so far as I know, is without parallel in other plants. In *Erodium hymenodes*, for example, where all the leaves are opposite, there are invariably, between each pair of leaves, on the one side of the axis a single, entire or slightly bifid, *interpetiolar stipule*, and on the other side a pair of *free stipules*, the one of which pair overlaps the other, from their bases passing each other. I find a similar arrangement in *E. cicutarium*, *Pelargonium zonale* and allied forms, *P. scutatum*, &c., whenever the leaves happen to be opposite.

Again, in *Spergula* (which, like *Malachium*, has ten stamens, and five carpels superposed to the petals) there are interpetiolar stipules; and similar stipules exist in the allied *Lepigonum*.

‡ *Cobaea scandens* affords a very pretty example of the association of basipetal

It has been suggested to me, that if we were to admit the occurrence of accessory stamens, there would be no reason why these might not sometimes be placed on the same circle with, or even internal to, the primary ones, just as stipulary lobes may appear on the same level with the base of a leaf, or, as in the so-called axillary stipule, above it or on its inner face. I have already, when treating of *Tiphrasia* and *Deutzia*, admitted the possibility of accessory stamens being on the same circle with the primary ones. As to accessory stamens being internal to the primary ones, I think it not at all improbable that such an arrangement may also occur; and, in compound stamens, an analogous phenomenon would be found in the Myrtaceæ, where the staminal lobes appear in centripetal succession as regards the axis.* Now, the possibility of accessory stamens being internal to the primary ones, may be supposed by some to invalidate the morphological distinction of diplostemonous flowers into two forms, which I have endeavoured to estab-

with basifugal development of leaf-lobes. Here, I find a succession of lobes, from a point both upwards and downwards. The upper pair of foliaceous pinnæ appears in the first instance; and, from this, as a point of departure, the cirrhose pinnæ appear in basifugal succession towards the apex of the leaf, while the other foliaceous pinnæ appear successively towards its base. Payer has described an analogous succession from a midway-point upwards and downwards, in the serrations of the leaf-lobes in *Cannabis sativa* (Organogénie, pl. 61, fig. 28; with descr. p. 288).

* Organogénie, pp. 460-1, pl. 98. Payer has, somewhat hastily, I think, compared the compound stamens in the Myrtaceæ to leaves with lobes developed from base to apex, or basifugally. (*Op. cit.* p. 718.) His figures however, distinctly indicate that here, as in the ordinary forms of compound stamens, there is a mesial stamen or lobe of the compound stamen, from which, as a point of departure, the evolution of the other stamens extends; and it appears to me improbable that a basifugal succession of lobes should be initiated by the development of a lobe in the middle line at the base of the compound stamen. The phenomenon seems more naturally explained by supposing that the first developed lobe of the myrtaceous compound stamen corresponds to the first developed or terminal lobe in the ordinary form, in which case the evolution in both forms would be basipetal—the only difference between the two being that, while in the Hypericaceæ, &c., the lobes are developed on the back or outer face of the rachis of the compound stamen (the staminal cushion), in the Myrtaceæ they appear on its front or inner face. In confirmation of this opinion, I may refer to the highly developed staminal groups in *Melaleuca purpurea*, where, in each phalanx, the stamens evidently proceed from the inner face of the flattened and elongated rachis.

lish. As to this, I would state that, although the accessory or non-accessory nature of the younger stamens when internal may be very difficult to determine in some cases, where the carpels, from multiplication, or reduction in number, fail to afford any indications, yet, when we consider the relations of the parts in the flowers of *Coriaria*, *Agrostemma*, *Cerastium*, &c., where the gynœcium is isomerous with the staminal whorls, and the carpels alternate with the younger stamens, we can have no doubt as to such flowers being truly diplostemonous, and therefore morphologically distinct from those of *Geranium*, *Erica*, *Malachium*, &c., where the younger stamens, in being external to the older, occupy a position irreconcilable with the idea of their forming a genuine whorl, and where the carpels alternate with the older stamens.

In the last place, we may consider certain anomalous and somewhat perplexing pseudo-diplostemonous forms, occurring in the Sapindacæ and Polygalacæ.

I have constructed, in accordance with Payer's observations, diagrams of the flowers of *Polygala*, *Kœlreuteria*, and *Cardiospermum*. In these plants, the outer and younger (accessory) whorl of stamens is incomplete.

In *Polygala* (Pl. III. fig. 9), the lower or anterior stamen of the primary, and the upper or posterior stamen of the accessory, whorl are absent.* It is worthy of remark, that in this plant, while the disappearance of the anterior primary stamen appears to be the direct cause of a solution of continuity of the staminal tube, the disappearance of the posterior accessory stamen is unaccompanied by any such solution.

In *Kœlreuteria* (Pl. III. fig. 11), the primary staminal whorl is complete, while the accessory whorl is reduced to three stamens alternate with sepals 1 and 4, 4 and 2, 5 and 3.† In *Pavia* (*Æsculus*), Payer has described a similar arrangement,—only, the accessory stamen between sepals 1 and 4 (and sometimes also that between sepals 4 and 2) is absent, and that between sepals 5 and 3 is occasionally resolved into two, then resembling those in *Peganum* and *Monsonia*.‡

Now, it may seem an objection to my doctrine of accessory stamens, that, in such plants, it requires us to admit

* Organogénie, p. 140.

† Ibid, pp. 150–1.

‡ Ibid, p. 180.

that some members of a primary whorl may be provided with accessory lobes, while others are not. Regarding this objection, it is sufficient for me to advert to the remarkable condition of the calyx of the hundred-leaved rose, where only two sepals are provided on both sides with lateral lobes: two of the remaining three being destitute of them, and the other having a lobe on one side only.

In *Cardiospermum* (fig. 10), the number and position of the stamens are exactly the same as in *Kœlreuteria*: the stamens superposed to the petals being reduced to three, and alternating with the same sepals as in that plant. It cannot be doubted that, in the two cases, the androecia are essentially the same, although it is to be remarked that, while the three carpels are, in *Kœlreuteria*, superposed to sepals 1, 2, and 3, in *Cardiospermum* they alternate with sepals 1 and 4, 4 and 2, 5 and 3. In *Cardiospermum*, however, the stamens in each whorl, instead of appearing simultaneously, as in *Kœlreuteria*, are developed in a remarkable succession, which I have indicated by the numbers accompanying the stamens in the diagram (fig. 10). In the first place, the two stamens which alternate with sepals 1 and 4, 4 and 2, make their appearance; next, the two stamens superposed to sepals 1 and 2; then, the three stamens superposed to sepals 3, 4, and 5; and lastly, the stamen alternate with sepals 3 and 5.* Payer has endeavoured to render intelligible the remarkable mode of staminal succession, in this and other analogous cases, by supposing that the irregularity of development, which so frequently manifests itself after the appearance of floral parts, is congenital in such cases; and it is hardly possible to doubt that his explanation is correct. The anomalous succession is evidently the effect of a disturbing force delaying or arresting, for a time, the appearance of some of the parts, and thus materially affecting the order of staminal evolution. This disturbing force seems to act in quite an arbitrary manner, as it affects different plants in very different manners: thus—to take examples from Payer's work—in *Viola odorata*, the stamens appear successively from before backwards; in the *Resedaceæ*, from behind forwards; while, in *Cardio-*

* Organogénie, pp. 150–1.

spermum, the succession may be described, in general terms, as obliquely from side to side. As I have already stated, the two stamens which first appear in *Cardiospermum*, are those which alternate with sepals 1 and 4, 4 and 2. Now, it may be supposed by some to be a formidable, if not fatal, objection to my views, that two of the supposed accessory stamens should appear before the primary ones to which they belong. At first sight, such a mode of appearance seems very improbable; yet, when we consider the arbitrary manner in which the disturbing force affects the order of staminal succession, we need scarcely be surprised even at such a result. At any rate, it cannot, *a priori*, be said to be more improbable that the appearance of primary staminal lobes should be delayed by a disturbing force until after that of their accessory or lateral lobes, than that the appearance of a normally older staminal whorl should be delayed until after the appearance of some of the parts of a normally younger one, which must be admitted on the ordinary supposition of there being two genuine staminal whorls. I do not think, therefore, that the case of *Cardiospermum*, although certainly a very strange one, can fairly be urged as invalidating my views.

While engaged in the attempt to determine the morphological constitution of double staminal whorls, I was led, incidentally, to examine the position of the carpels in some of the *Malvaceæ*.

I have already stated, regarding the *Büttneriaceæ*, that where, in these plants, there are two staminal whorls, the carpels (as alternating with the younger staminal whorl) are superposed to the petals,—*e.g.* in *Lasiopetalum*, *Büttneria*, *Melochia*, &c.;—but that where the andræcium is reduced to a single whorl (of simple or compound stamens), the carpels are superposed to the sepals, as in *Hermannia* and the *Dombeyæ*.

The researches of Payer leave no doubt that the andræcium of the *Malvaceæ* consists essentially of a single whorl of five compound stamens, superposed to the petals. With a staminal arrangement so closely analogous to what Baillon

has described in *Astrapæa*, we may expect to find the carpels, when of the same number, superposed to the sepals, as in that plant. Payer, indeed, has stated that the *Hernandaceæ*, *Dombeyæ*, and *Bombacææ*, in which the carpels are superposed to the sepals, are distinguished thereby from the *Malvaceæ*, *Sterculææ*, and *Lasiopetaleæ*:* but, as regards the *Malvaceæ*, I believe it can only have been through an oversight that he has associated them with the *Lasiopetaleæ*, since he describes the carpels in *Hibiscus* as being superposed to the sepals.† So far as my observations extend, the rule seems without exception, that in 5-carpellary *Malvaceæ* the carpels are superposed to the sepals, just as in the *Dombeyæ*. I have ascertained the occurrence of this arrangement in the following:—(*Hibiscææ*) *Hibiscus*, *Paritium*; (*Sidææ*) *Lagunea*. Moreover, in the *Malopeæ*, where Payer has shown the gynœcium to consist of five carpellary groups, I find that, in *Malope*, these groups are superposed to the sepals; so that in this plant we have a similar arrangement to that in *Hibiscus*—only, the five simple carpels of *Hibiscus* are replaced in *Malope* by five carpellary groups (or compound carpels, as they may be termed, being developments evidently of an analogous character with the compound stamens of polyadelphous plants). Payer has described the angles of the pentagon formed by the carpellary groups in *Malope* as superposed to the sepals: but I am quite satisfied that his statement is erroneous. In flowers at or near maturity, there is, sometimes, a slight want of perfect superposition of the carpellary groups to the sepals: but this seems to be never to such an extent as to justify Payer's statement. In the early condition of the ovarian pentagon, the superposition of its angles to the petals is quite unmistakeable. The cavity of the staminal tube is five-sided, the sides alternating with the petals; and the carpellary pentagon, in its origin, is pretty accurately fitted into the bottom of this cavity.‡

The superposition in many *Urenææ* of the loculi to the

* *Organogénie*, pp. 44-5.

† *Ibid.*, p. 83.

‡ I have not had an opportunity of examining the position of the carpellary groups in *Kitaibelia*; and there seems to be considerable confusion in Payer's works, on this point, as these groups are described in the "*Organogénie*" (p. 84) as alternate with, and in the "*Eléments de Botanique*" (p. 209) as superposed to, the petals.

petals is only an apparent exception to the rule I have stated above, as to the position of the carpels in 5-carpellary *Malvacæ*; for the researches of Payer on the organogeny of *Pavonia* leave no doubt that in this tribe the five loculi merely represent the fertile members of a circle of ten carpels, to which the ten styles correspond. In *Pavonia*, the gynœcium, in its origin, consists of ten carpellary mamillæ. Of these, however, only five have loculi developed in connection with them—every second carpel being, so to speak, barren. The ten carpels all equally develope styles; so that in the advanced condition there are five styles prolonged upwards from the loculi, and five continuing the lines of the dissepiments.* In *Malvariscus*, Payer describes the loculi (corresponding to the fertile carpels) as superposed to the petals.† In *Urena* (*U. americana*, *U. lobata*, *U. scabriuscula*, *U. sinuata*), I find the same arrangement. In *Pavonia*, I have ascertained the remarkable fact that the loculi are sometimes superposed to the sepals, and sometimes alternate with them. Thus, in *P. typhalea*, *P. begoniæfolia* (Gardner), *P. odorata*, *P. umbellata*, and *P. zeylanica*, the loculi are superposed to the sepals; while, in at least one species, named in the Edinburgh University Herbarium *P. hastata*,‡ the loculi are certainly alternate with them, as in *Malvariscus* and *Urena*.§

* Organogénie, p. 85, pl. 7.

† Leçons sur les fam. nat. des plantes, p. 281.

‡ I have expressed myself thus guardedly as to the specific name of this plant, because, by its indefinite stamens, it differs from that to which Baillon refers as *P. hastata* (Adanson, II. p. 176), which is described by him as having only five stamens in the adult state. The Edinburgh plant agrees with the description of *P. hastata* in Decandolle's "Prodromus" (vol. i. p. 443), in its lanceolate hastate dentate leaves, axillary unifloral pedicels, and five-leaved involucre. I cannot say much as to the colour of the petals, except that a deep red or purplish blotch remains at the base of each. The whole plant (especially the stem, the under side of the leaves, the involucre, and the sepals) is downy, being covered with a short stellate pubescence. The plant which Payer has examined as *P. hastata* appears to perfect a considerable number of stamens, as is seen in his representation of the andrœcium "shortly before blossoming," where there would seem to be 25 stamens, or thereabouts (Organogénie, pl. 7, fig. 9; with description, p. 38).

§ The position of the fertile carpels seems to offer a much more important character by which the genus *Pavonia* may possibly be disintegrated, than any derived from the awned or awnless condition of the fruit, the relative length of the involucre to the calyx, &c.



Fig 1



Fig 15



Fig 16

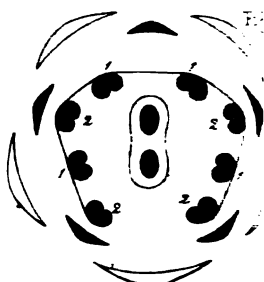


Fig 2

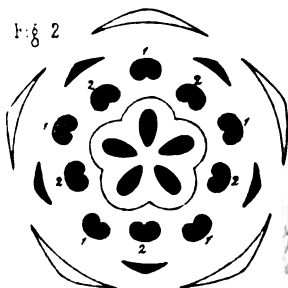


Fig 17

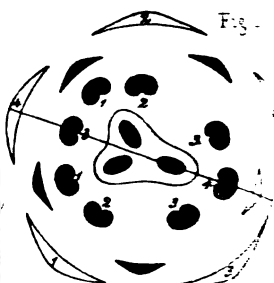


Fig 14



Fig 3

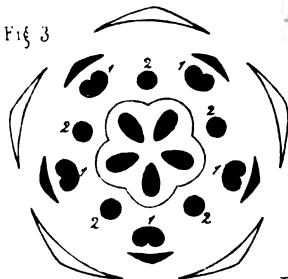


Fig 18



Fig 13



Fig 5

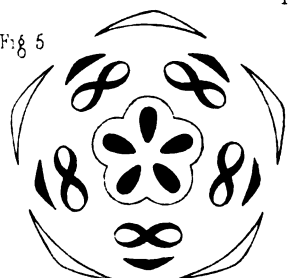


Fig 19



Fig 12

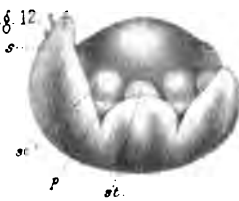


Fig 4

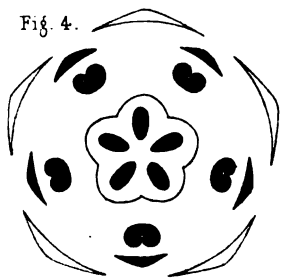


Fig 20

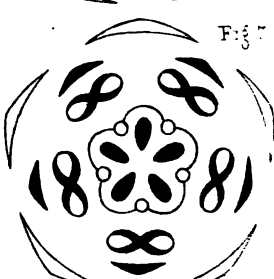
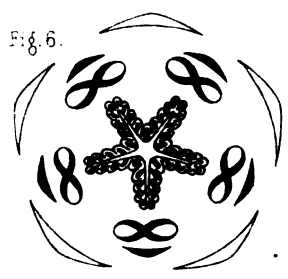


Fig 6



In constructing those diagrams which illustrate arrangements in the *Malvaceæ* (figs. 5, 6, 7, and 8), having found great difficulty in giving diagrammatic expression to the staminal groups, I have represented the said groups by symbols of infinity, which conveniently enough indicate the indefinite number of the staminal lobes.

Explanation of Plate III.

[Figs. 1-18 are from my own designs. Figs. 14 and 15 are taken from Payer's "*Organogénie*," plate xiii. figs. 28 and 32. The diagrams are constructed with the utmost conventional uniformity, being merely intended to represent the *position* of the parts, not their *form*. In the diagrams, the posterior aspect of the flower is above, the anterior below, and the stamens are numbered in the order of their appearance.]

- Fig. 1. Arrangement in *Geranium*, *Malachium*, &c. Younger (accessory) stamens external. Carpels alternate with older (primary) stamens.
- Fig. 2. Arrangement in *Coriaria*, *Agrostemma*, *Cerastium*, &c. Younger stamens internal, probably forming a genuine whorl. Carpels alternate with the younger stamens.
- Fig. 3. Arrangement in *Lasiopetalum corylifolium*, and probably in *Büttneria*, *Melochia*, &c. Outer and older stamens fertile, and superposed to the petals; inner and younger sterile, and alternate with the outer. Carpels, as in fig. 2, alternate with the younger (sterile) stamens.
- Fig. 4. Isostemonous arrangement in *Hermannia*. Fertile stamens, as in the last, superposed to the petals. The carpels are superposed to the sepals, apparently replacing the staminodes of the last form.
- Fig. 5. Arrangement in *Hibiscus*, *Paritium*, and *Lagunea*. Same as last form, except that, instead of five simple stamens, there are five staminal groups (indicated by symbols of infinity).
- Fig. 6. Arrangement in *Malope*. Same as last, except that, instead of five simple carpels, there are five carpellary groups.
- Fig. 7. Arrangement in *Pavonia Typhalea*, *P. begoniaefolia*, *P. odorata*, *P. umbellata*, and *P. zeylanica*. Staminal groups as in figs. 5 and 6, superposed to the petals. Ten carpels; five fertile, superposed to the sepals, and five sterile, superposed to the petals. The sterile carpels are indicated by small circles alternate with the loculi.
- Fig. 8. Arrangement in *Malcoaviscus*, *Urena*, *Pavonia* sp. (*hastata* ?). Same as last form, except that those carpels which are sterile there, are fertile here, and *vice versa*.
- Fig. 9. Arrangement in *Polygala*, as described in the text.
- Fig. 10. Arrangement in *Cardiospermum*, as described in the text. The sepals are numbered in the order of their appearance. Sepals 3 and 5 become connate, and the petal (indicated in outline) which alternates with them aborts. If an oblique line be drawn, as in the diagram, through sepal 4 and the abortive petal, the parts are arranged symmetrically on either side of it. This imaginary line, by torsion of the peduncle, becomes antero-posterior, the abortive petal becoming posterior (superior). See Payer's "*Organogénie*," p. 158.

- Fig. 11. Arrangement in *Kalreuteria*, as described in the text. As in *Cardiospermum*, the petal alternating with sepals 8 and 6 aborts.
- Fig. 12. Young flower of *Agrostemma Flos-Jovis*, just before the appearance of the carpels. The younger stamens are internal to the older ones. *s*, sepal; *p*, petal; *st*, older stamen; *st'*, younger stamen.
- Fig. 13. Young flower of *Cerastium triviale*, at same stage as the last. The younger stamens, as in *Agrostemma*, are internal to, or on a higher level than, the older ones. *sa*, *sp*, *sl*, anterior, posterior, and lateral sepals; *p*, petal; *st*, older stamen; *st'*, younger stamen; *ax*, convex extremity of the floral axis; *b*, *b*, lateral bracts, with secondary floral axes β^1 , β^2 , developed in their axils.
- Fig. 14. (From Payer). Young flower of *Monsonia ovata*. *s*, sepals; *p*, petals; *st*, older and inner (primary) stamens, superposed to the sepals; *st'*, younger and outer (accessory) stamens, superposed in pairs to the petals; *cp*, carpels, superposed, as in *Geranium*, to the petals.
- Fig. 15. (From Payer). Androecium and pistil from a flower of *Monsonia ovata*, at the time of blossoming. Each of the primary stamens has become connate with the two accessory stamens adjacent to it, one on either side, so that the androecium seems now composed of five phalanges superposed to the sepals.

The Classification of Animals based on the Principle of Cephalization. No. I. By JAMES D. DANA. Communicated by the Author.*

(Continued from page 102).

3. Classification of Animals.

1. *Subkingdoms*.—Of the four subkingdoms, first recognised by Cuvier and since by most zoologists, the Vertebrate, Articulate, and Molluscan, are typical, or of the true *animal-type*, and the Radiate is degradational, being *plant-like* in type. Using the terms alphetypic, betatypic, and gammatypic, *simply as a numbering of the grades of types* (see p. 96), their relations are as follows:—

Alphetypic,	.	.	.	.	1. Vertebrates.
Betatypic,	.	.	.	.	2. Articulates.
Gammatypic,	.	.	.	.	3. Mollusks.
Degradational,	.	.	.	.	4. Radiates.

An important dynamical distinction between Mollusks and Articulates has been already suggested by me.

2. *Classes of Vertebrates, Articulates, Mollusks, and Radiates*.—(1.) The classes of *Vertebrates* are four (see page 78),

* From the American Journal of Science and Arts, Vol. xxxvi., Nov. 1863.

namely, Mammals, Birds, Reptiles, and Fishes,—three of which are typical, of different grades, parallel with the above.

(2.) The classes of *Articulates* are but three, Insecteans, Crustaceans, and Worms. I have already shown that the three divisions of Insecteans, namely Insects, Spiders, and Myriapods, are distinguished by characteristics analogous to those which separate the divisions of Crustaceans,—Decapods, Tetradeapods, and Entomostracans. The facts on this point are briefly presented on page 97. Insects and Spiders do not, in fact, differ more widely in external form or in structure than Decapods and Tetradeapods.

Insecteans and Birds express in different ways the same type-idea,—that of aerial life, Birds being flying Vertebrates, and Insects flying Articulates; and, in accordance, they are of the same grade of type, both being *betatypic*. This follows, further, from the fact that there are but two grand divisions of Insecteans above the degradational division, that of Worms.

(3.) Among *Mollusks*, there are two well-characterised classes, the *first* including the *ordinary* Mollusks; the *second*, the *Ascidoids*, or the Brachiopods and Ascidians, which are mostly attached species and thus hemiphytoid. Besides these, there are the Bryozoans, which either make a third division under the Ascidoids (Edwards having long since pointed out their relations to the Ascidians); or they constitute a *third* class of Mollusks, characterised by being polyp-like both in external appearance and in being attached, and hence doubly hemiphytoid.

(4.) The *Radiates* are all degradational in their relations to the animal-type. But under the *Radiate-type*, the species of the first two classes are within type-limits, while those of the third are degradational, since almost all are attached and very inferior in type of structure, being the most phytoid of phytoid animals. The grades of structure, as marked in the digestive system, are as follows: (1.) Having approximately normal viscera, as in Echinoderms; (2.) Having, for the digestive system, only a stomach cavity, with vessels, imbedded in the tissues, radiating from it, as in Acalephs; (3.) Having, for the same, no system of viscera or radiating vessels; but only a central stomach surrounded by a cavity more or less divided at its sides by partitions as in Polyps.

The following table presents the relations and the parallelisms of these classes, and of each to the subkingdoms:—

	Subkingdoms.	Vertebrates.	Articulates.	Mollusks.	Radiates.
a.	Vertebrates.	Mammals.	—	—	—
β.	Articulates.	Birds.	Insecteans.	Ordinary.	Echinoderms.
γ.	Mollusks.	Reptiles.	Crustaceans.	Ascidoids.	Acalephs.
D.	Radiates.	Fishes.	Worms.	Bryozoans?	Polyps.

Arranging the divisions according to the relations of the groups to the *animal-type*, instead of the special type of each class, the table takes the following form :—

	Subkingdoms.	Vertebrates.	Articulates.	Mollusks.	Radiates.
a.	Vertebrates.	Mammals.	—	—	—
β.	Articulates.	Birds.	Insecteans.	—	—
γ.	Mollusks.	Reptiles.	Crustaceans.	Ordinary.	—
α. D.	—	Fishes.	Worms.	Ascidiods.	—
b.	—	—	—	Bryozoans.	—
c.	Radiates.	—	—	—	Echinoderms.
d.	—	—	—	—	Acalephs.
e.	—	—	—	—	Polyps.

The letters *c*, *d*, *e*, stand for different grades of phytoid degradational, *b*, hemiphytoid, and *a*, degenerative. The blank interval between Mollusks and Radiates is filled up by the inferior divisions of the higher subkingdoms.

We may now consider the subdivisions under some of the classes; and first, those of Vertebrates.

3. *Higher subdivisions of the class of Mammals.*—The higher subdivisions of the class of Mammals are four in number: Man, Megasthenes, Microsthenes, and Oötocoids, as explained in the preceding volume of the American Journal, Man is shown to stand apart from the Megasthenes on precisely the same characteristic that separates the two highest orders under the classes severally of Insecteans and Crustaceans; for, in passing from Man to the brute Mammals, there is a transfer of the forelimbs from the cephalic to the locomotive series.

Moreover, a study of the Vertebrate skeleton has shown that the forelimbs in the Vertebrate type, as well explained by Professor Owen, are *cephalic appendages*, being normally appendages to the posterior or occipital division of the head. In the Fish, these forelimbs (the pectoral fins) have at any rate an actual *cephalic position* (back of which position they are thrown, by displacement, in other Vertebrates). Now, in Man, they are not only cephalic in normal structural relations, but *cephalic* also in *use*. The transfer of these cephalic organs to the locomotive series, by which the brute structure is made, is a manifest degradation of the type. Man is thus the only Vertebrate in which the Vertebrate-type is expressed in its perfection, and therefore occupies *alone* the sublime summit of the system of life.

Three of the orders of Mammals, namely, Man, Megasthenes, and Microsthenes, are typical of different grades, and one, Oötocoids, is semidegradational.

The Oötocoids may be divided into three groups—a *megas-*

thenic, a *microsthenic*, and a *degradational*; the *first* to include the genera *Phalangista*, *Dasyurus*, *Macropus*, *Diprotodon*, &c.; the *second*, *Perameles*, *Didelphys*, *Phascolomys*, *Echidna*, &c., or *Marsupial Insectivores*, *Rodents*, and *Edentates*; the *third*, *Ornithorhynchus*.

The following table presents to view the subdivisions of Mammals and its orders. Under *Oötocoids*, the relations of the two higher groups are indicated by the above adjectives, without giving them special names:—

	Mammals.	Megasthenes.	Microsthenes.	Oötocoids.
<i>α.</i>	Man.	Quadrumanes.	Chiropters.	—
<i>β.</i>	Megasthenes.	Carnivores.	Insectivores.	Megasthenic.
<i>γ.</i>	Microsthenes.	Herbivores.	Rodents.	Microsthenic.
<i>D.</i>	Oötocoids.	Mutilates.	Edentates.	Ornithorhynchs.

4. *Higher subdivisions of the classes of Birds, Reptiles, and Fishes.*—(1.) In the class of *Birds*, there are three grand divisions: the first two, as recognised by Bonaparte, are the *Altrices* (Rapacious birds, *Perchers*, &c., and other birds that feed their young until they can fly), and the *Præcoces* (or the *Gallinæ*, *Anseres*, *Ostriches*, &c., which feed themselves as soon as hatched). The third includes the *Reptilian Birds* or *Erpetoids* (p. 77). The terms *Pterosthenics* and *Podosthenics* apply equally well with *Altrices* and *Præcoces* to the two higher divisions of *Birds*, as explained on page 83, and have an advantage in their direct dynamical signification.

The type of ordinary *Birds* (or *Pterosthenics* and *Podosthenics*) is stated on page 95 to be essentially *limitate*, like that of *Insects*, while the type of *Erpetoids* is *multiplicate*, like that of *Myriapods* or of ordinary *Reptiles*; so that the relation of *Erpetoids* to the higher division of *Birds* is in an important respect analogous to that of *Myriapods* to the higher division of *Insecteans*.

(2.) In the classification of *Reptiles* there are three prominent types of structure recognised by *Erpetologists*; (1.) That of the *Cheilonians*; (2.) That of the *Lacertoids* (including *Saurians*, *Lizards*, *Snakes*); and (3.) The *degradational* or *hemitypic* one of *Amphibians*. It is now well known that *Snakes* and *Lizards* are alike in type of structure, the two groups graduating almost insensibly into one another, some species ranked as *Lizards* being footless like the *Snakes*. The *Snakes* constitute the *degradational* group under the *Lacertoids*. The *Amphibians*, constituting the third order, are on the same level with the *Erpetoid Birds* and the *Oötocoid Mammals*, as presented in the following table.

The three orders of Reptiles—Chelonians, Lacertoids, and Amphibians—make a parallel series with the three lower classes of Vertebrates; the Chelonians representing the Birds, to which they approximate in some points, besides being betatypic like them; the Amphibians representing the Fishes, with a still closer approximation between the two; while the Lacertoids are the typical Reptiles. The Chelonians might be viewed as *hemitypic* Reptiles; not *hypotypic* like the Amphibians, but *hypertypic*, like the Selachians and Ganoids among Fishes.

(3.) *Fishes* are all degradational species in their relations to the animal-type. The two higher groups, or those of Selachians and Ganoids, as already explained (p. 96), are *hypertypic*. The third, including Teliosts, is *typical* if viewed with reference to the Fish-type. Below these, the Dermopters or Myzonts (including Amphioxus, Myxine, &c.), constitute an inferior *hypotypic* or degradational group,—that is degradational in its relations to typical Fishes (p. 94). Thus *typical* Fishes are gammatypic in their relations to other Vertebrates, while the alphetypic and betatypic groups are *hypertypic* orders.

The following table exhibits the relations of the orders in the classes of Birds, Reptiles, and Fishes; and, for comparison, those of Mammals are added:—

	Mammals.	Birds.	Reptiles.	Fishes.
Alphetypic,	Man.	—	—	Salachians.
Betatypic,	Megasthenes.	{ Altrices, or Pterosthenics.	Chelonians.	Ganoids.
Gammatypic,	Microsthenes.	{ Præcoeces, or Podosthenics.	Lacertoids.	Teliosts.
Hemitypic or Degradational,	{ Oötooids,	Erpetoids.	Amphibians.	Dermopters.

We pass now to Articulates.

5. *Subdivisions of the classes Insecteans, Crustaceans, and Worms into Orders.*—(1.) The higher subdivisions in each of the classes, *Insecteans* and *Crustaceans*, are three in number, none existing above the betatypic grade, which is that of Articulates among the subkingdoms, and of Insecteans among Articulates.

(2.) *Worms* are of four types of structure. First, *Annelids*, or *typical* Worms, including the Branchiates, Abranchiates, and Nematoids—the last the degradational group, and showing this in the obsolete body-articulations and some internal characters.—Second, *Bdelloids* or *Molluscoid* Worms, including the Hirudines or Leeches, Planarians and Trematodes; characterised by obsolescent or obsolete body-articulations, and by often wanting the nervous ganglia, excepting the anterior; by usually a Gastropod-like breadth and aspect, an *amplificate* feature; by being in

general *urosthenic*, even the highest having a caudal disk for attachment; and in an up-and-down movement of the body in locomotion, *Mollusk-like*, instead of the worm-like lateral movement of the Annelids. The fact of this mode of movement has been recently made known to the writer by Dr Wm. C. Minor, as a distinctive feature of the Bdelloids. Quatrefages remarks that the Planarians and Trematodes may well be regarded degraded forms of the Hirudines, and the three tribes are arranged in one group by Burmeister.—Third, *Gephyreans* (of de Quatrefages), or *Holothurioid* (*Radiate-like*) Worms, including the genera, *Echiurus*, *Sipuncula*, &c.*—Fourth, *Cestideans*, or *Protozoic* Worms, including the Cestoids, in which there is no normal digestive system, and the segments are independently self-nutrient.†

The orders of these classes of Articulates are the following:—

	Insecteans.	Crustaceans.	Worms.
Alphatypic,	—	—	—
Betatypic,	Insects.	Decapods.	Annelids.
Gammatypic,	Spiders.	Tetradecapods.	Bdelloids.
a. Degradational,	Myriapods.	Entomostracans.	Gephyreans.
b. „	—	—	Cestideans.

6. *Subdivisions of the Orders of Insecteans and Crustaceans into Tribes.*—(1.) The orders of *Insecteans* have each three divisions, excepting that of *Myriapods* in which but two have been recognised. The three of *Insects* are indicated on pages 83, 98. The fact that *Insects* are, in type-idea, *flying* Articulates, gives special importance to the wings in classification. The *first* order includes the *Prosthenics*, in which the anterior wings are flying wings, as the Hymenopters, Dipters, Neuropters, Lepidopters, and Homopters. The *second* consists of the *Metasthenics* or *Elytrotters*, in which the anterior wings are not used in flying, or but little so, as the Coleopters, Strepsipters, Orthopters, and Hemipters. The Hemipters and Homopters, united in one tribe by most entomologists, are hence profoundly distinct. The *third* tribe, or *Apters*, embraces the Lepismids and Podurellids; the remaining Apterous insects being distributed among the other

* The Holothurioid characteristics are well exhibited by de Quatrefages in Part ii. p. 248 and beyond, of "*Recherches Anatomiques et Zoologiques faites pendant un voyage sur les Côtes de la Sicile*," &c., in 8 vols. or parts, the second by de Quatrefages. Paris.

† The *Acanthocephali*, according to van Beneden and Blanchard, are Nematoids (with which they agree in form and general structure), although without a digestive system. Blanchard states that there is reason for believing that the digestive system becomes atrophied with the growth of the animal, and mentions that cases of like atrophy occur even in species of *Gordius* and *Nemertes*.

groups, as suggested by different entomologists. The *Lepismæ* show their degradational character in their larval forms and in other approximations to the *Myriapods*, and the *Podurellids* appear to be still inferior in having the abdomen elliptic in some segments.

(2.) The Orders of *Spiders* suggested by the principles of cephalization are in precise parallelism with those of the Decapod and Tetradeapod Crustaceans. They are, first, *Araneoids*, including all the *Pulmonates*, except the Pedipalps; second, *Scorpionoids*, or the Pedipalps from among the *Pulmonates*, and the Chelifer group from among the *Trachearians*; third, *Acaroids*.

The *Araneoids* or *Brachyural Spiders*; the *Scorpionoids*, *Macrural*; while the *Acaroids* are *degradational*. The last show their degradational character in having no division between the abdomen and cephalothorax; so that, while *Insects* have the body in *three* parts, head, thorax, and abdomen, and ordinary *Spiders* in *two*, cephalothorax and abdomen, the *Acaroids* have it *undivided* (page 86). Thus, one of the most prominent characteristics marking the descent from *Insects* to *Spiders* becomes the characteristic of a further descent among *Spiders* themselves—illustrating a common principle with regard to such subdivisions. The propriety of making the *Acaroids* a distinct group appears therefore to be well sustained.

The usual subdivision of *Spiders* into *Pulmonates* and *Trachearians* depends on *internal* characters, which is not the case with any other subdivisions in the table beyond. Moreover, these names, though *seeming* to mean much, are not based on any *functional* difference between the groups. *Spiders* have many relations to *Crustaceans*; and it is natural that the subdivisions in both should depend on the same methods of cephalization, the amplificative and analytic (p. 98).

(3.) The two orders of *Myriapods* are examples, one of case *a*, the other of case *b*, under multiplicative decephalization (p. 85).

The close relations between *Isopods* and the higher *Myriapods*, suggest that they are of like grade under their respective types, that is, betatypic.

(4.) *a*. Under *Decapod Crustaceans*, the subdivisions are *three*, as already remarked upon by the author.*

The *Anomurans* are only degradational *Brachyurans*, and do not represent an independent type of structure. The *Schizopods*, similarly, are degradational *Macrurans*, with which they should be united. The *third* type is that of the *Gastrurans*, which are peculiar among *Decapods*, in having the viscera extending into the abdomen, one of the marked degradational features of the type.

* See also Amer. Jour. of Science and Art, vol. xxv. [2], pp. 337, 338.

They are the Stomapods of Latreille ; but this author, in his last edition, made the group, in connection with the Schizopods, co-ordinate with that of Decapods. Being co-ordinate with Brachyurans and Macrurans, the change of name is necessary.

b. The *Tetradecapods* include two divisions precisely parallel with the first two of the Decapods, the first literally *brachyural*, the second *macrural*. (See p. 97 of this volume.) The *Anisopods* of the writer, are degradational Isopods, just as the Anomurans are degradational Brachyurans. The Lemodipods (Caprellids, &c.) are only degradational Amphipoda, the structure of the two being essentially the same in type. Hence, neither the Lemodipods nor the Anisopods are an independent type corresponding to a *third* division.

The *third* subdivision probably is made up of *Trilobites*, although these are generally regarded as Entomostracans. One of the most prominent marks distinguishing Entomostracans from Tetradecapods is the absence of a series of abdominal appendages. It is highly improbable that the large abdominal (or caudal) plate of an Asaphus, or the many-jointed abdomen of a Paradoxides, Calymene, &c., should have been without foliaceous appendages below ; and if these appendages were present, the species were essentially Tetradecapods, although degradational in the excessive number of body-segments.

c. *Entomostracans* (or Colopods, as they are more appropriately styled) embrace four orders. First, *Carcinoids* (as named by Latreille) consisting of the Cyclops group (Copepods of Edwards), whose species have a strong Macrural or shrimp-like habit ; to which should be added the Caligoids, (Cormostomes of the writer, Siphonostomes of others), since they are essentially identical in type of structure with the Cyclopoids, as may be seen on comparing *Sapphirina* of the latter with *Caligus*.—Second, *Ostracoids* (or the Daphnia, Cypris, and Limnadia groups), which have, besides a bivalve carapax more or less complete, a much more elliptic abdomen than the Carcinoids, it being short, incurved, and without a lamellar terminal joint or terminal appendages.—Third, *Limuloids*, which have the abdomen still more elliptic, it being reduced to a mere spine, or nearly obsolete, and which have the mouth-organs all perfect feet and the only locomotive organs. (The joint across the carapax of the *Limulus* corresponds in position to a suture or imperfect articulation in the carapax of the Caligi, &c.)—Fourth, the *Rotifers*, a low Protozoic grade of degradation, in which all members are wanting, and locomotion is performed by cilia. The Phyllopods are distributed between the first two divisions.

The Rotifers are sometimes arranged under Worms. If they are degradational species of a limitate type, they are Crustaceans ;

and if of a multiplicate, they are Worms. The very small number of segments present, when any are distinct, the character of the dentate mandibles (for mandibles are *not* found in the inferior subdivisions of Worms), and the resemblance in the form of some species to Daphniæ and other Entomostracans, sustain the view that they are Crustacean.

The Cirripeds appear to be only attached, amplificate Ostracoids. (See pages 84, 85.)

The subdivisions of the orders of Insecteans and Crustaceans are then the following :—

	Insects.	Spiders.	Myriapoda.	Decapoda.	Tetradecapa.	Entomostr.
<i>α.</i>	—	—	—	—	—	—
<i>β.</i>	Prosthenies or Ctenoptera.	Araneoids.	Chilopods.	Brachyurans.	Isopoda.	Carcinoids
<i>γ.</i>	Metasthenies or Elytrophera.	Scorpionoids.	Diplopods.	Maecurans.	Amphipoda.	Ostracoids.
<i>α.</i>	D. Aptera.	Acaroids.	?	Gastrurans.	Trilobites. ?	Limuloids.
<i>β.</i>	D. —	—	—	—	—	Rotifers.

7. *Subdivisions of the orders of the class of Worms.*—On the true method of grouping the typical (Branchiate and Abranchiate) Annelids, I here make no suggestions. The Cystics are there included with the Cestoids. If any of the *simple* Cystics are really adults, they may possibly make a second subdivision of the Cestideans.

8. *Subdivisions of the classes of Mollusks.*—The ordinary Mollusks include three orders, as usually given: (1.) *Cephalopods*, (2.) *Cephalates*, and (3.) *Acephals*; of which the first two correspond to different grades of typical Mollusks, and the last is degradational in its relations to the type, the species being imperfect in the senses and means of locomotion.

The Ascidioid Mollusks comprise (1.) *Brachiopods*, and (2.) *Ascidians*, with perhaps the *Bryozoans* as the *third* order. If the last, however, be made a *third class*, as already suggested (though with hesitation), there is no third order, unless the inferior of the compound Ascidians, having water-apertures to a group of individuals instead of to each one, and the mouth-opening of each usually *radiated* (the number of rays *six*), be regarded as the third. This would make the orders, (1) *Brachiopods*; (2) *Ascidians*; (3) *Incrustates*; the first two typical, the last degradational and strikingly hemiphytoid.

4. Conclusions.

The preceding review of zoological classification appears to sustain the following general conclusions.

1. *Number and typical relations of the subdivisions of groups.*

I. The number of subkingdoms, classes, orders, and tribes, in

the system of animal life is either *four* or *three*, that is, the division in each case is either *quaternate* or *ternate*.

II. The lowest of the subdivisions in each group is a degradational or semidegradational subdivision, or *hypotypic*.

III. The quaternate division is confined to *six* cases (excepting two or three among inferior types in which there are *two* degradational subdivisions): 1, the number of subkingdoms; 2, the number of classes under Vertebrates, the highest of the subkingdoms; 3, 4, the number of orders under Mammals and Fishes, the highest and lowest classes of Vertebrates; 5, 6, the numbers of tribes under two of the orders of Mammals.

IV. In *three* only of the six cases of *quaternate* division are the three higher subdivisions all *true typical*, namely: 1, in the division of the animal kingdom into subkingdoms; 2, of the Vertebrates into classes; 3, of Mammals into orders. In the last we reach Man. As Man alone is archetypic in the class of Mammals (p. 96), so the Mammal-type is archetypic among Vertebrates, and the Vertebrate-type among the subkingdoms.

b. Below this archetypic level, in the orders of Mammals, the number of *true typical* subdivisions is but *two*—and these are the *betatypic* and *gammatypic*; for the first or alphetypic subdivision in both Megasthenes and Microsthenes, as explained on page 96, is *hypertypic*, and not true typical.

c. Again, of the *four* orders of Fishes only *one* is typical, the *two highest* being *hypertypic* (p. 96).

V. In the rest of the animal kingdom, the number of *true typical* groups, in the classes, orders, and tribes that have been reviewed, is either *two*, the *betatypic* and *gammatypic*, or *one*, the *gammatypic* alone.

2. *Lines of gradation*.—Lines of gradation between groups are lines of convergence or approximation through intermediate species. Before mentioning under this head the deductions from the preceding classification (or VIII. and IX. beyond), two general principles (VI. and VII.), having an important bearing upon them, are here introduced.

VI. The approximations between two groups usually take place, as has been frequently observed, through their *lower limits*, or most inferior species, that is, between the degradational subdivision of the inferior as well as of the superior group.—For example, plants and animals approximate only in their simplest species, the Protozoans and Protophytes; Birds and Quadrupeds most nearly in the Ornithorhynchus or Duckbill—which, at the same time that it is the lowest of Mammals, is related to a very inferior type of Birds, the Ducks; Quadrumanes and inferior Mammals through the Lemurs of the former and the Bats and

Insectivores of the Microsthenes, and not through the higher Carnivores or even any of the Megasthenes.

The classes of Reptiles and Fishes may appear to be an exception. But the *Perennibranchs* (or the species with permanent gills) among Amphibians, if referred to the type of Fishes, and especially to the Ganoid type, would rank low, as is obvious from their exsert and loosely-hung gills without gill-covers, the absence of scales, and the general inferiority in all structural arrangements. The Ganocephs, known only as fossils, and generally regarded as Perennibranch Amphibians, have, it is true, a higher grade of organisation, both as regards gills and scales, being allied in these respects to the highest of Ganoids. And this fact, in view of the above canon, sustains the opinion of Agassiz, that the Ganocephs (or Archegosauroids) are actually Ganoids—having a Reptilian feature in the partial elongation of the limbs, but in little that is fundamental in the structure beyond what belongs essentially to the Ganoid-type.

VII. The lines of gradation between classes, orders, and tribes, are only approximating, not connecting, lines, there being often wide blanks of the most fundamental character. The Ornithorhynchus, although Duck-like in some points, leaves still a very wide unfilled gap between the Mammal and Bird, and the Marsupials a still wider. The species are fundamentally Mammalian, and Bird-like only in points of secondary importance. In a similar manner, there are long blanks between the Oötocoids and higher Mammals; between Myriapods and either Insects or Spiders; between Reptiles and Mammals. The intermediate groups belong decidedly to one or the other of the two approximating groups, and are never strictly intermediate.

VIII. Under any *class*, *order*, or *tribe*, the lines of gradation run in most cases between the *degradational* subdivision and severally the *gammatypic* and *betatypic* subdivisions, and far less clearly, or not at all, between the *gammatypic* and *betatypic* themselves; that is, between D and γ , and D and β , rather than β and γ . For example, in the class of Mammals, the lines run between Oötocoids and either Megasthenes or Microsthenes, and not distinctly between Megasthenes and Microsthenes; in Insectans, between Myriapods and either Insects or Spiders, and not distinctly between Insects and Spiders; In Crustaceans, between Entomostracans and either Decapods or Tetradeapods, and not distinctly between Decapods and Tetradeapods, &c. There are exceptions to the canon; and still it is a general truth.

IX. Under any *class* or *order*, the line of gradation between the *degradational* and the *betatypic* subdivision (or D and β) is often more distinct than that between the *degradational* and

gammatypic (or D and γ), although the *gammatypic* is nearer in grade to the *degradational*.—Thus, the line between Myriapods and Insects is more distinct than that between Myriapods and Spiders; or that between Entomostracans and Decapods, than that between Entomostracans and Tetradeapods.

There is an exception in the class of Mammals: the Oötocoids seem to graduate towards both Microsthenes and Megasthenes with nearly equal distinctness.

3. Co-ordinate grades and distinctions in Classification.

X. The co-ordinate value of subdivisions in the system of classification is brought out to view in the parallel columns of the preceding tables, and evidence is thence afforded as to what groups are rightly designated classes, orders, &c.

a. We thus learn that the subdivisions of the class of Mammals—Man, Megasthenes, Microsthenes—are properly *orders*, if we so call the subdivisions Decapods and Tetradeapods under Crustaceans, or Insects and Spiders under Insecteans.

b. Again, we have a solution of the question whether in each of the classes, Mammals, Birds, and Reptiles, the *hemitypic* division, as so-called on page 76, is a *subclass* co-ordinate with the *typical* division of the same, or whether it is an order co-ordinate with the three higher subdivisions of the class. The question appears to be decided (contrary to former views of the writer), that it is correctly made an *order*. These *hemitypic* divisions actually correspond severally to the *degradational* division in other columns of the different tables; and, therefore, if in the case of other classes as those of Crustaceans, Insecteans, &c., they are *orders*, so are they in the three classes of Vertebrates mentioned. They have also a relation to the *hemitypic* divisions among Fishes, which are the first and second *orders* of the class.

XI. In an *inferior* or *degradational* group, the distinctions of the subdivisions included are generally much more strongly and obviously exhibited in the structure than among *typical* groups. Thus, the orders of Fishes are based on characters that have nearly a class-value among the higher Vertebrates. In the same manner, Amphibians, or *hemitypic* Reptiles, differ from true Reptiles more obviously than Oötocoids, or *hemitypic* Mammals, differ from other Mammals. So, the distinctions among the groups of Crustaceans are very wide compared with those among Insects; and those among *degradational* Crustaceans far wider than those among the *typical* subdivisions. The relative force of the life-systems is, in all probability, as great between Oötocoids and *typical* Mammals as between Amphibians and *typical* Reptiles, although so unequally expressed in the structure of the high or concentrated groups and the low or lax groups of species. Over-

looking this principle has often led authors to allow too great importance to the structural differences among inferior or degradational groups.

XII. Under any class, order, tribe, the *typical* groups are often represented more or less clearly among the subdivisions of the *degradational*. Hence characteristics which separate the typical groups frequently separate only subordinate divisions under an inferior or degradational group. Examples occur in the class of Fishes under Vertebrates, in whose subdivisions the other classes of Vertebrates are partly represented; in the order Oötocoids under Mammals, which has its megasthenic and microsthenic subdivisions; under Worms, &c.

4. *Distinction between Animals and Plants.*

XIII. This subject well illustrates a fundamental distinction between animals and plants.

a. An animal, as has been stated on page 94, has *fore-and-aft*, or antero-posterior, polarity; that is, it has a fore-extremity and a hind-extremity which have that degree of oppositeness that characterizes polarity.

b. With this fore-and-aft polarity there is also *dorso-ventral* polarity.

c. The dorso-ventral and antero-posterior axes are at *right angles* to one another. In Invertebrates and a large part of Vertebrates the antero-posterior axis is horizontal and the dorso-ventral vertical; and only in Man, the prince of Mammals, is the former vertical and the latter horizontal.

d. An animal, again, has not only oppositeness between the fore-extremity and hind-extremity, but also a *head*, the seat of the senses and mouth, situated at the fore-extremity and constituting this extremity.

e. In addition, the typical animal is *forward moving*.

But in animals of the inferior type of *Radiates*, while there is an anterior and a posterior side, and also, in most species, forward motion, the mouth-aperture—which indicates the *primary centre* in an animal (p. 82)—is not placed at one extremity, but is more or less nearly *central*; and almost precisely central in the symmetrical (and therefore inferior) *Radiates*. The mouth-extremity and the opposite are at the *poles of the dorso-ventral axis*, and not at those of the antero-posterior; that is, they are at the extremity of the axis which in the inferior animals is normally *vertical*. This is true even in a *Holothuria*, the mouth of which is not at the *anterior* extremity, but is central, or nearly so, as in an *Echinus*. A *Limulus* has been referred to on page 90 as showing an approximation, under the true animal type, to this same central position of the mouth.

We pass now to *Plants*. The plant, in contrast with the fore-and-aft animal, is an *up-and-down* structure, having up-and-down polarity. The axis is *vertical* like the dorso-ventral in the lower animals, to which it is strictly analogous, as is shown from a comparison with Radiates,—Radiates and Plants being alike in type of structure. The primary centre of force is central, in the same sense, in the regular flower and the symmetrical Radiate.

Thus, the structures under the animal-type and plant-type are based on two distinct axial directions, one at right angles to the other: in the *animal-type* the antero-posterior axis being the dominant one, while the two co-exist; and in the *plant-type* the axis at right angles to this being the only one.

In the above way (as well as in its non-percipient nature), the plant exhibits complete decephalization—a condition to which the Radiate only approximates, as it has generally, if not always, an anterior and posterior side, besides other animal characteristics.

Synopsis of Canadian Ferns and Filicoid Plants. By
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Scotia.

(Continued from the January Number.)

SCOLOPENDRIUM.

S. vulgare, Smith.—Fronds (in tufts) strap-shaped, with a cordate base, undivided, margin entire, stipe scaly. *Scolopendrium vulgare*, J. E. Smith, Bab., J. Sm., Moore, &c. *S. officinarum*, Swartz, Schkr., Gray Man., p. 593; Torr. Fl. N. Y. ii. p. 490. *S. Phyllitis*, Roth. *S. officinale*, DC. *S. Lingua*, Cavanilles. *Asplenium Scolopendrium*, Linn. Sp. Plantarum, &c. *A. elongatum*, Salisb. *Blechnum linguifolium*, Stokes. *Phyllitis Scolopendrium*, Newman.—Owen Sound, Georgian Bay, Lake Huron, on soft springy ground, amongst large stones, growing in tufts, abundant, 1861, Robert Bell, junior, C.E. This interesting addition to our list of Canadian ferns has been collected in the same place by the Rev. Prof. William Hincks, F.L.S. Mr Bell's specimens agree, in every respect, with the typical European form of the species, which is exceedingly variable. Only one station was previously known for this fern in all North America, viz., limestone rocks along Chittenango Creek, near the Falls, respecting which Professor Torrey observed:—"This fern is undoubtedly indigenous in the locality here given, which is the only place where it has hitherto been found in North America." It was first detected by Pursh, who found it in shady woods, among loose rocks in the western parts of New York, near Onondago, on the plantations of J. Geddis, Esq. This species (he said) I have seen in no other place but that here mentioned, neither have I had any information of its having

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been found in any other part of North America. (Pursh.) Nuttall states that he found it in the western part of the state, without giving the locality; but according to Dr Pickering, the specimens of Mr Nuttall, in the herbarium of the Academy of Sciences in Philadelphia, are marked, "Near Canandaigua, at Geddis's farm, in a shady wood, with *Tarax canadensis*," Torrey Fl. N. Y. ii. p. 490. This fern occurs throughout Europe, and also in Northern Asia. Mr Moore considers the Mexican *S. Lindeni* as a mere variety of this species. In Europe there are many remarkable varieties, of which Mr Moore has figured and described more than fifty that occur in Britain. The great beauty and remarkable character of many of these render them very suitable for cultivation. None of the abnormal forms have as yet been found in America, probably merely because they have not been looked for.

CAMPTOSORUS.

C. rhizophyllus, Presl.—Fronde lanceolate, broad and hastate, or cordate at base, attenuated towards the tip, which strikes root and gives rise to a new plant; hence this fern is called the Walking Leaf; fronds evergreen. *Camptosorus rhizophyllus*, Link, Presl, A. Gray, Eaton, Hooker. *Asplenium rhizophyllum*, Linn. in part (Linnaeus's name included *Fadyenia prolifera*, a totally different plant), Michaux, Pursh Fl. Am. Sept. ii. p. 666, Bigelow, Torrey, Beck, Darlington, Lowe's Ferns, vol. v. pl. 14 a. *Antigramma rhizophylla*, J. Sm., Torrey Fl. N. Y. ii. p. 494. *Camptosorus rumicifolius*, Link.—On the flat perpendicular face of a rock in the woods, on the Spike's Corners side of the mills at High Falls, township of Portland, C.W., July 1862. In a rocky wood, a mile north-west from the Oxford station of the Ottawa and Prescott Railway, upon a rock slightly covered with mould, B. Billings, jr.; mountain side west from Hamilton, also at Ancaster and at Lake Medad, Judge Logie; Wolfe Island, E. J. Fox; not rare about Owen Sound, Rev. Prof. W. Hincks, F.L.S.; Montreal Mountain, M. L'Abbé Provancher; rather northern in its range in North America, but not common anywhere in Canada. This curious fern has been long in cultivation in the botanic gardens of Europe.

LASTREA.

L. dilatata, Presl.—Fronde spreading, broadly lanceolate, rather pale but vivid green, bipinnate; the pinnules pinnate or pinnatifid with pointed lobes; on the lower pinnæ, the posterior pinnules are longer than the anterior ones; stipe with rather distant pale unicolorous scales; sori small. This description refers only to the commonest form in Canada. It is a very variable species. *Aspidium spinulosum*, Gray.—Abundant in the woods about Kingston, as Collins's Bay, &c., Smith's Falls, Odessa, woods near the Falls of Niagara, Hinchinbrook, Gananoque Lakes, Farmersville, Hardwood Creek, Delta, Upper Rideau Lake, Newboro-on-the-Rideau, Longpoint; Mouth of the Awaganissis Brook, Gulf of St Lawrence, Goulais River, also Grand Island, and at Keewenaw Point, Lake Superior, R. Bell, jr.; Ramsay, Rev. J. K. M'Morine, M.A.; Prescott, very common, B. Billings, jr.; St John's, St Valentine,

and Beloeil, P. W. MacLagan, M.D.; Belleville, very common, J. Macoun; St Joy Woods, W. S. M. D'Urban; Daniel's Harbour, Newfoundland, James Richardson (a peculiar form); Pêche River, Chelsea and Cantley, Hull, D. M'Gillivray, M.D. Of varieties referable to var. *Boottii*, Gray, var. *dumetorum*, Gray, or others, differing from the common (which, however, is perhaps not the typical) form, I have seen specimens from, or obtained information of their having been collected in, the following localities:—Malden, Brighton, Point Rich, Newfoundland, Hamilton's Farm, Murray, Hamilton, &c. These varieties still require careful study, with a view to their identification with European forms, which are now well understood.

β. tanacetifolia.—Frond large and very broad, triangular, tripinnate, with the pinnules pinnatifid or deeply incised, lobed. *P. tanacetifolium*, DC. ?—Pointe des Morts, Gaspé, John Bell, B.A. Mr Bell's specimen seems to agree well with Mr Moore's description of var. *tanacetifolia*. The typical *L. dilatata*, with dark-centred scales, so common in Scotland, I have not yet seen growing in the Canadian woods; but a fragment, the upper portion of a frond, from Point Rich, Newfoundland, James Richardson, looks like it.

L. marginalis, J. Smith.—Frond ovate-oblong, a foot, more or less, in length, bipinnate, pale green, somewhat coriaceous, lasting the winter; pinnae linear-lanceolate, broad at base; pinnules oblong, very obtuse, obsoletely incised; sori marginal; stipe of a pale cinnamon colour when old, with large thin pale scales profuse below. *L. marginalis*, J. Sm., *Aspidium marginale*, Swartz, Pursh, Bigelow, Beck, Darlington, Gray, Eaton, Lowe's Ferns, vol. vi., pl. 6 (a bad figure), Torrey Fl. N. Y. ii. p. 495. *Polypodium marginale*, Linn. *Nephrodium marginale*, Michaux.—This species is as common in the Canadian woods as *Lastrea Filix-mas* is in those of Britain; woods around Kingston, abundant; near Odessa; Newboro-on-the Rideau; along the course of the Gananoque River and lakes, in various places; very fine at Marble Rock; Farmersville; Hardwood Creek; Valley of the Trent, found on the great boulder, &c.; on Judge Malloch's farm and elsewhere about Brockville; on limestone rocks above the Rapids at Shaw's Mill, Lakefield, North Douro, Mrs Traill; Sulphur Spring, Hamilton, Judge Logie; Cedar Island, A. T. Drummond, jr., B.A.; Smith's Falls, and Chippawa, P. W. MacLagan, M.D.; Ramsay, Rev. J. K. M'Morine, M.A.; Prescott, common, B. Billings, jr.; Belleville, in rich low moist woods, common, J. Macoun; above Blacklead Falls, W. S. M. D'Urban; Gatineau Mills, D. M'Gillivray, M.D.; Cap Tourmente, M. L'Abbé Provancher; Harrington, J. Bell, B.A.; London, W. Saunders. This is exclusively an American fern. It varies in size and appearance; in some specimens the pinnae are wide apart, their divisions small and narrow; in others, the pinnae overlap each other, and their divisions are broad and leafy, also overlapping, and in such forms they are usually toothed into rounded lobes. Mr Macoun sends a form from Belleville, more deeply serrate than usual.

β. Traillae.—Fronde very large (3½ feet long), bipinnate, all the pinnules pinnatifid.—Lakefield, North Douro, Mrs Traill. This is a very handsome variety, and would form an attractive plant in cultivation.

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It has the same relation to the type of *L. marginalis* which *incisa* (*erosa*) has to typical *Filix-mas*.

Lastrea Filix-mas is erroneously referred to in some American works on *Materia Medica* as a common North American and Canadian fern. It has recently, however, been found on the Rocky Mountains by Dr Parry. Professor Gray says that Dr Parry's specimens are apparently identical with the European plant. Nothing like it occurs in Canada, so far as I can ascertain. Varieties of *L. marginalis* have been sent to me under the name of *Filix-mas*.

L. cristata, Presl.—Fronds erect, rigid, linear-oblong in outline, vivid green, pinnate or slightly bipinnate; pinnae triangular-lanceolate; pinnules large, oblong, approximate, decurrent; sori large, in a single series on each side of, and near to, the vein; stipe with few pale scales. *Lastrea cristata*, Presl, Moore, &c. *Polypodium cristatum*, Linn. *Aspidium cristatum*, Swartz, Willd., Pursh, E. B., Beck, Torrey Fl. N. Y., ii. p. 496, Gray. *Aspidium cristatum*, *β. lancastriense*, Torrey; *A. lancastriense*, Spreng., Bigelow, Beck, Darlington, Hooker.—Woods around Kingston; near the Pêche River, Gatineau, a tributary of the Ottawa, D. M'Gillivray, M.D.; Three Rivers, St John's, and Chippawa, P. W. MacLagan, M.D.; Sproule's Swamp, east from Belleville (a cedar swamp), not common, J. Macoun; Ramsay, Rev. J. K. M'Morine, M.A.; Prescott, common, B. Billings, jr.; Lake of Three Mountains, W. S. M. D'Urban; Silver Brook, Gaspé, John Bell, B.A.; St Ferreol, M. l'Abbé Provancher; L'Orignal, J. Bell; London, W. Saunders.

L. Goldieana, J. Smith.—Frond very large (3 or 4 feet or more in length), dark green, bipinnate; pinnae 6 to 8 inches long, narrow, linear-lanceolate, not much attenuated towards the tips; pinnules (12–20 pairs), linear-oblong, approximate, uniformly curved forwards, scythe-shaped, sometimes with an extra lobe at base; sori small, near the midrib; stipe with pale shaggy scales above and larger dark-centred ones below; our largest Canadian fern, usually barren. *Lastrea Goldieana*, J. Smith. *Aspidium Goldieanum*, Hooker, Edin. New Phil. Jour. vi. p. 333, and Fl. Bor. Am., ii. p. 260, Gray. *Nephrodium Goldieanum*, Hook. and Grev. *Aspidium Filix-mas*, Pursh, not of Willd., &c.—Farmersville, in woods near the village, abundant and very fine, forming immense tufts; near Hamilton's Farm and De Salaberry, town line, W. S. M. D'Urban; Beloeil Mountain, Montreal and Malden, P. W. MacLagan, M.D.; Belleville Woods, near Castleton; woods below Heely's Falls, west side, and in Simon Terrill's Woods, Brighton, J. Macoun. Augusta, Robert Jardine, B.A.; about Montreal, Mr Goldie in Hook. Fl. Bor. Am. London, W. Saunders. This fine fern was appropriately named by Sir William Hooker in honour of its discoverer, a successful investigator of Canadian botany, now resident at Paris, C.W. The species belongs exclusively to the American Continent. In Canada we have two sub-varieties:—

α. serrata, in which the divisions of the pinnae are coarsely serrate. Montreal.

β. integerrima, in which the divisions of the pinnae are almost or quite entire. Farmersville.

L. fragrans, Moore.—Frond 8 to 12 inches long, coriaceous, bipinnate,

pinnæ triangular, of few (4 or 5 pairs) of pinnales, which are crowded and covered beneath by the large rusty membranous indusia, which conceal the sori. Rachis with profuse, large, palish scales, especially near the base. *Aspidium fragrans*, Swartz, A. Gray.—Rocks, Penokee Iron Ridge, Lake Superior, Mr Lapham, and north-west—Professor Woods, in Class-Book; shaded trap rocks, Falls of the St Croix, Wisconsin, Dr Parry, and high northward, Gray's Manual. I have not yet seen Canadian specimens of this species, which is quite a northern fern, stretching along the northern shores of the Pacific to the Russian Arctic dominions. I have specimens from Repulse Bay, collected by Captain Rae's party while wintering there in 1855. This plant does not appear to be in cultivation in any European garden.

L. Thelypteris, Presl.—Frond erect, lanceolate, mostly broad at base, and narrowed upwards, thin, and herbaceous, or slightly coriaceous, glabrous or downy, pinnate; pinnæ linear, rather distant, deeply pinnatifid; pinnales with revolute margins, veins forked, sori near their middle, becoming confluent. Stipe as long as, or longer than, the frond, and naked. *Lastrea Thelypteris*, Presl, Moore, J. Sm. *Aspidium Thelypteris*, Swartz, E. B. Willd., Pursh, Bigelow, Beck, Darlington, Torrey Fl. N. Y. ii. p. 496, A. Gray, Man. *Polypodium Thelypteris*, Linn. *Dryopteris Thelypteris*, A. Gr.—Swamps in the woods, Townships of Hinchinbrook, Portland, Ernestown, &c.; Millgrove Marsh, Hamilton, Judge Logie; Gatineau Mills on the Ottawa, D. M'Gillivray, M.D.; Prescott, common, B. Billing, jr.; Temiscouata, Thorold and Malden, P. W. MacLagan, M.D.; Belleville, very common in swamps, J. Macoun; Ramsay, Rev. J. K. M'Morine, M.A.; portage to Bark Lake, and on lumber road through the woods east from Hamilton's Farm, W. S. M. D'Urban; Montreal, Drs MacLagan and Epstein; Hudson's Bay Territories near Red River Settlement, Governor M'Tavish; St Joachim, M. L'Abbé Provancher; L'Orignal, J. Bell, B.A.; London, N. Saunders. In the State of New York this species is common in swamps and wet thickets (Torrey). I have it from West Point, N. Y. In the south, Eaton indicates Florida and northward. Very seldom found with fructification (Pursh). Fertile specimens are not rare with us. The forked veins of the pinnales distinguish this species from the next. In the Canadian plant the outline of the frond is a little different from Scotch and Irish specimens, being less narrowed at base. There are three forms of this species in Canada. The first (α) seems to be the plant of Gray's Manual, the second (β) is more like the *L. Thelypteris* of Europe, and the third (γ) is intermediate between this species and the next.

a. pubescens.—Frond somewhat coriaceous, densely pubescent or downy throughout. Odessa, Hudson's Bay, &c.

β . glabra.—Frond thin, herbaceous, glabrous. Montreal, Chelsea, Hinchinbrook, &c.

γ . intermedia.—Frond narrowed below, glabrous; stipe slightly elongated (veins forked). Gaspé, J. Bell, B.A.

L. Nov-Eboracensis.—Frond lanceolate, narrow at the base, thin and herbaceous, pinnate; pinnæ linear or linear-lanceolate, more or less approximate, deeply pinnatifid; pinnales oblong, usually flat; veins



simple (not forked); sori never confluent; stipe short, rachis, &c. downy, pinnules more or less distinctly ciliate. *Lastrea Novboracensis*, Presl; *Polypodium Novboracense*, Linn., Schk. *Aspidium thelypteroides*, Swartz. *Aspidium Novboracense*, Willd., A. Gray, Eaton—Pittsburg near Kingston; Lakefield, North Douro, Mrs Traill; Mountain side, Hamilton, Judge Logie; Prescott, common, B. Billings, jr.; Mount Johnson, Montreal, and Beloeil, P. W. MacLagan, M.D.; Ramsay, Rev. J. K. M'Morine, M.A.; near Chelsea, D. M'Gillivray, M.D.; London, but not common, W. Saunders; L'Original, J. Bell. This fern belongs exclusively to the American Continent. It seems to be more abundant and more distinct in the United States than with us. In *Flora Boreali-Americana*, Sir William Hooker observed—"The *Aspidium Novboracense* is quite identical with *A. Thelypteris*." In the recently published volume of *Species Filicum* (which at present I can only quote at second hand), doubts are still expressed as to its being a species really distinct from *L. Thelypteris*. Mr Eaton and other American pteridologists think it quite distinct. Its most obvious characters are—(1.) The tapering form of the lower part of the frond (although there is also a form of *L. Thelypteris* having this peculiarity; (2.) sori few, mostly near the base of the pinnules, and not confluent, not overlapped by a recurved margin; (3.) veins of the pinnules simple, not forked. The outline of the frond must not be depended upon, as the Scotch and Irish *L. Thelypteris* is narrowed at the base like *L. Nov-Eboracensis*. This species is allied to *L. montana*, Moore (*Oreopteris*, Bory).

POLYSTICHUM.

P. angulare, β . *Braunii*.—Frond soft, herbaceous, lanceolate, bipinnate; pinnules stalked, serrate; the small teeth tipped by soft bristles; stipe and rachis scaly throughout. In the Canadian plant the scales of the rachis are larger than in the typical *P. angulare* of England, from which it may be specifically distinct. *Aspidium Braunii*, Spenner. *Aspidium aculeatum* var. *Braunii*, A. Gray, Man. Bot., p. 599, *A. aculeatum*, Provancher; Harrington, Cap Bon Ami and Dartmouth, N. fork, Gaspé, John Bell, B.A.; base of Silver Mountain, W. S. M. D'Urban.

P. Lonchitis, Roth.—Frond rigid and shining, linear-lanceolate, simply pinnate; pinnæ scythe-shaped, auricled, spinose. *Polystichum Lonchitis*, Roth, Moore, J. Sm., &c. *Polypodium Lonchitis*, Linn. *Aspidium Lonchitis*, Swartz, Schk.—Limestone rocks, Owen Sound, C.W., 1859, Rev. Professor William Hincks, F.L.S. Professor Hincks has kindly furnished me with specimens from the above locality. Woods, southern shore of Lake Superior and north-westward, Professor Asa Gray, in Man. Bot., N.S.; British America, Professor Woods in Class-Book. It will be observed that Professor Hincks's station is the only definite Canadian one with which we are acquainted. Mr T. Drummond found this fern on the Rocky Mountains many years ago.

P. acrostichoides, Schott.—Frond pale green, shining, long and narrow, linear-lanceolate, simply pinnate; pinnæ long and narrow, linear-lanceolate, shortly stalked, auricled anteriorly at the base, more or less

distinctly serrate, with hair-tipped teeth; fertile (upper) pinnae slightly contracted, covered beneath by the large confluent sori; stipe profusely chaffy, with pale scales. *Polystichum acrostichoides*, Schott, J. Sm. *Aspidium acrostichoides*, Swartz, A. Gray, Eaton. *Aspid. auriculatum*, Schk. *Nephrodium acrostichoides*, Michx.—Abundant in the woods a few miles west from Kingston; also not rare in the woods of the Midland District of Canada generally; Upper Rideau Lake; woods around Toronto, Rev. Dr Barclay; Stanfold, M. L'Abbé Provancher; L'Orignal, J. Bell; London, W. Saunders: Sulphur Spring, Hamilton, Judge Logie; Prescott, common, B. Billings, jr.; Nicolet and St Valentine, C.E., and Chippawa, C.W., P. W. MacLagan, M.D.; Belleville, very common in rocky woods, as in Hop Garden, J. Macoun; Ramsay, Rev. J. K. M'Morine, M.A.; hills and woods, portage to Bark Lake, W. S. M. D'Urban; Gilmour's Farm, Chelsea, D. M'Gillivray, M.D.; Osnabruck and Prescott Junction, Rev. E. M. Epstein. This species is exclusively American.

[*β. incisum*; pinnae strongly serrate or incised into lobes. *Aspidium Schweinitzii*, Beck. This form, which I have from Schooley's Mountains, &c. (A. O. Brodie), will no doubt be found in Canada.]

CYSTOPTERIS.

C. fragilis, Bernhardt.—Fronds delicate, green, lanceolate in outline, glabrous, bipinnate; pinnae and pinnules ovate-lanceolate or oblong; the latter obtuse, incisely toothed, thin and veiny; sori large; stipe dark purple at the base. *Cystopteris fragilis*, Bernhardt, Hook., Bab., Moore, Newm., A. Gray. *Polypodium fragile*, Linn. *Cystopteris orientalis*, Desvaux. *Polypod. viridulum*, Desv. *Athyrium fragile*, Sadler. *Cyathea fragilis*, Sm. *C. cynapifolia* and *C. anthriscifolia*, Roth. *Cystea fragilis*, Sm. *Cyclopteris fragilis*, S. F. Gray.—Rocky woods and cliffs about Kingston, in various places, but not abundant; Farmersville; Mountain side, Hamilton, on moist rocks, Judge Logie; rocks by the bay shore, L'Anse au Cousin, and Dartmouth River, Gaspé, John Bell, B.A.; Mirwin's woods, Prescott, common, B. Billings, jr.; Montreal and Jones's Falls, P. W. MacLagan, M.D.; rocky banks of the Moira, rather rare, J. Macoun; Ramsay, Rev. J. K. M'Morine, M.A.; camp at base of Silver Mount, on rocks, also River Rouge, abundant, De Salaberry, west line, and at Black Lead Falls, W. S. M. D'Urban; St Joachim, M. L'Abbé Provancher; Grenville, C. E., John Bell, B.A.; London, W. Saunders. In Dr Hooker's valuable Table of Arctic Distribution this plant is indicated as a Canadian species that does not enter the United States, which I presume arises from a misprint, as the species is not uncommon in the Northern States, and extends south to the Mountains of Carolina. The delicate *C. tenuis* is the form known in the south, but in Canada we have the stout typical European form of *C. fragilis*.

β. angustata.—Pinnules incised, with longish and spreading teeth. *Cyst. frag. var. cynapifolia*, J. Lowe.—Gaspé, John Bell, B.A. Specimens referable to this form were likewise gathered at Lake of Three Mountains by Mr D'Urban. Mr Bell's specimens agree perfectly with English specimens from Dr John Lowe (*C. f. cynapifolia*). Italian specimens from Professor Caruel of Pisa, labelled "*Cyst. fragilis*," belong to this

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variety. Mr Bell has a fertile frond from Gaspé with very broad veiny pinnæ, deeply incised, but not pinnate.

C. bulbifera, Bernhardt.—Frond thin, green, lanceolate or linear-lanceolate, bipinnate, bulbiferous towards the apex on the under surface; pinnæ oblong-lanceolate, narrowed at the tips; pinnules oblong-obtuse, incisely toothed; sori small, not very numerous; indusium short. Very variable in the size and form of the frond. *C. bulbifera*, Bernhardt, A. Gray, J. Sm. *Aspidium bulbiferum*, Swartz, Schk., Pursh. *Aspidium atomarium*, Muhl.—Moist swampy woods about Kingston, as Collins's Bay, Kingston Mills, &c.; abundant on Judge Malloch's farm, a mile west from Brockville; Petit Portage, &c., Gaspé, John Bell, B.A.; Wolfe Island, A. T. Drummond, B.A.; Mirwin's woods, Prescott, common, B. Billings, jr. (short form); Beloeil Mountain, P. W. MacLagan, M.D.; rocky banks of the Moira, Belleville, and in cedar swamps and wet woods, very common, J. Macoun; Ramsay, Rev. J. K. M'Morine, M.A.; Mountain side, Hamilton, common, Judge Logie; Black Lead Falls, on limestone rock, W. S. M. D'Urban; Pied du cap Tourmente, M. L'Abbé Provancher; Grenville, C. E., J. Bell; London, W. Saunders. There are two distinct forms or varieties of this species.

a. horizontalis.—Frond triangular-lanceolate, broad at base, not more than three or four times longer than broad; pinnæ horizontal. Niagara Falls, within the spray, Collins's Bay, &c.

β flagelliformis.—Frond linear, attenuated upwards, very long and narrow, six or seven times longer than broad; pinnæ less horizontal. Frankville, Montreal, Gaspé, &c.

DENNSTÆDTIA.

D. punctilobula, Moore.—Frond broadly lanceolate, pale green, thin, with a stout rachis, bipinnate; the pinnules pinnatifid; sori minute, usually one on the anterior basal tooth of each lobe of the pinnule, which is reflexed over the sorus; the proper indusium is pale, cup-shaped, opening at top. Rhizome slender, creeping through the soil; whole plant glandular-downy. *Dennstædtia* (Bernhardt, 1800) *punctilobula*, Moore, Index Filicum, p. xcvi. *Dicksonia punctilobula*, Hooker, A. Gray, J. Sm. *D. pilosiuscula*, Willd., Hook. Fl. Bor. Amer. *Nephrodium punctilobulum*, Michx. *Aspidium punctilobulum*, Swartz. *Patania*, Presl. *Dicksonia pubescens*, Schkr. *Si'olobium pilosiusculum*, Desv., J. Sm. Gen. Fil.—Pittsburg near Kingston, John Bell, B.A.; River Rouge, W. S. M. D'Urban; Montreal, P. W. MacLagan, M.D.; Prescott, on Dr Jessup's moist pasture land, B. Billings, jr.; New Brunswick, E. N. Kendal, in Hook. Fl. Bor. Amer.; Ramsay, Rev. J. K. M'Morine. Mr Eaton has mentioned to me that the drying fronds have the odour of new hay.

WOODSIA.

W. Ilvensis, R. Br.—Frond lanceolate, usually 4 or 5 inches long, bipinnate, or nearly so, pinnæ approximate, pinnules oblong, obtuse, stipe (red), rachis, and whole lower surface of the frond clothed with chaffy scales, which are rusty at maturity. Sori usually confluent

around the margins of the pinnules. First observed in the Isle of Elba (Ilva), hence named, after Dalechamp, *Acrostichum Ilvense*. by Linnaeus, whose Phoenix was very wroth thereat; see English Flora, vol. iv. p. 323. *Woodsia Ilvensis*, R. Br., Hook., Moore, J. Sm., Gray, &c. *Nephrodium lanosum*, Michx.—Abundant on the ridge of Laurentian rocks at Kingston Mills; Rocks west from Brockville and at Chelsea, B. Billings, jr.; Mount Johnson and Beloeil Mountain, P. W. MacLagan, M. D.; mountain gneiss rocks, opposite Rouge River, W. S. M. D'Urban. I have likewise specimens from the Hudson's Bay territories (Governor M'Tavish), but without special locality. On rocks, Canada, Pursh; Canada to Hudson's Bay, Hook. Fl. B. A.; Pied du cap Tourmente, M. L'Abbé Provancher. I think our plant must be much larger and more scaly than the European one. A tuft which I have from Catskill Mountains (A. O. Brodie) has richly fruited fronds a foot long and 2 inches wide. (I find that large American forms of this species have been mistaken for *W. obtusa*. The involucre, which is large and not split into hairs in the latter species, serves readily to distinguish it.) Much of the *Ilvensis* in cultivation in Europe is probably the American form.

β. gracilis.—Frond more slender, more hairy and less scaly than the type; pinnæ rather distant, deeply pinnatifid, or partially pinnate. Dartmouth River, Gaspé, John Bell, B.A. In technical characters, this form agrees better with *W. alpina* (*hyperborea*), but it has quite a different aspect.

W. alpina, S. F. Gray.—Frond small (from 1 to 2 or 3 inches long), broadly linear, pinnate, somewhat hairy without distinct scales; pinnæ ovate, somewhat triangular, obtuse, pinnatifidly divided into roundish lobes. *Woodsia alpina*, S. F. Gray, Brit. Pl., Moore. *Woodsia hyperborea*, R. Br. in Linn. Trans., vol. xi.; Pursh. Fl. Am. Sept. ii. p. 660.—In the clefts of rocks, Canada, Pursh; Canada to the Saskatchewan, Hooker. Noticed in Dr Hooker's Table of Arctic Plants as a Canadian species that does not extend into the American States.

W. glabella, R. Br.—Frond a few (2-4) inches long, linear, bright-green and glabrous on both sides, simply pinnate; the pinnæ short, rounded or rhombic, cut into rounded or wedged lobes. Stipe with a few scales at the base only. *Woodsia glabella*, R. Br., Hook. Fl. Borcali Americana, tab. 237; Gray. Canada, Professor Woods in Cl. Bk. Sir W. Hooker, in the Fl. B. Amer., gave Great Bear Lake as the only station then known for *W. glabella*. Mr D. C. Eaton has kindly furnished me with specimens from Willoughby Lake, Vermont (Goodale leg.), and Professor Gray notices its occurrence on rocks at Little Falls, New York (Vasey), and "high northward."

β. Belli.—Frond larger (6-7 inches long); pinnæ more elongated, pinnatifidly incised into rounded lobes (bright green, glabrous). Gaspé, on the Dartmouth River, twenty miles from its mouth, John Bell, B.A.

W. obtusa, Torrey.—Frond nearly a foot long, linear-lanceolate, glandulose, bipinnate; pinnules slightly decurrent, oblong, obtuse, crenate, or somewhat pinnatifid; indusium large, enveloping the sorus, torn into a few marginal lobes; stipe with few scattered, pale, chaffy scales. *Woodsia obtusa*, Torrey; A. Gray, J. Sm. *Aspidium obtusum*, Willd. *Physematum obtusum*, Hook. Fl. Bor. Am. *Woodsia Perriniana*,

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Hook. and Grev. Ic. Fil. *Polypodium obtusum*, Swartz.—An impression prevails that this plant, which is said to be common in the Northern States, especially towards the west, grows also in Canada. Mr D. C. Eaton, in the kindest manner, cut out of his own herbarium a specimen for me, from near High Bridge, New York city, in an excellent state for examination, which has enabled me to understand the species and to ascertain that we have as yet no satisfactory evidence of its occurrence in Canada. Large forms of *W. Ilvensis* have in some cases passed for it. (I introduce this notice of the plant with a view to promote farther inquiry.)

OSMUNDA.

O. regalis β . *spectabilis*.—Fronds erect, pale-green, glabrous, bipinnate; pinnules oblong-lanceolate, oblique, shortly stalked, very slightly dilated at the base, nearly entire; fertile pinnules forming a racemose panicle at the summit of the frond. *Osmunda spectabilis*, Willd., J. Smith. Farmersville; Hardwood Creek. Hinchinbrook, and other places in rear of Kingston, usually in thickety swamps, by corduroy roads, &c.; Millgrove Marsh, Hamilton, Judge Logie; Ramsay, Rev. J. K. McMorine, M.A.; woods near the Hop Garden, Belleville, not common, J. Macoun; Prescott, common, B. Billings, jr.; around Metis Lake, &c.; opposite Gros Cap; also Sou-sou-wa-ga-mi Creek and Schibwah River, R. Bell, jr.; near Montreal, Rev. E. M. Epstein and W. S. M. D'Urban; mountain, Bonne Bay, Newfoundland, on rocks 1000 feet above the sea, James Richardson (a small form); Welland, J. A. Kemp, M.D.; Osnabruck and Prescott Junction, Rev. E. M. Epstein; Nicolet, Wolfe Island and Navy Island, P. W. MacLagan, M.D.; Lake St Charles, M. L'Abbé Provancher; Caledonia Springs and L'Orignal, J. Bell; Portland, Thos. R. Dupuis, M.D.; Bedford; London, W. Saunders. The fronds of our plant are a little more drawn out than those of the European one; the pinnules are often distinctly stalked, and the overlapping auricles either altogether absent or only slightly developed. This is *O. spectabilis*, Willd.; *O. regalis*, β . Linn. Sp. Pl. Some botanists distinguish two American forms, one agreeing with the typical *regalis* of Europe; but it is difficult to do so. The typical *O. regalis* is a larger, more robust, and more leafy plant, with more widely spreading or divergent pinnae, and more leafy auricled sessile pinnules, more or less pinnatifid at the base; in our Canadian plant they are quite entire. The divisions of the fertile portion of the pinnae are also more widely divergent in a *regalis*. The frond, moreover, is of a darker colour.

O. cinnamomea, Linn.—Sterile and fertile fronds distinct, the former ample, broadly lanceolate, pinnate; the pinnae rather deeply pinnatifid; lobes regular, entire; fertile frond contracted, erect, in the centre of the tuft of sterile fronds, and not at all foliaceous. Sporangia ferruginous. Fertile frond decaying early in the summer. *Osmunda cinnamomea*, Linn., Gray, J. Sm. *O. Claytoniana*, Conrad, not of Linn.—Fairfield farm and elsewhere about Kingston, not uncommon; Millgrove Marsh, Hamilton, Judge Logie; Sandwich and Montreal, P. W. MacLagan, M.D.; opposite Gros Cap; also Two Heart River, Lake Superior, R. Bell, jr., C. E.; Belleville, swamps and low grounds, common, J. Macoun; Ramsay,

Rev. J. K. M'Morine, M.A.; St Joy Woods, on the river shore, near Gatineau Mills, D. M'Gillivray, M.D.; Newfoundland, Miss Brenton, in Hook. Fl. Bor. Am.; Prescott, common, B. Billings, jr.; Nicolet, M. L'Abbé Provancher; L'Original, J. Bell; near London, W. Saunders.

O. Claytoniana, Linn.—Frond narrowly lanceolate, pinnate; pinnæ lanceolate, about three pairs of pinnæ near or below the middle of the frond contracted and fertile; sporangia brown, with green spores. This species, when fresh, has a strong odour, resembling that of rhubarb (Pie-plant) stalks. *O. Claytoniana*, Linn., Gray, J. Sm. *O. interrupta*, Michaux.—Between Kingston and Kingston Mills, in wet swampy places by the roadside; Little Cataraqui Creek; Waterloo; banks of the Humber, near Toronto; Princes Island, Hamilton, Judge Logie; Ramsay, Rev. J. K. M'Morine, M.A.; Ke-we-naw Point, in wet soil, R. Bell, jr.; Belleville, low rich grounds, not rare, J. Macoun; Prescott, common, B. Billings, jr.; Round Lake, W. S. M. D'Urban; Lake Settlement, and on the river shore near Gatineau Mills, D. M'Gillivray, M.D.; Newfoundland, Miss Brenton, in Hook. Fl. Bor. Am.; Osnabruck and Prescott Junction, Rev. Dr Epstein; on Judge Malloch's farm and elsewhere about Brockville; Dartmouth River, Gaspé, John Bell, B.A.; St Ferreol, M. L'Abbé Provancher. Abundant on uncleared land along the Bedford Road, where the dried fronds are used by the farmers as winter fodder for sheep. Augmentation of Grenville, C. E., J. Bell, B.A.; near Komoka, C.W., W. Saunders. This fern is common also in the Northern States. I have a lax form, with long stipes and remarkably short somewhat triangular pinnæ, from Schooley's Mountains.

SCHIZÆA.

[*S. pusilla*, Pursh.—Newfoundland, De la Pylaie. I have no further information respecting its occurrence in British America. Professor A. Gray indicates its distribution in the United States thus:—"Low grounds, pine barrens of New Jersey, rare," which is not at all favourable to its being found in Newfoundland or Canada. Mr Eaton has sent me beautiful specimens from sandy swamps in Ocean County, New Jersey.]

Nat. Ord. OPHIOGLOSSACEÆ.

BOTRYCHIUM.

B. virginicum, Swartz.—Barren branch sessile, attached above the middle of the main stem, thin, delicate, veiny, tripinnate, lobes of the pinnules deeply incised; fertile branch bi- or slightly tri-pinnate. Very variable in size, usually a foot or more in height, but sometimes only a few inches. *Botrychium virginicum*, Swartz, A. Gray, J. Sm. *B. virginianum*, Schk. *Osmunda virginica*, Linn. Sp. Pl. *Botrypus virginicus*, Michx.—Not uncommon in the woods about Kingston and the surrounding country, as near Odessa, in Hinchinbrook, &c.; Delta; Toronto; Sulphur Spring, Hamilton, Judge Logie; Prescott, in woods, common, B. Billings, jr.; Nicolet, Montreal, Wolfe Island and Chippawa, P. W. MacLagan, M.D.; Belleville, rich woods, very common, J. Macoun; Ramsay, Rev. J. K. M'Morine, M.A.; River Marcouin, St Lawrence

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Gulf, also opposite Grand Island, Lake Superior. R. Bell, jr., C.E.; Marsoni, Riviere Rouge, and De Salaberry, west line, W. S. M. D'Urban; Montreal, Osnabruck, and Prescott Junction, Rev. E. M. Epstein; Hill Portage above Oxford House, Governor M'Tavish; Newfoundland, Miss Brenton, in Fl. Bor. Am.; Lake Huron to Saskatchewan, Hook. Fl. Bor. Am.; Gaspé, John Bell, B.A.; Stanfold, M. L'Abbé Provancher; Grenville, C. E., J. Bell; London, W. Saunders.

β. gracile.—Very small (5 or 6 inches high), fertile branch less divided. *B. gracile*, Pursh. Hill Portage, above Oxford House, Governor M'Tavish.

γ. simplex.—Barren branch oblong, pinnatifid, the lobes ovate, incised, veiny. *B. simplex*, Hitchcock. Grenville, C.E., John Bell, B.A.

B. lunarioides, Swartz.—Barren branch long-stalked, arising from near the base of the main stem, thick and leathery, bipinnate, the pinnules slightly crenate; fertile branch bipinnate. Root of long thick tuber-like fibres *Botrychium lunarioides*, Swartz, Gray. *B. fumarioides*, Willd., Provancher. *Botrypus lunarioides*, Michx.—Gananoque Lake, May 1861; Plains near Castleton, and woods near the Hop Garden, Belleville, rare, J. Macoun; Three Rivers, C.E., P. W. MacLagan, M.D.; Waste places west from Prescott Junction, rare, B. Billings, jr.; St Joachim, Provancher; L'Orignal, J. Bell; English's Woods, W. Saunders; in the Northern States this species grows in dry rich woods, "mostly southward," according to Professor Gray's Manual.

B. obliquum (Muhl.), appears to be chiefly distinguished by its larger size, more compound fertile frond, and the narrower oblique divisions of the barren one. *B. obliquum* (Muhl.), Pursh. Fl. Amer. Sept., vol. ii. p. 656. Newfoundland, Dr Morrison in Hook. Fl. Bor. Am.; "Wesleyan Cemetery, London," W. Saunders.

B. Lunaria, Swartz.—Barren branch sessile, arising from the middle of the stem, thick and leathery, oblong, pinnate; pinnæ lunate or fan-shaped, slightly incised on the rounded margin. *Botrychium Lunaria*, Swartz, Schk., Hook., Moore, J. Sm. *Osmunda Lunaria*, Linn.—Nipigon, 1853, Governor M'Tavish; N.E. America, Dr Hooker's tab.; Newfoundland, Saskatchewan, and Rocky Mountains to Behring's Bay in N. W. Am., T. Moore, Hbk. Brit. Ferns.

OPHIOGLOSSUM.

[*O. vulgatum*, L., which is widely distributed throughout Europe and Northern Asia, and grows also in the Northern United States, although there "not common," is to be looked for in Canada. In one of its forms (*O. reticulatum*, Linn.), it extends to the West Indies.]

Nat. Ord. LYCOPODIACEÆ.*

PLANANTEUS.

P. Selago, Pallisot-Beauvois.—Stem dichotomously branched, erect,

* In this order the arrangement of A. M. F. J. Pallisot-Beauvois is adopted, as it seems to afford the best basis for a readjustment of the genera of *Lycopodiaceæ*, which is much required. For P.-B.'s genus *Lepidotis*, I have thought it better to substitute the name *Lycopodium*, an old name that should not be discarded.

fastigiate; leaves in about 8 rows, more or less convergent or spreading, lanceolate, acuminate, entire; sporangia in the axils of the common leaves (not in spikes). *Lycopodium Selago*, Linn., E. B., Bigelow, Beck, Hook. and Grev., Torrey Fl. N. Y. ii. p. 508, Gray.—Labrador, Hudson's Bay to Rocky Mountains, Hook. Fl. B. A.; shore of Lake Superior and northward, Professor A. Gray, Man. Bot., N. S., p. 603. I have not seen Canadian specimens of this plant. The stations known show that it encircles Canada, and some of them are probably within our limits. Principal Dawson obtained the alpine variety on the White Mountains, Herb. Bot. Soc. Canada. It is a rare plant in the United States. There are two forms of this species (both of which are figured by Dillenius). *α. sylvaticus*, leaves convergent, almost appressed. *β. alpinus*, leaves widely spreading, stems shorter.

P. lucidulus. Stem dichotomously divided into long erect branches; leaves bright green, in about 8 rows, reflexed, linear-lanceolate, acute, denticulate; sporangia in the axils of the common leaves (not in spikes). *Lycopodium lucidulum*, Michaux, Pursh, Bigelow, Torr. Fl. N. Y. ii. p. 508, Gray, Beck, Darlington, Hook. and Grev. Bot. Mis. *L. reflexum*, Schk. *Lycopodium suberectum* of Lowe, a Madeira plant. *Selago americana*, *foliis denticulatis reflexis*, Dill. Hist. Mus. t. lvi.—Gananoque Lakes, Collins's Bay, Newboro-on-the-Rideau, woods in rear of Kingston, &c.; Prescott, common, B. Billings, jr.; Nicolet, C.E., St Catherine's and Grantham, P. W. MacLagan, M.D.; Belleville, in swamps and cold woods, rather common, J. Macoun; River Ristigouche, St Lawrence Gulf, R. Bell, jr., C.E.; L'Original, J. Bell, B.A.; London, W. Saunders; Ramsay, Rev. J. K. M'Morine, M.A. This species is stated by Professor Torrey to be rather common in New York State. "Frequently bears bulbs instead of capsules," Pursh.

[*P. alopecuroides*, P. Beauv.—The habitat "Canada" is given for *Lycopodium alopecuroides*, Linn., in the "Species Plantarum," ed. 3, vol. ii. p. 1565; but it is probably not a Canadian plant.]

P. inundatus, P. Beauv.—Stems prostrate, adherent to the soil, the fertile ones erect; leaves secund, yellowish green, lance-awl-shaped, acute; sporangia in distinct, terminal, leafy, sessile, solitary spikes. *Lycopodium inundatum*, Linn., E.B., Michaux, Pursh, Beck, Tuckerman, Torr. Fl. N. Y. ii. p. 508, Gray. *Plananthus inundatus*, Beauv. *L. alopecuroides*, Linn., in part?—In cedar swamps and overflowed woods, Canada, Pursh. Professor Torrey notices its occurrence in the north-western part of the State of New York. Professor Gray observes, that the leaves are narrower in the American than in the European plant, and suggests that it may be a distinct species. I have not yet seen Canadian specimens.

LYCOPODIUM.

L. clavatum, Linn.—Stems robust, and very long, prostrate, rooting, forked, with short ascending branches; leaves pale, incurved, linear-awl-shaped, tipped with a white hair point; sporangia in scaly catkins, which are usually in pairs on common peduncles. *Lycopodium clavatum*, Linn., E. B., Michaux, Pursh, Bigelow, Beck, Darlington, Spring, Hook.,

Torrey, Gray. *L. tristachyum*, Pursh? *L. integrifolium*, Hook. *L. aristatum*, Humboldt.—Occasionally found in the woods in rear of Kingston, but not common; Newfoundland, Hook. Fl. Bor. Am.; between Thessalon and Mississauga Rivers, Lake Huron, R. Bell, jr.; Prescott, common, B. Billings, jr.; Three Rivers, Temiscouata, and Wolfe Island, P. W. MacLagan, M.D.; Seymour, in pine woods, rare, J. Macoun; Ramsay, Rev. J. K. M'Morine, M.A.; River Ristigouche, St Lawrence Gulf, R. Bell, jr.; London, W. Saunders, C.E.; L'Orignal and L'Anse au Cousin, Gaspé, J. Bell; Belmont. The spores, chiefly of this species, constitute *pulvis lycopodii*, which is used by apothecaries, and was at one time employed for making artificial lightning in the theatres.

L. annotinum, Michaux.—Stems very long, prostrate, creeping, forked, with ascending branches; leaves bright green, spreading or slightly deflexed, in about five rows, linear-lanceolate, mucronate, serrulate; sporangia in scaly catkins, which are sessile, solitary, oblong-cylindrical, thick. *Lycopodium annotinum*, Michaux, E. B., Pursh, Beck, Tuckerman, Torrey, Fl. New York State, ii. p. 509.—Pine forests in Hinchinbrook; rocky woods in Pittsburg, on the north bank of the St Lawrence, near Kingston; Gananoque Lakes; L'Anse au Cousin, Gaspé, John Bell, B.A.; Prescott, common, B. Billings, jr.; Rivière du Loup, Nicolet, Montreal, and Kingston, P. W. MacLagan, M.D.; Belleville, in cool woods, common, J. Macoun; Ramsay, Rev. J. K. M'Morine, M.A.; Priceville, C. I. Cameron, B.A.; Newfoundland, Hook. Fl. Bor. Am.; St Augustin and Cap Tourmente, M. L'Abbé Provancher. Frequent in New York State, according to Professor Torrey. Of this species there are two forms, only one of which, the normal one, or type, I have as yet observed in Canada. The var. *β alpestre*, Hartm. Scan. Fl., having broader, shorter, paler, less spreading leaves, I have from the Dovrefeldt (T. Anderson, M.D.), Lochnagar (A. Croall), and entrance to Glen Fee, Clova, where I found it growing with the typical form.

L. dendroideum, Michx.—Stems upright, bare below, bushy above (giving the plant a tree-like aspect), arising from a long creeping rhizome, leaves more or less appressed; sporangia, in scaly catkins, which are sessile, cylindrical. *Lycopodium dendroideum*, Michx., Pursh, Bigelow, Hook., Beck, Darlington. *L. obscurum*, Linn., Bigelow, Oakes.—White cedar woods near Bath, abundant, and throughout the woods generally in rear of Kingston; Gananoque River; Priceville, C. I. Cameron, B.A.; Prescott, common, B. Billings, jr.; Nicolet, Mount Johnson, and Montreal, P. W. MacLagan, M.D.; Seymour and Cra-nahe, in cold moist woods, J. Macoun; River Ristigouche, Gulf of St Lawrence, R. Bell, jr.; Ramsay, Rev. J. K. M'Morine, M.A.; New Brunswick, Hook. F. B. A.; Osnabruk and Prescott Junction, Rev. E. M. Epstein; London, W. Saunders; Harrington, L'Orignal, and Gaspé, John Bell, B.A.; St Joachim, M. L'Abbé Provancher.

L. complanatum, Linn.—Stems rhizome-like with ascending branches, which are dichotomously divided, flattened; leaves short, in four rows, those of two rows imbricated, appressed, of the other two somewhat spreading; sporangia in scaly cylindrical catkins, in twos, threes, or fours, on a common peduncle. *Lycopodium complanatum*, Linn., Gray,

Blytt. *L. chamæcyparissias*, Braun. *L. sabinæfolium*, Willd.—Not uncommon in the woods about Kingston, and in rear; Newboro-on-the-Rideau; Gananoque River; River Ristigouche, St Lawrence Gulf, and St Joseph's Island opposite Campment D'Ours, Lake Huron, R. Bell, jr.; Ramsay, Rev. J. K. M'Morine, M.A.; pine grove near Blue Church Cemetery and woodlands west from Brockville, not common, B. Billings, jr.; Three Rivers and Temiscouata, C.E., P. W. MacLagan, M.D.; sandy woods around Castleton, sterile hills Brighton and Murray, J. Macoun; L'Original and L'Anse au Cousin, Gaspé, J. Bell, B.A.; Trois Pistoles, M. L'Abbé Provancher; London, W. Saunders. To this species is referred *L. sabinæfolium*, Willd., *L. chamæcyparissias*, A. Braun, with branches more erect and fascicled. Professor Asa Gray remarks:—"The typical form of *L. complanatum*, with spreading, fan-like branches, is abundant southern (in N. States), while northward it passes gradually into var. *sabinæfolium*." I have only one rather imperfect specimen of the European *L. chamæcyparissias*, collected at Bonn, on the Rhine, by my friend Professor G. S. Blackie, which does not differ in the branching from ordinary Canadian forms of *L. complanatum*. It appears to be quite a common species in the States, for I have it from a great many places.

SELAGINELLA.

S. spinulosa, A. Braun.—Small, prostrate, leaves lanceolate, acute, spreading, spinosely toothed; fertile branch stouter, ascending spike sessile. *Selaginella spinulosa*, A. Braun, Blytt, Norges Fl. *Lycopodium selaginoides*, Linn., Pursh Fl. Am. Sept. ed. ii. p. 654. *Selaginella spinosa*, Beauv. *Selaginella selaginoides*, A. Gray, Man. Bot. N. States, p. 605.—Gaspé, John Bell, B.A.; Canada, Michaux; Lake Superior and northward, pretty rare, Professor Asa Gray in Man. Bot. N. States; Canada, Pursh, who observes: "The American plant is smaller than the European."

STACHYGYNANDRUM.

S. rupestre, P. Beauv.—Much branched, leaves slightly spreading when moist, appressed when dry, carinate, hair-tipped; compact and moss-like, growing on bare rocks. *Selaginella rupestris*, Spring, A. Gray, Eaton. *Lycopodium rupestre*, Linn., Pursh Fl. Am. Sept. ed. ii. p. 654.—On the perpendicular faces of Laurentian rocks, along the north bank of the St Lawrence, in Pittsburg, and on the Thousand Islands at Brockville, &c.; Longpoint on the Gananoque River; near Farmersville, C. W., T. F. Chamberlain, M.D.; rocks in pine groves two miles west from Prescott, near the river, and on rocks west from Brockville, not common, B. Billings, jr.; Ramsay, Rev. J. K. M'Morine, M.A.; Beloeil and Mount Johnson, C.E., P. W. MacLagan, M.D.

DIPLOSTACHYUM.

D. apodum, P. Beauv.—Stems creeping, branched; leaves pale vivid green, of two kinds,—the larger spreading horizontally, ovate oblique, the smaller appressed, acuminate, stipule-like. Forms compact tufts. *Lycopodium apodum*, Linn., Pursh. Fl. Am. Sept. ed. 2, ii. p. 654.

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Selaginella apus, Gray, Eaton.—Abundant on low wet ground east of Front Street, Belleville, below the hill, where it was pointed out to me by Mr J. Macoun, July 1863. In September 1863, I found it sparingly but fertile, on grassy flats by the river side at Odessa. Near London, W. Saunders; Detroit River, C. W., P. W. MacLagan, M.D. Apparently not common in the United States. I have it from Schooley's Mountains. This is a very small, compactly-growing moss-like species, well adapted for cultivation under a glass shade. It was a great favourite with the late Dr Patrick Neill, in whose stove, at Canonmills, Edinburgh, I first saw it many years ago.

Nat. Ord. MARSILEACEÆ.

AZOLLA.

A. Caroliniana, Willd.—Pinnately branched with cellular, imbricated leaves; plant reddish, circular in outline, $\frac{1}{2}$ –1 inch in diameter; leaves ovate obtuse, rounded and roughened on the back (Eaton). Resembles a floating moss or *Jungermannia* (Torrey). Gray, Man. Bot., t. 14. Floating on the waters of Lake Ontario, Pursh Fl. Am. Sept., ed. 2, ii. p. 672. In the adjoining states, Professor Asa Gray notices it as occurring in pools and lakes, New York to Illinois and southward, and observes that it is probably the same as *A. magellanica* of all South America.

SALVINIA.

[*Salvinia natans* = *Marsilea natans*, Linn. Sp. Pl. "Floating like lemna on the surface of stagnant waters, in several of the small lakes in the western parts of New York and Canada."—Pursh Fl. Am. Sept. ed. 2, ii. p. 672. Professor Asa Gray states, that it has not been found by any one except Pursh, and he therefore omits it from his Manual of Botany of the Northern States.]

ISOETES.

I. lacustris, L.—Beloeil, C. E., P. W. MacLagan, M.D.; Saskatchewan, Hook. Fl. Bor. Am. This plant is spoken of by Pursh as growing in the Oswego River, near the Falls; and Professor Gray and others allude to it as not rare in the New England States. It should be carefully looked for in the numerous lakes and creeks of Upper Canada. It grows in muddy bottoms, forming green meadows under water. Much interest is attached to the genus *Isotetes*, since Professor Babington has shown that instead of one there are many species, or at least distinct races or forms, in Britain. In the United States four are known:—*I. lacustris*, Linn.; *I. riparia*, Engelm.; *I. Engelmanni*, Braun; and *I. flaccida*, Shuttlew., the last a southern form. Professor Babington is certain of the existence of at least eight European species:—*I. lacustris*, L.; *I. echinospora*, Dur.; *I. tenuissima*, Bor.; *I. adspersa*, A. Br.; *I. setacea*, Del.; *I. velata*, Bory.; *I. Hystrix*, Dur.; and *I. Duriei*, Bory. As yet we know of only one Canadian species, which is here rendered, rather uncertainly, *I. lacustris*. The American species are described in Gray's Manual, the British ones in the new Journal of Botany, London.

Nat. Ord. EQUISETACEÆ.

EQUISETUM.

The *Equiseta* having been described in a previous paper, it will be sufficient to give here a mere list of the species, with some additional notes obtained since the former paper was written.

E. sylvaticum, Linn. Newfoundland and New Brunswick, Hook. Fl. Bor. Am.

E. sylvaticum, β . *capillare*. Much branched; branches very long, straight, and exceedingly slender (capillary). Farmersville.

E. umbrosum, Willd. Belmont.

E. arvense, Linn. West from London, W. Saunders. The rhizome bears large spherical pill-like nodules, which are, however, more conspicuous in var. β . *granulatum*.

E. arvense, β . *granulatum*.

E. Telmateja, Ehrhart. Shores of Lake Ontario, Beck.

E. limosum, Fries.—The great value of this species and of *E. arvense* as fodder plants, is confirmed. On the western prairies horses are said to get "rolling fat" on *Equisetum* in ten days; and experienced travelers tell me, that their horses always go faster next day after resting at night on *Equisetum* pasture. The horses do not take to it at first; but after having a bit of *Equisetum* put occasionally into their mouths, they soon acquire a liking for it, and prefer it to all other herbage. Near Komoka, W. Saunders.

E. hyemale, Linn. Lake Huron, Hook. Fl. Bor. Am.; St Joachim, M. l'Abbé Provancher; London, W. S.

E. robustum, Braun. Stems much thicker than in *E. hyemale*, the ridges with one line of tubercles; sheaths shorter than broad, with a black band at base, and a less distinct one at the margin; teeth about forty, three-keeled. *E. robustum*, Braun, A. Gray. Grenadier Pond, on the Humber River near Toronto, 3d June 1862. It is difficult to decide whether this and other forms are really distinct from *E. hyemale*; certainly that species varies in size, in roughness, and other characters. In *robustum* the teeth are twice as many as in *hyemale*, but even this is perhaps not a constant character.

E. variegatum, Weber and Mohr.; St Joachim, M. l'Abbé Provancher.

E. scirpoides, Michaux.

E. scirpoides, β . *minor*.

E. palustre, Linn.—"Canada, from Lake Huron, Dr Todd, Mr Cleg-horn, Mrs Perceval, to the shores of the Arctic Sea, Dr Richardson, Drummond, Sir John Franklin, Captain Back."—Hook. Fl. Bor. Amer. Professor A. Gray speaks of "the European *E. palustre*," "attributed to this country (the N. American States) by Pursh, probably incorrectly." Dr Hooker indicates its existence, without doubt, in Arctic West America and Arctic East America. The name of the plant has occasionally appeared in Canadian lists, but I have as yet seen no Canadian specimen. It remains for Canadian or Hudson's Bay botanists to trace its southern limit on the American Continent. In Europe and Asia it has no tendency to Arctic limitation.

The Hypothesis of Molecular Vortices.

TO THE EDITOR OF THE EDINBURGH PHILOSOPHICAL JOURNAL.

SIR,—As the article on Thermodynamics in the "North British Review" is perhaps the most complete history of that science which has yet appeared, and is written with a scientific precision which is unusual in journals not specially devoted to science, I wish to correct an oversight that the Reviewer has committed in describing the "Hypothesis of Molecular Vortices," or "Centrifugal Theory of Elasticity," as proposed by me in 1849.* He speaks of atmospheres of ether surrounding nuclei of ordinary matter; whereas in the hypothesis, as I put it forward, the nuclei perform the functions of ether, and the atmospheres those of ordinary matter. Radiance is supposed to consist in oscillations of the nuclei, transmitted in waves by means of the forces which they exert on each other at a distance; and thermometric heat is supposed to consist in an agitation of the atmospheres, producing outward pressure according to the known laws of centrifugal force. Emission of radiance takes place when the atmospheres whirl faster than the nuclei oscillate, so that the nuclei are undergoing acceleration, and the atmospheres retardation; absorption of radiance takes place when the nuclei oscillate faster than the atmospheres whirl, so that the nuclei are undergoing retardation and the atmospheres acceleration. In perfect gases, the nuclei oscillate with little impediment from the atmospheres, and the transmission of radiance is rapid; in substances in a more dense condition, each nucleus is, as it were, loaded with a part of its atmosphere (like a pendulum in a resisting medium), and the transmission of radiance is slower. It is this peculiar view of the respective functions of the nuclei and the atmospheres, that constitutes the main distinction between the hypothesis put forth by me and other hypotheses involving atomic nuclei and atmospheres (as that of Mossotti), or accounting for the phenomena of heat by molecular motions (as that of Mr Herapath).

Of course a mechanical hypothesis does not form an *indispensable* part of Thermodynamics, more than of any other physical science; but if a hypothetical theory of Thermodynamics is to be used, it appears to me that its fundamental principles must be such as I have described.—I am, Sir, your most obedient servant,

W. J. MACQUORN RANKINE.

GLASGOW, 1st March 1864.

* Transactions of the Royal Society of Edinburgh, 1850-51.

REVIEWS AND NOTICES OF BOOKS.

Journal of the Scottish Meteorological Society, New Series,
No. I. William Blackwood and Sons, Edinburgh and
London. January 1864.

The Council of the Scottish Meteorological Society having recently resolved to publish their proceedings in the form of a quarterly journal, the first number of the new series, got up in a handsome form by the Messrs Blackwood and Sons, has just made its appearance. The Society will now be better able to carry out the important objects it has in view, and which cannot be better stated than in the words of the prospectus, namely,—“to investigate Scottish meteorology, and particularly to ascertain the leading features of the climate of different districts of the country; to point out the bearings of meteorology on public health, and on the prevalence of diseases affecting crops and live stock; to investigate the origin, progress, and recurrence of storms in Scotland; to point out the difference between the climate of Scotland and that of other countries; to ascertain the peculiar causes of the climate of Scotland in summer as well as in winter; to investigate the general laws regulating atmospheric changes, the discovery of which may lead to a knowledge of the coming weather; and to disseminate meteorological information by the circulation and publication of interesting papers.”

For the first few years of the Society's existence, its efforts were almost exclusively devoted to the collection and reduction of meteorological data, and to the establishment of observatories over the country, of which there are now seventy-two. As it is now eight years since the Society began its operations, a sufficient time has elapsed to warrant conclusions being drawn regarding the climate of Scotland in its relations to pressure, temperature, humidity, rain, and wind. Accordingly, various papers have of late appeared in their proceedings, some of which we noticed in this Journal at the time of their appearance, giving a *resumé* of these elements of the weather, and pointing out the influence they severally exercise on the health of the people and on vegetation. In these papers the chief principles have been deduced that give its peculiar character to the Scottish climate, and in this way an amount of reliable information respecting its advantages and disadvantages in the different seasons of the year has been disseminated among the people. The good services thus rendered to science and the public service are now, we are happy to see, begun to be recognised and appreciated, as evinced by the comparatively

large accession to its membership the Society has lately received. But considering the importance of the objects aimed at, the support yet given is not nearly such as might have been expected; and there is no doubt that many more will be induced to join the Society when they are made aware of its claims, and the easy terms of admission to membership. The claim to greater support is strengthened by the consideration that in the case of meteorological investigations, numbers and extensive combination are indispensable to insure anything like success. We hope the time is not far distant when Government will recognise the claims this and other scientific bodies have on its support.

The first paper in the present number of the journal is a very interesting one by Mr Buchan on the "Weekly Extreme Temperatures and Rainfall in Scotland for March, April, May, and June, on an average of seven years ending with 1863." From the valuable tables which accompany this paper, the following highly interesting and important conclusions are drawn regarding the occurrence of frost over Scotland:—

"1st, That up to the end of May frosts may be expected to occur every week somewhere in Scotland. This remark is applicable in its fullest extent to the inland parts of the country taken as a whole.

"2d, That in every part of the country, frost will occur some time or other in the months of March and April; in inland and eastern districts nearly every year some time in May; but in the islands and in the west, the probability of its occurring or not occurring in May is about the same.

"3d, That in June frosts do not occur except in high situations situated in hollows and surrounded by hills.

"4th, That at each particular station frost may be expected to occur every week in inland and eastern districts up to about the end of the first week in May, but at western and island stations only till the middle of April.

"5th, That from the second week in May till the end of the month, frost is as likely to occur as not in inland districts on the east coast till the middle of May, and in the west and islands from the middle of April to the end of the first week in May.

"6th, That at inland places frost rarely—that is, practically—ceases to occur from the beginning of June; and at all other places in the west and east, and islands, this takes place at least a fortnight earlier, or in the middle of May.

"7th, That at inland places in elevated situations and in hollows, such as Braemar, Thirlestane, and Stobo, frost cannot be considered as ceasing to occur till about the middle of June, or a full month later than in the islands and along the sea-coast.

"8th, That at elevated stations among hills, such as Castle

Newe, frost may be regarded as of certain occurrence every week up to the end of the first week of May; in the west and islands to the end of March; and elsewhere to the middle of April.

"9th, That at Glasgow and Baillieston, frost may not be expected to occur after the first week of May, being a fortnight earlier than the other inland stations. Query—Is this due to the proximity of the Atlantic? and is the effect of that ocean with respect to frosts really felt in so marked a manner in localities at so great distances from the west coast?

"These conclusions will be at once recognised to be of the utmost value in their bearings on the character of the climate of the different parts of the country, and in determining how far each locality may be suitable in the treatment of different classes of diseases, and in supplying valuable information in deciding whether particular crops may be cultivated with a fair prospect of success. To farmers and gardeners they are invaluable, as indicating the proper and safe time for planting the potato and other tender exotics grown in the open air, inasmuch as the time when frosts most commonly cease to occur is thereby made known."

If the temperature during the day repeatedly rise to 65°F , the heat thus received by the crops is sufficient for the growth and ripening of oats and the coarser sorts of grain; it is also sufficient for the growth of wheat and the finer cereals up to the period of flowering. The observations made respecting the occurrence of this important degree of temperature over the country are thus summarised:—

"1st, That as regards the months, in the central and eastern districts, a temperature of 65°F will occur some time in May and June; in April the chances of its occurring or not occurring are about equal; and in March it is not likely to occur at all;—in the western districts these occur nearly a month later; and in islands nearly two months later.

"2d, That as regards the weeks, 65°F occurs so seldom, that it need not be expected on the mainland till the end of April, and in a few places there not till a week or a fortnight later; and in the islands till the second week of June.

"3d, That in these different localities, for four weeks after the above times, it occurs just so often as to make it a matter of equal probability, whether in any of these weeks it happen or not.

"4th, That, from and after the last week of May in the central and eastern districts, from the first week of June in the western districts, it will probably occur every week; and in the islands during the whole of June, the chance of its not occurring is the greater.

"5th, That the districts where 65°F is of most frequent occur-

rence, are Strathmore, Strathearn, the Lothians, and the Merse in Berwickshire; and where it occurs least frequently are the northern islands."

The temperature of 70°O , the occasional occurrence of which is necessary for the proper ripening of wheat, is shown to have occurred only once in Orkney, and twice in Shetland and the outer Hebrides during the period. It is of most frequent occurrence in Berwickshire, the Lothians, Fife, Strathmore, and along the Moray Firth; in other words, in the best wheat-producing counties.

The second paper, also by Mr Buchan, on the "Isothermals of the British Isles in January and July," is of great value, and is accompanied by a beautiful map in colours, engraved by the Messrs Johnston, Edinburgh. The results are given in the following extract:—

"With regard to the lines of equal summer heat, there are one or two points of some interest which may be pointed out. The most noticeable of these points is the general slope of the lines from north-east to south-west, thus indicating a higher summer temperature in the east as compared with the west of the British Isles. This is occasioned by the general direction of the wind in July being from the Atlantic Ocean, which at this time of the year is colder than the land, and also by the greater amount and less height of the clouds in the west as compared with the east. The curving northward of the lines in the centre of England is deserving of attention. This is owing to the circumstance, that it is only this part of the island which is broad enough to allow of the central parts acquiring so much of a Continental summer climate as to give a decidedly northern flexure to the lines. The southern part within the isothermal of 64°O , of which London may be said to form the centre, is interesting as being undoubtedly the hottest district in the British Isles during summer, and consequently that part best fitted for the culture of exotics which require a high summer temperature. The colder summer climate of the south coast, as compared with the district immediately to the north of it, just referred to, is caused by the moderating influence of the sea which flows round it. The whole of the British Isles south of the Dornoch Firth and Skye may be considered as suited to the successful cultivation of wheat and the finer cereals, except those districts where the rainfall is excessive or the elevation too great.

"The most noticeable point with respect to the lines of equal winter temperature is the crowding of the high temperatures in the western, and particularly the south-western, parts of the British Isles. Ireland and the peninsula of Cornwall, being, as it were, more completely bathed in the warm waters of the Gulf-Stream during the winter months, exhibit for this time of the

year a markedly high temperature for their position on the globe, being no less than 26°.0 above their normal winter temperature.* The winter temperature, 39°.0 , of the west coast of Great Britain is the same from the most northern of the Shetland Isles as far south as North Wales; and the winter temperature, about 37°.0 , of the east coast, from Kinnaird's Head in Aberdeenshire to the mouth of the Thames. Hence in winter the west coast is generally advantaged at least 2°.0 , and the south-west of Ireland as much as 6°.0 of mean winter temperature, as compared with the eastern parts of England in the same latitude; whereas the eastern coast of Great Britain receives no benefit whatever, as regards its mean winter temperature, from the North Sea, since the cold of winter is quite as low in these as in the central parts of the island. Does not the higher winter temperature of the west coast of Ireland, as compared with that of Great Britain, supply additional proof of the Gulf-Stream reaching our shores, as it would be difficult to account for the decrease in the temperature of the Atlantic as we proceed from west to east, unless on the supposition of a slow gradual translation of its whole waters eastward, under a cold wintry atmosphere? The higher winter temperature of the west coast in winter is owing to these causes:—(1.) The warm south-west wind, which loses some of its heat before reaching the east coast. (2.) The larger amount of vapour deposited in rain on the west coast—being nearly double that in the east—which thus liberates a very large quantity of latent heat. (3.) The larger amount of vapour in the west, which obstructs radiation, not only when in a visible state in the form of clouds, but also when dissolved through the atmosphere in an invisible state, in which state, as shown by the recent researches of Professor Tyndale, it serves as a covering to the earth both from its own radiation by night, and from the sun by day, which would otherwise burn up everything in the fierceness of his heat."

The next paper is an "Address on the Importance of Medical Climatology," by Dr Scoresby-Jackson, the convener of this department, lately added to the Society's operations. The address is judicious and able, and contains much useful information, and many suggestive practical remarks. The subject is one of great importance and complexity, and will require for its elucidation the active and willing co-operation of medical men and meteorologists for a series of years. The little that is yet known of the

* The excess of the actual temperature of January over the normal temperature is even more striking in the case of Shetland. Thus Lerwick should only possess a January temperature of 3°.0 , if it received no more heat than is due to its position on the globe in respect of latitude, whereas, owing to the causes mentioned in the text, its temperature for that month is 39°.0 , or 36°.0 higher than it would otherwise be.

subject, is given by Dr Jackson with a modesty and candour truly admirable. The committee on this subject, indeed, find it almost necessary to commence investigation at the very beginning, and to limit themselves very much to the accumulation of accurate observations of well-attested facts. It is to be hoped that the medical faculty will at once come forward and give their hearty support to the Society in carrying out this object.

Flora of Ulster and Botanist's Guide to the North of Ireland.

By G. DICKIE, A.M., M.D., F.L.S., Professor of Botany, Aberdeen. Aitchison, Belfast, 1864. 24mo, pp. 176.

We have here a most useful and accurate botanical guide to the flora of the north of Ireland, written by one who is already known as the author of an excellent flora of Aberdeenshire. The district included in the work lies to the north of the 54th parallel of latitude, extends due west from Dundalk, and therefore includes nearly the whole of Ulster, and the northern portions of Leitrim, Sligo, and Mayo, belonging to Connaught. As to geological structure, it may be said that "Silurian formations occur in the south-east; granite in the Mourne Mountains; in the east there is an extensive mass of basalt,—chalk, greensand and oolite being here and there exposed; metamorphic and granite rocks appear on the north and north-west, carboniferous limestones in the south-west, and Devonian rocks in parts of the interior. The higher mountains are chiefly grouped in the east, north-west, and west. The extreme highest points are,—in County Down, Slieve Donard, 2796 feet; in Donegal, Muckish and Erigal, respectively 2190 and 2460 feet; in Mayo, Nephin, 2646 feet."

The climate, in the northern parts of Ireland, which are indented with arms of the sea, is comparatively mild and moist, and the extreme ranges of temperature are moderate. The temperature of the Atlantic is about 3° above that of the North Sea, for the six months beginning with October and ending with March; and there are many seaweeds, which indicate the milder temperature of the sea on the Irish coasts.

Dr Dickie follows in his work Hooker and Arnott's British Flora, and he adopts Watson's types as regards the distribution of species. The general characters of the flora are thus stated: plants of Germanic and Highland types, especially the former, constitute a very insignificant part of the Ulster flora; in the more northern counties a few of the Scottish type are plentiful, as *Empetrum nigrum* and *Ligusticum scoticum*; a third of the entire number belong to the Atlantic type; plants of the English type form a decided character in the flora. The author mentions 570 *dicotyle-*

dons, 192 *monocotyledons*, and 43 ferns and their allies, making in all 805 species. The *Pteris aquilina* ceases at a lower level in the north of Ireland than it does in Scotland, and so does *Erica Tetralix*. We want the true alpine flora in Ulster, as none of the mountains attain a sufficient elevation. We have only such sub-alpine plants as *Saxifraga stellaris*, *Salix herbacea*, *Carex rigida*, *Lycopodium Selago*, *Saxifraga umbrosa*, *Arbutus Uva-Ursi*, *Rubus Chamæmorus*, *Alchemilla alpina*.

With the Latin name of the species there is given the English name, the time of flowering, and the range, with the various localities in Ulster in which it has been found. In a supplement, lists are given of species doubtfully native, and of others which are not strictly indigenous.

We have great pleasure in recommending the book as being well executed, and as being a most valuable pocket companion for students who are examining the botany of the north of Ireland. To botanical pupils in the College of Belfast, the work supplies a long-felt desideratum; and to all who are examining the distribution of plants in Great Britain and Ireland, the facts in this volume must prove highly useful.

A Hand-Book of Descriptive and Practical Astronomy.

By GEORGE F. CHAMBERS, F.R.G.S. Murray, London, 1861. 12mo, pp. 514.

While we have abundance of manuals in various departments of science in this country, there is undoubtedly a deficiency as regards astronomy. Mr Chambers has produced a book which fully supplies this want. The work is designed to occupy a middle place between purely elementary works and advanced treatises. He has rendered it attractive to the general reader, useful to the amateur, and valuable to the professional astronomer as an occasional book of reference. The author first gives a sketch of the Solar System—the sun and the various planets, each of which are discussed in separate chapters. He next considers Eclipses and their associated phenomena,—eclipse of the sun and moon, transits of the inferior planets, and occultations. The next subject taken up is the Tides. Then follow miscellaneous astronomical phenomena, as the Obliquity of the Ecliptic, Precession, Nutation, Aberration, Parallax, Refraction, Twilight, Zodiacal light, &c. The subject of Comets occupies the whole of the fifth book; and, after making some interesting general remarks, the author proceeds to treat of Periodic comets, remarkable comets as Donati's, the comets of 1811, 1843, and 1860, cometary statistics and historical

notices. Book sixth is occupied with Chronological Astronomy, time in its various phases—hours, days, years, measurement of time, solar and zodiacal time, &c. Book VII. is entitled *The Starry Heavens*, embracing the pole star, double stars, variable stars, clusters and nebulae, the milky way, and the constellations. The various astronomical instruments are then noticed and described—telescopes, the equatorial, the transit instrument, &c. A sketch of the history of astronomy is given in Book IX., and the author concludes in Book X. with Meteoric Astronomy, including the consideration of aerolites, shooting stars, and other meteors. The whole is illustrated by an extensive series of excellent woodcuts, and references are given to the principal authorities. Amidst such a multiplicity of subjects it is not possible to select passages for quotation. Suffice it to say, that the work is one of standard merit; that it will completely repay a careful perusal; that it is carefully executed, and that it gives the account of most interesting phenomena in a pleasing and popular style. We do not know a better astronomical text-book for a student.

PROCEEDINGS OF SOCIETIES.

Royal Society of Edinburgh.

Monday, 7th December 1863.

Professor Innes, one of the Vice-Presidents, delivered the following Opening Address:—

GENTLEMEN,—The opening of our Session requires that I should lay before you the state and prospects of our Society, which I hope may to some extent be considered the criteria of the state and prospects of the sciences which it cultivates.

The Society has lost since the commencement of last Session by death, six Fellows, viz.,—Robert Allan, Esq., Beriah Botfield, Esq., Dr James Keith, Dr David Boswell Reid, Professor Connell of St Andrews, Professor Mitscherlich of Berlin; and by resignation, two, the Rev. G. V. Faithfull and D. R. Hay, Esq.

In room of whom the Society has elected twenty-five new Fellows, viz.,—Professor Blackie, William Brand, Esq., W.S., Robert Campbell, Esq., advocate, Dr Hugh F. C. Cleghorn, India, Charles Cowan, Esq., W. Dittmar, Esq., Dr J. Matthews Duncan, the Right Hon. Lord Dunfermline, Professor Everett, Nova Scotia, James Hannay, Esq., William Jameson, Esq., India, Hon. Lord Jerviswoode, Charles Lawson, Esq., Hon. G. Waldegrave Leslie, G. R. Maitland, Esq., W.S., Edward Meldrum, Esq., Rev. Dr

Nesbit, Hon. Lord Ormidale, David Page, Esq., Dr A. Peddie, James Sanderson, Esq., Deputy-Inspector of Hospitals, Dr John A. Smith, Dr Murray Thomson, Dr J. G. Wilson, Dr John Young.

Our roll, therefore, stands thus:—The number of Fellows in 1862 was 258, of which we have lost by death 6, by resignation 2=8, leaving 250. To which add the new Fellows, 25, making the whole number of the Fellows of our Society 275, a larger number than has appeared on the list for many years.

I am enabled, chiefly through the active kindness of our Secretaries, to offer a few notices of the members we have lost, during the past Session.

ROBERT ALLAN, son of Mr Thomas Allan, a banker in Edinburgh, a Fellow of the Society, and for many years Curator of its Museum and Library, and well known as an early and successful collector of a fine cabinet of minerals, was born in 1806, and educated at the High School and University of Edinburgh. He inherited his father's taste for minerals, and while still a youth followed out the study in extended travels in company with Professor Haidinger, who introduced him to the acquaintance and to the cabinets of all the chief foreign mineralogists—among others, Berzelius and Mitscherlich.

Mr Allan passed advocate in 1829, but never practised, and was admitted a Fellow of this Society in 1832. He was also a member of the Geological Society of London.

Mr Allan published in 1894 a *Manual of Mineralogy*, the classification founded on the external character or natural historical arrangement.

In 1837 he edited a fourth edition of "Phillips' Mineralogy," in which he added notices of 150 new minerals.

On his return from an excursion to the volcanic district of Italy and Sicily, Mr Allan presented to this Society a set of specimens of volcanic rocks of the Lipari Isles, with a descriptive notice, an abstract of which is in our Transactions, of date 16th January 1831.

He communicated an account of a visit to the Geysers and Hecla to the British Association at Glasgow, in 1855.

Mr Allan died in consequence of a fall in his garden.

BERIAH BOTFIELD was of a Shropshire family, in which county his grandfather, Thomas Botfield, made his large fortune as a manager and lessee of the Dawlay Collieries. Thomas's third son inherited Norton Hall, near Daventry, in Northamptonshire, and lived the life of an English sporting squire. He married Charlotte, daughter of William Withering, M.D., F.R.S., the author of "The Botanical Arrangement of British Plants." The only child of that marriage was Beriah, the subject of the present notice, who, in addition to his father's property, inherited the estates of both his uncles, and had become before his death a man of very large fortune.

Beriah was born 5th March 1807, and succeeded his father in 1813. He was educated at Harrow and Christ Church, where he took his Bachelor's degree in 1828.

After leaving Oxford he made a tour in the Highlands of Scot-

land, a journal of which he printed for private circulation,—printed at Norton Hall, 1830, 12mo.

He was High Sheriff of Northamptonshire in 1831.

In 1840 he was elected Member for Ludlow, and again in 1841.

In 1847 he was beaten by the Whig candidate.

In 1857 he was solicited to stand again, and he sat in Parliament for Ludlow for the rest of his life.

Mr Botfield was a member of the Royal Society of London, the Royal Geographical Society, Royal Institution, Society of Arts, of the Antiquaries of London, Scotland, and Copenhagen, of the Royal Irish Academy, l'Institut d'Afrique, and of all the principal Societies in the Kingdom, and of a great number of literary Clubs,—as the Roxburghe, Bannatyne, Maitland, Spalding, Surtees, Abbotsford, Camden, Percy, Ælfric, Hakluyt, Cheetham; to most of which he gave valuable contributions, his part being generally to defray the expense.

In addition to these, and some smaller tracts printed for private circulation, Mr Botfield published "Notes on the Cathedral Libraries of England," from a personal examination, 1849; "Prefaces to the First Editions of the Greek and Roman Classics, and of the Sacred Scriptures," 1861. Large 4to.

Another work, for which he was making collections when he died, and which would have been of great interest and value, was intended to illustrate the history of the old monastic libraries of England. A collection of the extant catalogues and inventories of these was already in type, to which he meant to add the catalogues of other Middle Age libraries. His collections, made for these objects will, it is feared, be lost to the world by his death. He had previously edited (in 1838), for the Surtees Society, catalogues of the Library of Durham Cathedral, at various periods.

In 1858, Mr Botfield printed, for private circulation, *Stemmata Botteuilliana*, a large volume illustrating the descent and antiquities of all the Botteuilles, Thynnes, and Botfields.

He was a liberal collector of pictures, and was also known as a zealous book-hunter.

Mr Botfield married Isabella, daughter of Sir Baldwin Leighton, Bart., but left no family; and has entailed a considerable part of his property on the second son of the Marquis of Bath, in respect of a very old but perhaps real connexion between his family and the Thynnes.

JAMES KEITH, second son of William Keith of Corstorphine Hill, accountant in Edinburgh, was born 29th November 1783, and was educated at the High School and University of Edinburgh. He was apprentice to Messrs Bell, Wardrope, and Russell; went to London in 1804, and attended the London Hospital and Guy's. Was surgeon of the Berwickshire Militia for two or three years, which he resigned on entering into partnership with Dr Andrew Wardrope, which connection terminated by Dr Wardrope's death in 1822.

Mr Keith took the degree of M.D. in the University of Edin-

burgh in 1804, and he became a Fellow of the College of Surgeons in 1810. He was physician to the Deaf and Dumb Institution for many years. From the extreme shyness of his disposition, his worth and ability were known only to a limited circle of intimate friends. He died 12th May 1863. His widow and two sons survive—William Alexander, M.A. Oxon., and Charles Maitland.

DAVID BOSWELL REID was the second son of Dr Peter Reid, physician in Edinburgh. His mother, Christian Arnot, was the eldest daughter of Hugo Arnot of Balcormo, advocate and antiquary, well known to the last generation by his book on the history of Edinburgh and his collection of Scotch criminal trials—and perhaps still better by the extraordinary attenuated, almost skeleton, figure of the old gentleman preserved to us in Kay's Portraits. Dr Peter Reid (whose mother was a Boswell of the Balmuto family) was the editor of Dr Cullen's "First Lines of the Practice of Physic," 1802. A new edition was published, with supplementary notes, in 1810. He was also the author of a little duodecimo volume, entitled "Letters on the Study of Medicine and on the Medical Character, addressed to a Student," Edin., 1809. Besides the subject of my present notice, Dr Peter Reid had two sons,—Dr William, a lecturer in Edinburgh on the practice of Medicine, and Dr Hugo, well known as the author of several popular works, the last of which is a modest and temperate memoir of his distinguished brother, to which I beg to acknowledge my obligation.

David Boswell Reid was educated at the High School and University of Edinburgh. At the former, Mr Pillans, the rector, has mentioned him as "among the head boys of the Rector's class." While a medical student he became a member of the Royal Medical Society, of which he was chosen senior president in 1826–27, his junior being James Kay, now Sir James Kay Shuttleworth.

In 1827, Mr David Reid commenced a course of practical chemistry, which was very useful and very popular. He aimed at enabling each student to familiarise himself, by experiments made under the directions of a teacher, with the properties of the chief chemical substances, and the phenomena attending their action on each other.

After much approval in his extra-mural lecture-room, he joined Dr Hope in the College, and was again quite successful in the object of his course. But the Professor and Assistant had some misunderstandings, which led Mr Reid to leave the College, and renew his independent lectures, which were highly appreciated—attended by all classes,—the young ambitious student,—the veteran philosopher and man of science,—the man of intelligence feeling the want of science. On his benches met Dr Chalmers and Sir John Leslie, Professors George Joseph Bell and Pillans, Dean Ramsay and Mr Combe.

After the burning of the Houses of Parliament, and in contemplation of a new building, when a committee of the Commons was inquiring on the subject of its ventilation and acoustics, Dr Reid

was examined as a witness, from having devoted much attention to those subjects, and having shown excellent examples of his skill, first in his own lecture-room, and, later, in the great temporary edifice, erected 15th September 1834, in the High School ground, for the Edinburgh dinner to Lord Grey, at which 2768 persons were present, and 240 ladies in the gallery, and each individual speaker was distinctly heard.*

The result of his examination was, that Dr Reid was employed to direct the ventilation and acoustics of the temporary House of Commons in 1836. It is not pretended that his plans gave universal satisfaction to the 700 members, each of whom had a different notion, and of course a peculiar constitution of body to be suited. But, after ten years' experience, in 1846, a fair committee of the House reported as to "the great improvement effected," and "concurred in the general opinion in its favour."

In 1840 arrangements were made for Dr Reid settling in London, and, while taking charge of ventilating the temporary House of Commons, superintending also the ventilation of the new building then in progress. This brought Dr Reid necessarily into close contact with the architect of the new palace, Mr Barry, and unfortunately they did not agree. The difference got worse and worse, till in 1845 they were no longer on speaking terms, and every detail of such extensive operations had to be settled by correspondence,—a state of things which could not be allowed to last. The quarrel broke out in some strong expressions of Dr Reid,—a prosecution for libel by Mr Barry,—a pretty general attack on Dr Reid by the public press, and a Reply by him to "The Times" newspaper [1845-47].

In 1852 a negotiation was entered into, by which the Government proposed to secure Dr Reid's services permanently, and to throw the ventilation of the whole buildings of the Houses under his charge,—one part of which, the House of Lords, had hitherto been managed by Mr Barry on a different system,—but "these negotiations were abruptly broken off." In fact, Dr Reid was turned off, after sixteen years successful service, and, as his brother tells us, "a small sum was given to him as some compensation for the loss which he had sustained. His friends who knew his whole career, and the proceedings connected with his removal to London, to take the charge of ventilating the Houses of Parliament, were of opinion that the sum awarded was totally inadequate to compensate for the sacrifices he had made."

Dr Reid went to New York in 1855. He delivered lectures in the Smithsonian Institution there, and at Boston. In the beginning of this year (1863), he received the appointment of Inspector of Military Hospitals, but soon after, while engaged in an official journey, he died suddenly at Washington, on 5th April 1863.

Dr Reid's system of ventilating great buildings, where crowds habitually assembled, consisted in forcing in a current of air by

* The Pavilion was 118 feet in length by 101 feet in breadth.

means of a powerful engine—the air being previously washed to free it from dust and to give it the requisite moisture. Some of his experiences are curious.

“The house is heated to 62° before it is opened, and maintained in general at a temperature between 63° and 70°, according to the velocity with which the air is permitted to pass through the house. This velocity is necessarily regulated by the numbers present, the temperature to which the air can be reduced in warm weather, and the amount of moisture which it may contain when the quantity is excessive. Some members are much more affected by an excess or deficiency of moisture than by alterations of temperature. In extremely warm weather, by increasing the velocity, air even at 75° may be rendered cool and pleasant to the feelings.”

He goes on to say—“The temperature may always be advantageously increased and the velocity diminished before the usual dinner hour. After dinner, other circumstances being the same, the temperature should be diminished, the velocity increased, and the amount of moisture in the air reduced. During late debates, as they advance to two, three, four, or five in the morning, the temperature should be gradually increased as the constitution becomes more exhausted, except in cases where the excitement is extreme.”

Next to the Houses of Parliament, Dr Reid's greatest and most successful undertaking of ventilation was the St George's Hall at Liverpool, in which immense building, on some occasions, there have been as many as 4500 persons for about ten hours; the air during all that time having been supplied to all that multitude in a pure state, and in a comfortable and agreeable condition as to temperature and moisture.

Dr Reid superintended while in this country the arrangements for ventilating the royal yacht, “The Victoria and Albert,” and the steamships used in the expedition to the Niger, in both instances to the entire satisfaction of his employers; and since going to America, he was employed in the ventilation of a Russian frigate, “The Grand Admiral,” built at New York

ARTHUR CONNELL, eldest son of Sir John Connell, Judge of the Admiralty Court, and author of a well known work on the Law of Scotland respecting Tithes, entered the High School of Edinburgh in 1804, and the University of Edinburgh in 1808, where he studied under Playfair, Leslie, Dugald Stewart, and Hope. From Edinburgh Mr Connell went to Glasgow College, where he studied under Jardine and Young, and, having obtained a Snell exhibition, went to Balliol College, Oxford, in 1812.

In 1817 Mr Connell passed advocate at the Scotch Bar, but he had from boyhood a remarkable turn for science, especially botany and chemistry, and he ultimately devoted himself exclusively to the latter science.

In 1840 he was presented to the Chair of Chemistry in the University of St Andrews.

In 1843 Mr Connell was candidate for the Chemistry Chair at

Edinburgh, vacant by the death of Dr Hope, and though not successful, produced a collection of testimonials of the highest character. Most of these were the more worthy of attention as not made for the occasion and so in some degree influenced by private friendship. They are for the most part notices in the published works of eminent chemists and in scientific journals, of Mr Connell's chemical labours, and the papers in which these were announced and described.

Having failed in this object of his ambition, Mr Connell continued to study and teach his favourite science at St Andrews till 1856, when the fracture of a limb, and its effects upon a constitution already long enfeebled, completely incapacitated him from active duty.

Mr Connell became a member of this Society in 1829, from which time till 1843 he contributed to the Transactions, or published in the pages of the "Edinburgh Philosophical Journal," memoirs to the number of 29.

His chief merit lay in his skill and unrivalled accuracy as a mineral analyst. To him we are indebted for several new mineral species—for the discovery in the minerals Brewsterite and Harmotome of the earth barytes in combination with silicic acid—that earth previously having been found combined only with the sulphuric and carbonic acids; while his ascertaining the constitution of the mineral Greenockite, on one grain of the substance, displayed a dexterity seldom if ever surpassed.

Mr Connell also engaged in somewhat more ambitious researches on the voltaic decompositions of alcohol, ether, and other liquids, and has presented us with an instrument for ascertaining the dew point, superior in several respects to that generally used.

Mr Connell was of a very retiring nature, modest, gentlemanly, and gentle in disposition. He expired peacefully on 31st of October last.

EILARD MITSCHERLICH, born 7th January 1794, at Neurede, in the Grand Duchy of Oldenburgh, where his father was a minister of the Lutheran Church, was educated at Heidelberg and Paris, and studied afterwards at Göttingen. His first objects of study were language and ethnology. Later in life he devoted himself more to natural science, and especially chemistry. He assisted Berzelius at Stockholm for some years.

In 1821 he was appointed Professor of Chemistry in the University of Berlin, and attached to the Friedrich Wilhelm Institut. His lectures were held in high estimation, and attended by numerous classes of students.

In 1828 he was elected an Honorary Member of this Society, and in 1829 was awarded a Medal by the Royal Society of London for his discourses "regarding the laws of crystallization and the properties of crystals."

In 1852 Mitscherlich was elected an Associate Member of the Institute of France. His great European reputation is founded on his studies on crystallization and some ingenious adaptations of instruments for practical chemistry. His text-book—*Lehrbuch der Chemie*—has gone through a great many editions.

Mr Mitscherlich died in the present year.

His experiments and disquisitions tended to establish the rule that bodies crystallizing in the same shape (isomorphous) have an analogous chemical composition—throwing great light on chemical classification, and giving us one of the greatest generalizations (after the Atomic theory) which chemistry has gained by the researches of philosophers.

When I have laid before you these slender memorials of our deceased brethren, I may claim to have discharged the real duty of my office to-night. If indeed I were worthy to fill the chair in which your favour has placed me,—if I had, like some of our distinguished Fellows, a knowledge of all science, or even a special acquaintance with any *one*,—it would be my duty to submit to you a survey, or at least some outline, of the progress of science among us and among our neighbours. But for such a task you know me to be ill qualified. I should not venture to speak in the language of science anywhere, and least of all in the presence of the men whom I now see around me.

There are subjects, however, in which scientific men and men of no science feel an equal interest—which must engage the attention of every person of common intelligence.

Among these is the great step recently made in African geography—the discovery of the head of the Nile. No other geographical discovery can ever compare with this. It is not the solution of a puzzle in the Geographical Society. It is removing the “*Impossible*”—the very type of impossibilities—from our books. It is opening to the whole world the mystery which was a mystery even to the initiated. Poets have lost a topic! What philosophers and historians guessed and speculated about, is now written down plain on the map. That is now clear which has been wondered at since men began to ask the meaning of anything. We have lost the oldest subject of curiosity in the world!

A grave, prosaic mind loses its equanimity, and gives way to the charm of romance at the thought of the veil being raised that has for so many thousands of years covered the head of the great mysterious river which was worshipped of old—not more for its beneficent overflowings, regular as the seasons, yet unaccountable, than because of its unknown, unapproachable source.

I do not mean that the facts which our travellers have brought to light run counter to the conclusions of former geographers. On the contrary, I think the body of history on the one hand, the speculations of science on the other, had prepared the world for such a discovery. Glancing at the ancient, I mean the classical authorities, without arraying them before you, I may say that among innumerable fables and much unphilosophical reasoning, they almost concur in giving the Nile its source in a mighty lake—some say two immense lakes—fed by periodical rains,—fed also, say some, by subterraneous streams flowing from the west (these subterraneous rivers were favourites with the wonder-loving naturalists of old).

This great lake was further believed to lie at the foot of lofty, snow-covered mountains, named the Mountains of the Moon. Herodotus indeed demurs to the snow. The Reservoir Lakes become immeasurable marshes in some of the accounts. Indeed I should despair of producing a *catena* of witnesses for any single point of the statement; but such as I have described was nearly the mind of ancient Greece and Rome, speaking on the information obtained in Egypt.

It is more remarkable to find a similar shadow of the truth from a different quarter, and perhaps of an earlier date. The ancient inhabitants of India seem to have felt the same interest, and to have had an equal glimmering of the course of the Nile. In a well-known paper by Mr Wilford, in the Asiatic Researches, we have a sort of abstract of the ancient Indian belief concerning the Nile, drawn from the Purānas and other Hindu or Sanscrit books.

The name of the river in those most ancient books is *Kali*, black. (Though Homer names the river *Aegyptus*, it was known to ancient Greeks as *Μελας*.) According to the same authorities, that famous and holy river takes its rise from the lake of the gods, thence named Amara or Deva, Saróvera in the region of Sharma or Sharmasthan, between the mountains of Ajagara and Sitanta, part of Soma-giri, or the Mountains of the Moon, the country round the lake being called Chandristhan or Moon-land. The Hindus believed in a range of snow-covered hills in Africa.

From thence the Kali flows into the marshes of the Padma-van, and through the Nishada Mountains into the land of Barbara; whence it passes through the mountains of Hemacáta; then entering the forests of Tapas (or Thebais) it runs into Kantaka-desa, or Miha-sthan, and through the woods emphatically named Aranya and Atavi into Sanchabdhi (or our Mediterranean).

From the country of Pushpaversha, it received the Nanda or Nile of Abyssinia, the Asthimati or smaller Krishna, which is the Takazzi or little Abay, and the Sanchanaga or Mareb.

The Ajagara Mountains, which run parallel to the eastern shores of Africa, have at present the name of Lupata, or the back-bone of the world. Those of Sitanta are the range which lies west of the lake Zambre or Zaire, words not improbably corrupted from Amara or Sura. This Lake of the Gods is believed to be a vast reservoir which, through visible or hidden channels, supplies all the rivers of the country.

The Hindus, for mythological purposes (says Mr Wilford), are fond of supposing subterranean communications between lakes and rivers, and the Greeks, we know, had the same leaning.

We really had made little progress beyond these ancient guesses, till in the year 1858 Captains Speke and Burton saw and sailed upon the great lake Tanganyika, 600 miles from the coast at Zanzibar. The lake is narrow, but 300 miles long, and 1800 feet above the level of the sea. Very soon after, Captain Speke alone had the glory to see and bear witness to the great inland sea which he has named Victoria. Having only seen this mighty lake, and being obliged to leave it unexplored, Captain Speke made haste to

return to it, and this time in company with his old comrade and brother-in-arms Captain Grant, and through toils and dangers which men like these love almost for their own sake, they, together, reached in 1861 the Victoria Lake, which Speke had discovered three years earlier.

It happened (and such coincidences are frequent in science) that at the very time when Speke and Grant were fixing the bearings and heights of the great lake and its mountains, Baron von Decken and Mr Thornton measured and estimated the altitude of *Kilima Njaro*, one of a mountain range to the eastward of our travellers' route, at 20,000 feet, while the snow line descended below 16,000.

At present our information is necessarily meagre, but on the testimony of these two veteran travellers, furnished as they were with instruments for observation, we have some actual certainty, and room for infinite speculation.

The Victoria Sea of fresh water is about 150 miles square. The equator line runs through it, though nearer its north shore. Its waters are 3563 feet above the sea level. It is skirted, if not quite surrounded, by ranges of mountains of 10,000 feet high. Without farther evidence, independent even of the high authority of Captain Speke's opinion, we receive as certain that in the Victoria great lake is the source, or rather the great reservoir of the Nile, for of course the lake is fed by numerous streams, in fact by a stream from every valley among the surrounding mountains, and then it follows that the White Nile, not the Blue Nile as Bruce believed, is the chief of the two streams that join at Kartom, lat. $15^{\circ} 30'$.

Thus was the mystery cleared up that had defeated the ingenuity and enterprise of philosophers and travellers, of kings and Cæsars, since the days of Herodotus.

Captain Speke thinks very highly of the country he has explored in a commercial and agricultural view. He found the people not all savage, but capable of intelligent interest and quite awake to kindness and friendship. But the country is everywhere thinly peopled, and productive much beyond the wants of the population. Along the equator, at heights varying from 6000 to 12,000 feet, the travellers found a delicious climate, with abundance of water, and no excessive heat, full of cattle and corn. In the kingdom of Karagwé (Lat. $1^{\circ} 40'$, elevation 5100 feet), the temperature for five months ranged from 60° to 70° at 9 morning. From what they could learn of the country to the westward of the lake, it preserves the same character for several hundred miles, and I know that Captain Speke believes there is a continuance of that which he calls the *Fertile Zone* almost to the coast of the Atlantic. He tells his friends he has "discovered a great fertile zone there, caused principally by the Mountains of the Moon, situated close to the equator, in the midst of the continent of Africa. These are great rain condensers. Round them are the sources of several rivers, the Nile on one side, the Tanganyika and the Congo on the other. The rains falling all round make that a fertile zone—the most fertile in the world. There is nothing in India or China to equal it."

It is in that direction the indefatigable traveller proposes to make his next expedition, and let us hope that in two years more we shall welcome Captain Speke returning from the mouths of the Congo.

I know not whether to congratulate or condole with the Society upon another advance in science, or whether that is to be called an advance which some consider a double trespass, a breaking down of the boundaries between geology and archæology, and overleaping the ancient landmarks which divided natural science from sacred history.

Certain well-known discoveries of hand-shaped weapons and implements, found along with the remains of some extinct animals, in undisturbed beds of a very ancient alluvial deposit both in France and in England, led the antiquary, whose department is limited to the human period, to seek to extend that period into what had hitherto been the exclusive province of the geologist; and the geologist again, driven to admit that these flint spear-points have been shaped by man's hand, and used upon (or among) the *Elephas primigenius*, the *Rhinoceros*, and other extinct animals whose teeth and bones now bear them company, has to seek for an extension of the period hitherto allotted for the operations and deposits which the race of man has witnessed.

This only brought out more palpably what geologists had for some time taught—had taught indeed almost as early as geology took the dimensions of a science—that the globe itself was immeasurably older than the age assigned for man.

That period—the creation of man—the age of man on the globe—had been early, and nearly unanimously fixed, by calculations based upon the data afforded by the Mosaic books.

Such calculations were necessarily more or less conjectural, founded on interpretations of archaic forms of language, and of words which might have different meanings. Numbers and figures were to be read in varying manuscripts, often from faulty copies; and although great men like Newton had satisfied themselves that the received age of the world and its inhabitants was the true one, new facts, of a science unknown to Newton, had shaken that opinion, and it seemed probable that the Biblical scholar, the student of sacred history, in the view of geological facts, would, in the first place, abandon the position that the age of the creation, the antiquity of the earth, was to be determined by the interpretation of the Mosaical books; and, *secondly*, that he would not shut his eyes to new evidence offered upon the questions, whether the Mosaical books intended to affirm the age of man upon the globe, and whether the interpreters of those books had accurately and precisely and definitely ascertained their meaning and intention in that matter.

I should perhaps do better in using the terms of the latest authority on this subject, which comes with "Oxford" on its title page to vouch its orthodoxy, and with the sound sense of our friend Dr Hannah to commend it to our acceptance:—*

* Dr Hannah's Bampton Lectures. Oxon., 1868.

"It is surely mere misapprehension to suppose that the revelation with which Moses was really entrusted could traverse the path of the modern geologist, or contain any thing that would either confirm or contradict his readings of those buried rocks. From whichever side the error comes, we are bound to shake ourselves free from it, not by saying with some that God cared not though His instruments should make mistakes on scientific subjects, but by pointing out that there can be no error where there is no assertion, and that a purely theological revelation contains no assertion which falls within the proper sphere of science."

I say then the two parties, the scientific inquirer and the Mosaic scholar, both earnest for truth, would have come to some understanding, not surely to conceal or shut out the truth, but to give each full license to inquire and experiment, and to draw all legitimate inferences from facts discovered; for after all, the disputes between theologians and geologists relate rather to inferences from facts than to the reality of the facts themselves. The theologian infers certain truths from the words of the first chapter of Genesis; the geologist infers certain notions from what he sees in an open quarry. The inferences are mutually contradictory; but as the theologian and the geologist are both capable of drawing false inferences, such inferences may be contradictory, and neither may be true. A new light on the meaning of the word "Day," in the Mosaic language, might end the controversy; so might some evidence that the best instances of hand-formed flint implements found in ancient drift were fictitious and fraudulent.

We must suppose that a candid student of the Divine books will take what help is in his power for explaining their difficulties, and, be sure, he will not neglect the testimony of the rocks—the history of creation written in other letters but by the same Author. So a candid geologist, who reflects that the purpose of Moses was clearly not to teach natural philosophy, but to inculcate and enforce the worship of the true God, will acknowledge that the order of creation given in Genesis does agree marvellously with the inverse order of the fossils actually found—plants, marine or aquatic animals, birds, mammals, man.

I say these disputants might have come to terms—explaining the Scripture history of the creation by the help of a careful and reverent study of the created universe. But a third party has lately rushed among the combatants, and now fight with two-edged weapons. These are theologians too—at least they are churchmen, and Hebraists, and mighty arithmeticians; but, with a singular view of their duty to their Church, they cavil at the foundations of its history and doctrine, and think it necessary to tell the world so. These critics insist, that no interpretation, construing of a phrase, word, or numeral of the Mosaic books shall be admitted—that all shall stand or fall together; and then, having picked out some words, especially some numbers, which they judge erroneous—though not affecting a single point of doctrine or morals, or the



essentials of history—they say the books ascribed to Moses are devoid of authority, and must be abandoned!

That is not the way in which we are accustomed to read any ancient history; and, though different canons are used for criticising the inspired writers from those applied to other historians, yet, as to the mere text, the books of Moses are entitled beyond others to a fair and liberal construction, as the most ancient books in the world, and as having passed through an infinite number of transcriptions and translations.

But I must declare my entire concurrence with Dr Hannah, that "it is a dangerous and mistaken policy to raise these disputes to adventitious importance, by treating them as though they necessarily involved the issue of our highest interests."

For the persons of tender conscience, who feel themselves constrained "to build up those scattered fragments of difficulty into a coherent edifice of doubt," they would themselves surely feel easier, as it would be a relief to the world, who are judging in the quarrel, if they could cease to be members of a Church which founds so confidently on the Mosaical history. They would *assail* with more satisfaction if they had not promised to *defend*.

For the geologist, if my voice were wanting to encourage him, I would bid him go forward, cautiously, reverently, yet without fear. Let him test the evidence with all care before publishing a discovery. He must consider he has everything to prove, and he should assert nothing without evidence, and take nothing for granted. We want proof of the antiquity of the Drift-deposit, and of the fossils contained in it belonging to the extinct animals named. We want proof that the flints are hand-wrought, and not chipped accidentally in the rolling drift. Much more, we desire proof that they *were* found there, and not *placed* to be found by some cunning quarryman. It is not only the flint instrument but its manufacture, its chipping into shape, that must be tested. Is the fracture of the flint such that it might have been made many thousand years ago?

Farther, the geologist should publish to the world the evidence of his facts; for the inquiry is one that concerns the public, and in which the public take an interest. But why should I intrude my advice upon men who have shown they know well what is required at their hands in a momentous inquiry? Nine of the most eminent geologists of France and England met in friendly conference at Paris, and, later, at Abbeville, to compare specimens, to test the evidence, to do everything for ascertaining the truth; and they published the *procès-verbal* of their proceedings in the "Natural History Review" of last August, with the sanction of Dr Falconer's name, and others equally well known. It seems hardly to be doubted, that numerous frauds have been perpetrated upon the naturalists. When specimens are well paid for, they become plentiful, both in England and in France, but there may be means of detecting the impositions, and these means our geologists are using with all care. The iron horse-shoe, lately put forth among the primeval relics, has

been, as I understand, withdrawn; the bones of elephant and other animals, bearing marks of human hands, are not yet accepted by these naturalists. As to the Abbeville jaw-bone of a man, whose jaw must have ceased chewing long before the flood, there is but one opinion in England, which I am informed by Mr Evans is also gaining ground in France—that the whole thing was an impudent imposition. Mr Prestwich, who was once a believer, published his recantation in the last Quarterly Journal of the Geological Society.

It may be permitted me, perhaps, as one of the public, to offer one more advice to the naturalist. He must take care not only that his reasoning is logical, his inferences cautious and careful, but he will do well to avoid even the appearance of disputing for victory. Science has no enemies if its votaries do not raise them up by indiscretion and intemperance.

Monday, 21st December 1863.—DR CHRISTISON, V.P.,
in the Chair.

The following Communications were read:—

1. On the Morphological Relationships of the Molluscoida and Coelenterata and of their leading members, *inter se*. By John Denis Macdonald, R.N., F.R.S., Surgeon of H.M.S. "Icarus." Communicated by Professor MacLagan.
2. On the External Anatomy of a New Mediterranean Pteropod. By John Denis Macdonald, R.N. Communicated by Professor MacLagan.
3. On the Limits of our Knowledge respecting the Theory of Parallels. By Professor Kelland.

The Author has in this paper traced to its consequences the assumption, as if it were an axiom, of the proposition "That the angles of a triangle are together less than two right angles." The results as regards the theory of parallels are such as to imply that such lines would have most of the properties of equal circles exterior to one another.

Professor Tait reminded the Society that, at the close of last session, he and Balfour Stewart, F.R.S., of the Kew Observatory, had deposited with the Secretary a sealed packet containing the coincident results of certain investigations which they had separately carried on from totally distinct points of view, and which appeared to lead to a new principle in Natural Philosophy.

Experimental attempts at verifications of this principle have since been made by them in various ways, and others are in progress. Meanwhile, the authors desire to put on record that it appears probable, from their experiments, that the viscosity, &c., of

air are not the only causes of the increased radiation from a moving body. (Compare Joule and Thomson, *Phil. Trans.* 1860.) A vacuum apparatus now in course of construction, will, it is hoped, lead to decisive results.

Monday, 4th January 1864.—PROFESSOR KELLAND, V.P.,
in the Chair.

The following Communications were read:—

1. On the great Drift-Beds with Shells in the South-west of Arran. By the Rev. Robert Boog Watson, B.A., F.R.S.E., Hon. Mem. Naturw. Ver. Lüneburg.

These beds, as examined by the author, lie in the Torlin or Kil-morie Burn basin, in the Scoradale or Slidry Water basin (more strictly in the first north or north-west tributary of each, reckoning upwards from the sea), and in the Clachan Glen,—all in the south-west of Arran. They are of great extent and depth; at certain points they contain boreal shells in considerable numbers. They are divisible into two classes, (1.) underlying fine dark sands and clays; and, (2.) overlying coarse red clay with striated stones, probably boulder clay.

They are interesting, because,

1. They present, in a striking form, proof of the immense destruction of the surface of the land.
2. They afford unusually good sections, from the rock on which they rest, upwards.
3. They throw some light on the formation of the boulder clay.
4. They present sea shells, at one point land plants, and also at one point a later lake basin.

The special information they give is,—

1. That *all* the latest geological changes have not materially affected the relations of hill and valley.
2. That the valleys were largely excavated by ice.
3. That the ice covered the land till it was submerged.
4. That the depression of the land below the sea was continuous, and ultimately attained 1000 feet at least.
5. That the depression was, at one point at least, sudden.
6. That this sudden fall did not begin later, at least, than the time at which the present 90 feet line above the sea-level reached the level of the sea.
7. That this sudden subsidence could not have amounted to less than 200 feet.
8. That it could not have much exceeded 300 feet.
9. That under obvious limitations, the beds which lie *nearest the sea-level and deepest below the surface*, are the *oldest*, and that those are *contemporary* which occupy the *same relative position to the sea-level and the underlying rock*.

2. On the Agrarian Law of Lycurgus, and one of Mr Grote's Canons of Historical Criticism. By Professor Blackie.
3. On the Occurrence of Amœbiform Protoplasm and the Emission of Pseudopodia in the Hydroida. By Professor Allman.

The author described the contents of the small tubular appendages, named Nematophores by Busk, which are developed upon certain definite points of the hydrosome in the *Plumularide*. These contents were shown to consist of a granular protoplasm, with occasionally a cluster of large thread-cells embedded in it.

The protoplasm has the property of emitting pseudopodia, which are very extensile and mutable in shape, and exactly resemble the pseudopodial prolongations, whose occurrence among the *Rhizopoda* is so eminently characteristic of this group of *Protozoa*. The contents of the nematophores, indeed, except alone in the presence of thread-cells, are indistinguishable in structure, and in the phenomena presented by them from the sarcode or protoplasm, which forms the substance of an amœba, a diffugia, or an arcella.

Monday, 18th January 1864.—His Grace the DUKE of ARGYLL, President, in the Chair.

The following Communications were read:—

1. Description of the Lithoscope, an instrument for distinguishing Precious Stones and other bodies. By Sir David Brewster, K.H.

The Instrument was exhibited.

2. On the Temperature of certain Hot Springs in the Pyrenees. By R. E. SCORESBY-JACKSON, M.D., F.R.C.P.

In the year 1835, Principal Forbes very carefully observed the temperatures of certain springs in the Pyrenees, with the view of ascertaining to what extent changes of temperature occur in them. Observations previously made were, for several reasons, of uncertain worth, and Principal Forbes was desirous of then fixing "data for future observers with a degree of accuracy hitherto unattempted."

The author having determined to spend his autumn holidays in the Pyrenees, believed that a careful repetition of such observations, after the lapse of twenty-eight years, would afford results of some interest. He furnished himself with accurate thermometers

made for the purpose, and, during the month of August, carefully observed the temperatures of several of the springs visited by Principal Forbes in 1835. In the tables which are distributed through the paper, the temperatures of the springs are given as recorded by different observers at various periods between the years 1835 and 1863. From these records it would appear that whilst there is perhaps in no instance a general or permanent change of temperature, neither is there in any an undeviating temperature. It is probable that the temperatures of the springs in the interior of the globe have undergone no change, and that the changes observable upon the surface of the earth are due to superficial causes, such as external temperature, the infiltration of cold surface water, &c. To a certain extent, an allowance must be made for inaccuracies; for it is scarcely to be supposed that all the observers dipped their thermometers exactly at the same points, nor does the author know that in all cases the instruments employed were without errors.

3. On Human Crania allied in Anatomical Characters to the Engis and Neanderthal Skulls. By Wm. Turner, M.B., Senior Demonstrator of Anatomy in the University.

The Author compared the above crania with various human skulls which had come under his observation. He exhibited a skull brought by Mr Henry Duckworth, F.G.S., from St Acheul near Amiens, which in its general contour presented a striking resemblance to the Engis skull. The St Acheul skull was somewhat smaller, being probably that of a female. It might almost have been regarded as a reduced copy of the Engis skull. There was no evidence that the skull from St Acheul was of an earlier date than the Gallo-Roman period of French history. The Neanderthal skull was compared with several modern crania, mostly British, especially with reference to the projection of the supra-orbital ridges, the retreating forehead, and the slight convexity of the occipital region. He exhibited several skulls which were closely allied to it in one or other of these features. It was shown also that the Neanderthal skull, although below the European mean in its internal capacity, yet exceeded the dimensions of some normal modern European crania which had been carefully measured—its large transverse parietal diameter compensating for the brain space lost by the retreating forehead and flattened occiput.

As the history and geological age of the Neanderthal skull were both unknown, and as many of its most striking anatomical characters were closely paralleled in some modern European crania, the Author considered that great caution ought to be exercised in coming to any conclusion, either as to the pithecoïd affinities or psychical endowments of the man to whom it originally appertained.

4. Notice of a Simple Method of Approximating to the Roots of any Algebraic Equation. By Edward Sang, Esq.
5. Notice of the State of the Open-Air Vegetation in the Edinburgh Botanic Garden, during December 1863. By J. H. Balfour, A.M., M.D., F.R.S., F.L.S.

The state of the vegetation in the open ground of the Botanic Garden during the month of December 1863 was so very remarkable that I have been induced to submit a notice of it to the Royal Society. The number of phanerogamous species and varieties in flower during the month amounted to 245; of these 35 were spring-flowering plants which had anticipated their period of florescence, while the rest were summer and autumn flowers which had protracted their flowering beyond their usual limits.

The following are the details as given by Mr M'Nab:—

Plants in flower in the Royal Botanic Garden, Edinburgh, from 1st to 31st December 1863.

Annual plants, chiefly summer and autumn flowering species and varieties, ...	36
Perennial plants, chiefly summer and autumn flowering species and varieties, ...	133
Trees and shrubs, chiefly autumn flowering, ...	41
Spring-flowering trees, shrubs, and perennial herbaceous plants, ...	35
Total flowering plants, ...	245

The state of vegetation in December was much influenced by the nature of the weather during the preceding months of October and November. No marked check was given to it during these months, and the temperature was such as to stimulate the action of the cells and vessels of the plants. In December the comparatively high temperature continued. I have asked my friend, Mr Alexander Buchan, Secretary of the Scottish Meteorological Society, to draw up for me a tabular view of the temperature of the three months during the last seven years. I have supplied the lowest daily temperatures, as observed in the Botanic Garden; and for want of sufficient data in the garden, he has selected for the other data a station in Fife (Balfour) where the temperatures in general resemble much those noticed at the Botanic Garden. (I hope to be able to make arrangements in future for a full series of thermometrical, barometrical, and hygrometrical observations being made at the Botanic Garden.)

1. *Mean Temperatures at Balfour, in Fife, in the months of October, November, and December, during seven years ending with 1863. A comparison of 1863 with the Means of the previous six years is also given.*

YEAR.	OCTOBER.			NOVEMBER.			DECEMBER.		
	Mean of Day.	Mean of Night.	Mean Temp.	Mean of Day.	Mean of Night.	Mean Temp.	Mean of Day.	Mean of Night.	Mean Temp.
1857	54.0	41.8	47.9	48.1	36.0	42.0	48.7	39.0	43.8
1858	50.8	38.3	44.5	45.0	33.8	39.4	43.3	33.9	38.6
1859	51.7	39.9	45.8	42.8	32.7	37.8	38.1	30.8	34.4
1860	52.7	42.1	47.4	42.5	37.6	40.0	36.3	29.0	32.6
1861	55.4	43.7	49.6	43.7	34.0	38.8	41.2	32.8	37.0
1862	54.3	41.1	47.7	42.9	30.6	36.8	46.2	38.6	42.4
Mean of 1857-62	53.2	41.1	47.2	44.2	34.1	39.1	42.3	34.0	38.1
1863	53.6	42.5	48.0	49.4	37.3	43.4	46.6	36.5	41.6
Excess of 1863 over the Mean of the previous 6 years	0.4	1.4	0.8	5.2	3.2	4.2	4.3	2.5	3.5

- II. *The Highest Temperature of the Day observed at Balfour, in Fife, in each week of the three months October, November, and December, during seven years ending with 1863.*

YEAR.	OCTOBER.					NOVEMBER.					DECEMBER.					Oct.	Nov.	Dec.
	1	8	15	22	29	5	12	19	26	3	10	17	24	Oct.	Nov.			
1857	63.0	56.0	59.5	56.0	57.0	51.0	51.5	52.5	54.0	56.0	50.5	53.5	51.5	63.0	57.0	56.0		
1858	61.0	59.0	51.5	54.0	52.0	47.0	46.0	46.0	56.0	52.0	45.0	50.5	46.5	61.0	56.0	54.0		
1859	65.6	61.1	57.6	39.6	45.9	51.6	47.9	46.4	45.6	46.5	45.4	40.1	43.6	65.6	51.6	46.6		
1860	60.1	56.6	55.6	59.1	48.6	47.6	46.6	45.1	45.6	45.1	44.6	37.6	36.1	60.1	48.1	45.6		
1861	59.6	63.6	56.1	59.6	50.6	49.6	44.6	51.6	54.6	44.6	54.1	47.6	41.6	63.6	54.6	54.6		
1862	65.6	65.1	59.6	55.6	54.6	47.6	48.1	43.1	44.1	52.6	49.6	51.6	49.6	65.6	54.6	52.6		
1863	61.0	57.0	56.0	58.0	55.0	47.5	56.0	57.0	58.0	52.0	53.0	56.0	50.5	61.0	58.0	55.0		

- III. *The Lowest Temperature of the Night observed at Balfour, in Fife, in each week of the three months October, November, and December, during seven years ending with 1863.*

YEAR.	OCTOBER.					NOVEMBER.					DECEMBER.					Oct.	Nov.	Dec.
	1	8	15	22	29	5	12	19	26	3	10	17	24	Oct.	Nov.			
1857	31.0	44.0	42.0	31.0	31.0	32.5	33.0	36.0	36.0	36.0	36.0	33.0	28.0	31.0	36.0	36.0		
1858	36.0	34.5	33.0	30.0	30.0	31.5	28.0	22.0	36.0	36.5	26.0	31.5	31.0	30.0	22.0	25.0		
1859	41.3	18.5	29.5	24.5	24.3	24.5	27.5	29.5	28.5	26.5	23.5	15.0	26.5	24.3	24.5	15.0		
1860	41.5	30.5	40.0	39.0	37.5	38.5	37.5	36.5	30.0	39.5	30.5	18.0	1.5	30.5	26.5	1.5		
1861	36.0	37.5	34.0	35.5	31.5	25.5	23.5	23.5	25.5	29.0	31.5	26.5	22.5	23.5	23.5	23.5		
1862	34.5	37.0	36.5	35.0	29.0	27.0	30.5	20.5	22.0	35.6	33.0	34.5	31.5	29.0	20.5	31.5		
1863	29.5	45.5	40.0	34.0	28.5	26.5	32.5	35.0	30.0	31.0	38.5	30.0	23.5	29.5	26.5	28.5		

IV. *Number of Nights each Week on which the Thermometer, exposed in the Botanic Garden, four feet above the ground, fell to freezing (32°0). An asterisk (*) is put to indicate the nights on which it fell to at least 6°0 below freezing.*

YEAR.	OCTOBER.					NOVEMBER.				DECEMBER.			
	1	8	15	22	29	5	12	19	26	3	10	17	24
1857	1		1				1	3	2				2
1858				1		5	5	6		2	5	1	
1859			2	5	3	2	4	3	5	3	5	6	6
1860		2			3		4	2	2		3	7	7
1861			2		3	5	3	3	2	1	1	4	7
1862					2	5	6	7	5			1	
1863	1			2	1	1	1	5	3			3	5
Lowest each week } in 1863, . . . }	28·0	40·0	35·0	31·0	26·0	23·5	7·0	32·0	26·0	30·0	35·0	25·0	19·0

Report on the Weather of October, November, and December 1863, as compared with the previous Six Years. By MR ALEXANDER BUCHAN.

The first three tables present a detailed statement of the temperature in October, November, and December, during the seven years ending with 1863, as observed at Balfour, near Markinch, in Fife, one of the stations of the Scottish Meteorological Society. It is the nearest station to Edinburgh at which full and well-authenticated observations on temperature have been made for so long a period, and, besides, its position is such as to represent fairly both sides of the Forth.

Table I. gives the mean monthly temperature of the day and of the night, and the mean temperature of these months for the past seven years, and a comparison of 1863 with the means of the previous six years. From this Table, we learn that the peculiar features of the weather of October, November, and December last, as respects temperature, were as follow :—

In *October*, the mean temperature was nearly a degree (0°·8) above the average of the month ; but whilst the mean temperature of the day was less than half a degree (0°·4), that of the night was about a degree and a-half (1°·4) above the average, thus indicating a cloudy sky and comparative absence of frost. At the Botanic Garden, the thermometer fell only three times to freezing, the lowest being 28°·0 on the night of the 6th. This frost continued but for a short time, and very little damage was done except to *Heliotropes*; dahlias were only slightly affected.

In *November*, the mean temperature was $4^{\circ}2$ above the average, which increase was very unequally distributed between day and night,—the mean temperature of the day being $5^{\circ}2$, and of the night only $3^{\circ}2$ above the mean of the month. This temperature is not only greatly above the average, but it is also about a degree and a-half higher than any previously recorded November, and $6^{\circ}6$ higher than the November of 1862.

In *December*, the mean temperature was $3^{\circ}5$ above the average, and the manner of its distribution between day and night similar to November,—the mean of the day being $4^{\circ}2$, and of the night $2^{\circ}5$ above the average.

Hence the characteristic feature of the weather of this period is the unprecedentedly high temperature during the day in November and December,—a point to which special attention is directed.

Table II. gives the highest temperature of the day, and Table III. the lowest temperature of the night, in each week of the period under consideration.

Table IV. gives the number of nights each week on which the temperature at the Botanic Garden fell to freezing or lower, and an asterisk is put to mark those cases when it fell to at least $6^{\circ}0$ below freezing.

These tables furnish the data from which an explanation may be had of the remarkable vegetation of December last, in so far as that depended on the character of the then current weather. The explanation is twofold—*first*, the high temperature during the day in November and December; and, *secondly*, the comparative absence of frost during the night.

This remarkably high temperature was preceded by a period of cold weather, extending from the 29th of October to the 12th of November, during which frosts were of frequent occurrence. At Balfour the thermometer, four feet above the ground, and protected, fell to $26^{\circ}5$; and at the Botanic Garden, four feet above the ground, but exposed, it fell to $23^{\circ}5$, and indicated freezing on ten nights. Though dahlias and other plants were destroyed, yet many survived, owing, it is supposed, to the remarkably dry state of the weather, and to the very brief periods during which the severity of the cold in each instance lasted. This cold period also contributed to the remarkable growth which followed, since by playing the part of winter, though in a modified degree, it arrested the vital functions, and gave plants the benefit of a fresh start with the warmth which succeeded.

The unprecedentedly high temperature began on the 13th of November, and continued with scarcely any interruption till Christmas: see Table II. Of this period the warmest part extended from the 13th of November to the end of the month, during which the mean temperature of the day was $52^{\circ}5$, or $9^{\circ}0$ above the average. This day temperature usually prevails about the end of April or beginning of May, when the temperature of the night is much lower than obtained in November last.

In the end of November and beginning of December (see Tables

III. and IV.), the temperature of the night declined occasionally to freezing. At Balfour it fell to $30^{\circ}0$, and at the Botanic Garden (exposed) to $26^{\circ}0$,—a degree of frost insufficient to damage those autumn flowers which had stood the more severe frost in the beginning of November, or check the growth of the spring flowers rapidly coming into bloom.

This anomalous weather sufficiently accounts for the strange spectacle of *sweet peas* and *Hepaticas* blooming together.

Monday, 15th February 1864.—DR CHRISTISON, V.P.,
in the Chair.

The following Communications were read :—

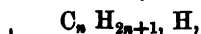
1. On the Influence of the Refracting Force of Calcareous Spar on the Polarization, the Intensity, and the Colour of the Light which it Reflects. By Sir David Brewster, K.H., F.R.S.

In the "Philosophical Transactions" for 1819, the author had shown that the doubly refracting force of calcareous spar extended beyond the sphere of the reflecting force, producing a change in the polarising angle varying with the inclination of the incident ray to the axis of the crystal, and producing a deviation of the plane of polarisation from the plane of incidence and reflexion, when the reflecting force of the crystal was reduced by contact with oil of cassia and other oils. These experiments were made on the face of the primitive rhomb.

In the present paper, the author gives an account of the results which he obtained upon other natural and artificial faces of calcareous spar, inclined 0° , $5\frac{1}{2}^{\circ}$, 12° , $22\frac{1}{2}^{\circ}$, $67\frac{1}{2}^{\circ}$, and 90° , to the axis of the crystal. On all these surfaces, when the reflecting force is reduced by contact with oil of cassia or other oils and fluids, the intensity and colour of the reflected pencil, and the deviation of the plane of polarisation from the plane of reflection, experiences remarkable changes, depending on the inclination of the incident ray to the axis of double refraction.

2. On the Most Volatile Constituents of American Petroleum. By Edmund Ronalds, Ph.D.

It was shown by this paper that the gases dissolved in American petroleum, and which gave to it such a high degree of inflammability, were composed of the lower members of the marsh gas series, having the general formula,



and to which the liquid products have already been referred.

The gases evolved from the Pennsylvanian oil were collected at a temperature of -1° Cent., as they floated, mixed with air, over the

surface of the liquid in the casks in which it is imported into this country and the hydrocarbons were shown by eudiometrical analysis to have the composition of a mixture in nearly equal proportions of the hydrides of ethyl and propyl.

The first portions of incondensable gas evolved on warming the most volatile product of the distillation of petroleum on a manufacturing scale were also found to contain a mixture of these hydrides, while portions of gas collected at a later period of the operation approached more closely to the composition of pure hydride of propyl, or were mixtures of the hydrides of propyl and butyl; the last gas collected being nearly pure hydride of butyl.

The liquid condensed by a mixture of ice and salt during the collection of these gases gave, upon redistillation, a considerable portion boiling between 0° and 4° Cent.; this, as well as that which passed over as high as 6° Cent., was shown by analysis to be nearly pure hydride of butyl having the composition C_4H_{10} , H.

This liquid has a specific gravity of 0.600 at 0° Cent.; it is consequently the lightest liquid known. Its vapour density was by experiment found to be 2.11. It is colourless, possesses a sweet, agreeable smell, is soluble in alcohol and ether, but not in water. Alcohol of 98 per cent. dissolves between 11 and 12 times its volume of the gas at 21° Cent. The liquid and the gas are not preceptibly affected by sulphuric or nitric acid, nor by bromine; mixed with twice its volume of chlorine in diffuse daylight, the gas is converted into liquid chloride of butyl, while the original three volumes become condensed into two volumes of hydrochloric acid.

3.—On the Action of Tetrachloride of Phosphorus on Aniline. By Magnus M. Tait, F.C.S.

More than a year since my attention was directed to a statement of Hofmann's, that the action of tetrachloride of phosphorus on aniline yielded a white substance of crystalline character, as the investigation of this compound so produced seemed likely to be of interest, I began its examination, but circumstances prevented me from completing it at that time. The publication of Schiff's papers, however, on the metal anilides again drew my notice to the subject, and I considered it a duty to myself to publish the results of my experiments, more especially as the reaction which forms the subject of this paper appears to have escaped the attention of that chemist.

Tetrachloride of phosphorus was added, drop by drop, to the aniline, which required to be kept cool by ice, as the reaction tends to be of a rather violent character, great heat being produced. In a short time the whole solidified into a soft granular mass, which dissolved readily in water, alcohol, and ether. The mass was dissolved in hot water, and, on cooling, the excess of aniline rose to the surface as an oily layer, and was separated by passing it through a moistened filter. The watery solution was evaporated at ordinary temperatures over sulphuric acid, and when it had reached a syrupy

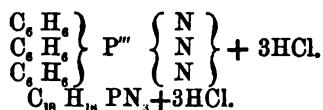
consistence it slowly solidified into a mass of fine needle-shaped crystals, which were the hydrochlorate of a new base, to which I give the name of Phosphaniline.

The crystals were well pressed between folds of filter paper, and then being placed on a filter, were washed with a very small quantity of alcohol and ether; again dissolved, and evaporated as before, the crystals were pure. The substance so obtained dissolves easily in water, alcohol and ether, and is neutral to test-papers. Gently heated, it sublimes, and gives fine prismatic crystals. Treated with solution of potash it is decomposed. Strong sulphuric acid expels hydrochloric acid, and gives a colourless solution. Nitric acid oxidises it, and gives a coloured solution.

On analysis, it yielded the following results:—

	I.	II.	Theory.
Carbon, . . .	45.12	...	44.63
Hydrogen, . . .	4.94	...	4.84
Phosphorus, . . .	...	...	
Nitrogen, . . .	...	...	
Chlorine, . . .	24.82	25.89	25.40

This analysis shows that the substance is produced by the direct union of the chloride of phosphorus and aniline. It is, however, a hydrochlorate, and is formed from three equivalents of aniline, in which three equivalents of hydrogen are replaced by phosphorus: thus—

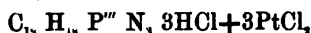


Platinochloride of Phosphaniline.—A portion of the original salt was dissolved in water, the solution acidified with hydrochloric acid, and bichloride of platinum added, in a short time crystals began to appear; these, after a sufficient quantity had formed, were placed on a filter and washed with a small quantity of alcohol and ether, and dried over strong sulphuric acid, until their weight was constant. They were in the shape of small granular crystals of a light yellow colour, soluble in alcohol and water, but not in ether. On analysis, the following numbers were obtained:—

	I.	II.	III.	Theory.
Carbon, . . .	28.52	...	...	28.33
Hydrogen, . . .	2.40	...	...	2.85
Phosphorus, . . .	...	...	...	
Chlorine, . . .	...	...	...	
Nitrogen, . . .	...	...	...	
Platinum, . . .	...	32.23	32.78	31.98

The platinum in the third column was estimated by direct ignition, the high result obtained was probably owing to the formation of a little phosphide of platinum.

The analysis corresponds with the following formula:—

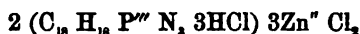


Zincchloride of Phosphaniline.—Hydrochloric acid was added to a portion of the solution of the hydrochlorate in water, and then some fragments of pure zinc were thrown in. After the zinc had dissolved the fluid was evaporated at a gentle heat (about 200° Fahr.), and filtered from a few greenish flakes which had formed, as even that low temperature appears to decompose the salt. The clear fluid was then put over sulphuric acid and left for some days, when crystals of a zinc salt were obtained, but under the same conditions as the hydrochlorate, as the solution was very concentrated before the salt appeared, and then it solidified completely. The crystals were pressed between folds of filter paper, and washed with a mixture of alcohol and ether, and dried in vacuo over strong sulphuric acid. They were white needles, slightly deliquescent, and soluble in alcohol. In ether it does not dissolve, but it becomes liquid, having the same appearance as a drop of oil in water.

The chlorine only was estimated, the analysis gave—

	I.	Theory.
Chlorine, ...	84.57	84.28

which agrees with the following formula :—



Bromine water immediately precipitates a brownish-coloured substance from an aqueous solution of the hydrochlorate. This precipitate was washed with water, and a portion of it boiled for some time, and found to be insoluble in water; but a substance appeared to have volatilized along with the vapour of the water, as the neck of the flask and a glass rod which was held over the mouth of it were covered with white feathery crystals. The fluid was filtered, and, after drying the brown residue, it was put into a beaker covered with filter-paper, and left over a water-bath, when it nearly all sublimed in crystals corresponding to those obtained when attempting to dissolve it. A few of these crystals were dissolved in alcohol and bichloride of platinum added, but no precipitate was formed, and on evaporation the original substance crystallized out. These circumstances indicated the body to be Tribromaniline. The filtrate from the brown substance obtained originally was treated with bichloride of platinum also, but no precipitate forming, it was presumed no bromaniline or bibromaniline had been formed.

Cadmium Salt.—Chloride of cadmium gives, with strong solutions of the hydrochlorate, scaly crystals of a double salt, moderately soluble in water.

Copper Salt.—On adding chloride of copper to a solution of the hydrochlorate, and evaporating over sulphuric acid, small granular crystals of a beautiful green colour are obtained.

Mercury Salt.—If a strong solution of chloride of mercury is added to a concentrated solution of the hydrochlorate, beautiful white scaly crystals precipitate out immediately; but if the solu-

tions are dilute no precipitation takes place, if the solution is now warmed, a crystalline substance is thrown down. This appears the more curious, when it is known that the other salts cannot be heated without decomposition.

When the hydrochlorate is heated with potash the phosphaniline, at the moment of separation, appears to undergo decomposition, for the smell of aniline is apparent even in the cold, but no precipitation takes place, so that phosphaniline must be itself soluble in water. An attempt was made to obtain it in the separate state by acting on the hydrochlorate with oxide of silver, a precipitate of chloride of silver was formed immediately. The filtered fluid was alkaline to test paper; it clearly contained phosphaniline, but on evaporating the fluid it became coloured, owing to the decomposition of the base, which is very changeable, and cannot be obtained in the pure state.

4. On Fermat's Theorem. By Professor Tait.

The author stated that in consequence of Legendre's work, the proof of Fermat's Theorem is reducible to showing the impossibility of

$$x^m = y^m + z^m,$$

when m is an *odd prime*, x, y, z being integers.

Talbot has shown that in this case x, y, z are necessarily *composite* numbers.

The author shows, among other results of very elementary processes, that if numbers *can* be found to satisfy the above equation, x and y leave the remainder 1 when divided by m ; and that z has m as a factor. Many farther limitations are given on possible values of x, y, z —the process being based on the consideration of their prime factors, and on Fermat's Elementary Theorem $N^m - N = Nm$.

5. Professor Archer called attention to a curious binocular telescope, bearing the following inscription :—

PETRVS PETRONVS
SAC : OÆS^æ ET CAT^æ
MAIES^{is} OPTICVS
MEDLANI 1726

The instrument belongs to the Royal Institution of Liverpool, and is supposed to have been part of a collection of rarities, made by Wm. Roscoe, in Italy. As a telescope, it is of great power; the focus is adjusted by one portion of the case acting as a draw-tube within the other part.

Royal Physical Society.

Thursday, 25th November 1863.—JAMES M'BAIN, M.D., R.N., President, in the Chair.

The PRESIDENT delivered an Opening Address "On the Theory of a Central Heat."

The following Communication was read :—

On the Strata discovered in making the East of Fife Extension Railway; with Special Remarks on the Brick-Clay Beds at Elie, Fife. By the Rev. WALTER WOOD, Elie.

Wednesday, 23d December 1863.—JAMES M'BAIN, M.D., R.N., President, in the Chair.

The following Communication was read :—

On the Central Heat of the Earth. By Dr STEVENSON MACADAM, Ph.D., &c., Lecturer on Chemistry.

Dr J. A. SMITH Exhibited a male and female of the *Anser Egyptiacus*, the Egyptian Goose, recently shot near Dunbar.

Wednesday, 27th January 1864.—WILLIAM TURNER, Esq., M.B., President, in the Chair.

The following Communications were read :—

I.—*Deductions from the Hypothesis of the Internal Fluidity of the Earth.* By WILLIAM STEVENSON, Esq., Dunse.

II.—*Remarks on Dr Macadam's "Spheroidal Theory" of the Interior of the Earth.* By T. STRETHILL WRIGHT, M.D.

Wednesday, 24th February 1864.—THOMAS STRETHILL WRIGHT, M.D., President, in the Chair.

The following Communications were read :—

I.—*A Wernerian Examination of the Six Points of Pluto-Huttonism.* By Professor W. MACDONALD, St. Andrews.

II.—*Some Objections to the Nebulo-Geological Hypothesis, as stated in Dr James M'Bain's Opening Address to the Royal Physical Society.* By PATRICK MACFARLANE, Esq., Comrie.

III.—*On the Irregularities of the Earth's Surface, and the probable Mean Line of the Terraqueous Circumference.* By WILLIAM RHIND, Esq.

The periphery of the earth's surface consists of land and water at different levels, and the question may be suggested—whether is the surface of the ocean or the mean level of the land the true line of the earth's circumference, or in other words, what is really the medium line of the irregularities of the earth's surface?

Though our knowledge of the sea-bottom is yet very limited, yet the investigations of recent years have added very considerably to this knowledge. The soundings of Sir James Ross in various parts of the ocean

have disclosed interesting facts concerning its depth and temperature ; these were followed by other British and American navigators, and the whole has been collected and published by the labours of Lieutenant Maury. From these and other researches we find that the earth's superficies both under the ocean and above it presents an exceedingly irregular form, consisting of a series of elevations and depressions. On this irregular surface are diffused the waters of the ocean, spreading over and concealing from view more than two-thirds of the superficies, while the higher portions only, amounting to somewhat less than one-third, appear as dry land. The greater amount of soundings have been made in the Atlantic Ocean, and thus we have become better acquainted with its bottom than with that of the other oceans of the globe. A section from the Cape de Verd Islands, on the coast of Africa, to the mainland of South America, gives soundings of 17,000 and 22,800 feet. A section south of Newfoundland gives the greatest depth yet authentically ascertained as 27,180 feet. Further north in the line of the late electric telegraph from Ireland to Newfoundland, the depths are 10,000, 11,000 and 12,000 feet. On taking a mean of twenty-seven soundings in various parts of the Atlantic, and rejecting a few doubtful ones, the mean depth of this ocean is indicated as 13,100 feet, or two and a half miles. Soundings in the Pacific, also, indicate depths equal to the above in some positions, but the probability is that the central portions of that vast ocean, occupied by extensive coral reefs and innumerable islands, are of less depth than the mean of the Atlantic. The Mediterranean exhibits depths of 5000, 10,000, and south west of Malta, 15,000 feet. The North Sea is shallow. On the whole an approximate mean of the ocean depth may be estimated at two miles. Now, if we turn to the elevations of dry land we find that a few mountain peaks attain heights equal, if not surpassing, the extreme depressions of the ocean, in the Andes, 20,000 to 23,000 ; in the Himalaya, Kin-Kinchunga, 28,000 feet ; and Mount Everest, 29,000 feet. But the mean elevation of land is far inferior to the mean depth of ocean. According to Humboldt's calculations, were the whole surfaces of the continents of Asia, America, and Europe reduced to a uniform level, that mean level would stand at only 1000 feet above the sea-level. From recent explorations of Africa, by Beke, Livingstone, and Speke, we find that extensive table lands of 2000 to 3000 feet occupy the central portions. Some mountain peaks attain a height of 20,000 feet, and these, contrasted with the vast level deserts and low lying river valleys and shores, would seem to indicate not a higher general level for Africa than that of the other continents. Similar recent explorations in Australia also indicate that that region may be also comprehended in a general mean elevation of 1000 feet of the whole dry land of the globe. We thus find, that while the extremes of elevation of land about equal the extreme depressions of ocean, the mean depth of the ocean is 10,560 feet, or two miles, while the mean elevation of land is only 1000 feet. If we add these two means together, we have 11,560 feet as the mean of the irregularities of the earth's surface. Now, it will be perceived that the ocean surface does not stand at the mean or half section of these irregularities, but on the contrary, stands at the base of the dry land, or 1000 feet from its mean upper surface, and nearly 5000 feet above the line which we have indicated as the mean of the earth's irregularities ; for if the ocean were entirely wanting, the line would, in reality, be the mean line of the earth's irregularities, and consequently the true circumference and central line of gravity regulating the invariable diurnal revolution of the spheroid. Or to extend the expression of the formula used by Humboldt, if the whole irregularities of the earth's surface were levelled down to

one uniformity, the earth's circumference line would occupy a position nearly 5000 feet below the present ocean level. But as water is only about half the specific gravity of the materials composing the superficial strata of the earth, twice the volume of water is thus necessary to fill up the depressions and to bring about that equilibrium which is required. In looking, therefore, at a section of the earth's surface, constructed according to the above measurements, it will at once be seen how small a proportion the mean elevation of land bears to the mean depth of the ocean—that while the extremes of both are nearly equal, the extreme elevation of mountains equalling the extreme depressions of ocean—the ocean surface occupies a level half way between these extremes, and thus becomes the actual line of circumference. But then its surface is thus raised from the true central line of the earth's inequalities, in consequence of the less relative specific gravity which water bears to the superficial strata of the earth's crust. There would appear also to be this general arrangement in the seeming irregularities of the earth's surface, that the depressions of the sea bottoms accompany and compensate the elevations of continents, and thus preserve the due equilibrium of the rotating spheroid. Thus the Atlantic is a great hollow basin between the elevated continents of Asia and Africa on the east and America on the west; while the deepest portions of the Pacific are on the west side of the American continent, and its central portions are comparatively shallow. Even in inland seas, the depths of the Mediterranean, ranging from 5000 to 15,000 feet, correspond with the elevations of Mont Blanc and the Alpine range, while the shallow Baltic and North Sea are surrounded by lands of no great elevation. We thus find in the great operations of nature that adaptation of means to ends which pervades the whole works of creation, and which are as perfect in the arrangements of the mechanism of worlds as in the minutest objects which exist on their surfaces.

Botanical Society of Edinburgh.

Thursday, 14th January 1864.—Professor BALFOUR, President,
in the Chair.

The following Communications were read :—

- I. *New Researches on Hybridity in Plants.* By M. CH. NAUDIN. Part I. Translated from the French, and communicated by Mr GEORGE M. LOWE.

(This paper appears in the present number of the Journal.)

- II. *Letter from ROBERT BROWN, Esq., Botanist to the British Columbia Association.* Communicated by Professor BALFOUR.

Valley of the Sea-haast Indians, Barclay Sound,
Vancouver Island,
Lat. 48° 47' 28", Nov. 4, 1863.

Though I will be in Victoria about ten days after this date, when I will write you a full account of my transactions for the last three months, yet the politeness of the master of a trading schooner enables me to save a mail, and inform you that I am still in life and at work. Since I last wrote I have been to Washington Territory, U. S., British Columbia, and I have (first of white men) reached the head of the "great" central lake of Vancouver Island—that *ignis fatuus* of the local geographers of this far western portion of her Majesty's dominions. I have obtained seeds of between one and two hundred species of plants (in almost

every case in quantities sufficient to allow of a complete division), including some very pretty and previously unintroducted species of herbaceous plants, shrubs, and forest trees. Among the last are some good coniferæ—including *Juniperus*, three species—one, a large tree, *Taxus*, n. sp. : fine lot of the rare *Cupressus nutkanus*, Lamb; *Thuja Craigana*; a large quantity of *Abies Bridgei*, not yet introduced into England; about 100 ounces of the finest seed of *Abies Douglasii*; *Pinus*, species undescribed; *Abies grandis*, Dougl.; 20 or 30 ounces *Abies Menziesii* (is this *Pinus Sitchensis*, Bognard Veg. Sitch. in the St Petersburg Academy Transactions?) &c., and among non-coniferæ, *Quercus garryana*? *Arbutus Menziesii*, Pursh; *Spiræa*, sp. n.; *Oreodaphne*, sp. n., a fine shrub; and, what I think justly entitles the expedition to the credit which it originally laid claim to—viz., national importance,—about three pounds weight of a fine pasture grass from the Upper Fraser, which survives all winter, and is accounted by the Cariboo muleteers superior in fattening qualities to hay, and certainly much more valuable to them, as the cattle and the mules have nothing else to subsist on during the long bleak winter, when the ground is covered with snow, and hard as iron. I have gathered, and am still gathering, many particulars of its properties from the muleteers and teamsters, which in due time I will submit to you. I believe that it is superior to the much-lauded "Tussac grass" of the Falkland Islands, and might be introduced with great advantage into some of the bleak islands of the Hebrides, or of Orkney and Shetland, where I have known cattle to die off in the winter from want of proper fodder. My attention was originally drawn to this grass by Colonel Moodie, R.E., to whom, therefore, all credit is due; and I believe that I am at liberty to say that he coincides fully in the above statement. You may remember that it was he who introduced the Tussac grass, and obtained a gold medal for his discovery. The great region for coniferæ I have found, since my present summer's travels have terminated, to lie south of this latitude, and to it therefore, with your permission, I shall in future devote more attention. My funds are in a satisfactory condition, and although the stormy winter, now fairly set in, will prevent me doing much more for some months, I am prepared to start in early spring with renewed vigour to a widely different, and what I now believe to be a better region than the one I have explored. I trust that I shall be able to take the field by the 1st of April with advantage to the Association, and with every prospect of much greater success attending my efforts than during the previous six months. I know of some good species of coniferæ which I could not obtain this year, but I am almost certain of next year—such as *Abies Williamsoni* from "The Three Sisters," in the Cascade Mountains, *Abies bracteata*, *Picea nobilis*, and a new species of *Thuja*.

I have had much to contend against this season, but the difficulties are now, I am glad to say, in a great measure over. I now know the country, and what is just as important, the people, so well, that should there be only half the subscribers of the previous year, I will be able to do tolerably well. Many expenses were incurred last year which will not be required again. These are in addition to the sum paid for travelling to this distant part of the world (Kamtschatka is the furthest sea voyage, and that is only a few days' sail from here). Last night I was awake from my camp-fire sleep by the "long cry" of the same wolf that "howls from Unalaska shore."

On my return to civilisation, I will send my seeds to England by Wells, Farge, and Company's expresses, in several air and water-tight boxes, addressed to you, "care of the Hudson Bay Company," prepaying

the freight. This will be expensive; but the agent here refuses to receive them on any other terms, a standing order having been issued to that effect. I will at the same time send a fuller account of my procedure, and by later mail a statement of my intromissions will be sent to the treasurer. In order that you may be early advised of the despatch of the seeds, and be ready to make the necessary arrangement with the secretary of the Hudson Bay Company, I will send this and the next letter overland by the pony rider, *via* the Salt Lake City, to St Joseph, in Missouri, and thence *via* New York to Liverpool. It will save nearly three weeks, and I hope will escape the emissaries of President Davis.

I am in good health and strong; rough, ragged, weather-beaten, perhaps a little dirty, and certainly a most unrepresentable figure at the meeting where this communication will be read. I am sleeping on a curious Indian blanket, woven from the liber of *Pinus Strobus* (is it the same as the *P. Strobus* of the East?), and to keep the hoar frost off my blanket, there is a mat (clay hulk) of the bark of *Thuja gigantea*. Both will, I hope, at some future day, ornament the museum at the Botanic Garden.

III. Extracts of Letters received from Mr WILLIAM MILNE, Old Calabar. Communicated by Mr JOHN SADLER.

Creek Town, Old Calabar,
June 29, 1863.

I am fairly settled in the district of Old Calabar, exploring the creeks and corners of this majestic river. Africa is certainly rich in botany and other branches of natural history. Years must roll away before the botany of this vast continent is thoroughly investigated, and that will not be until Christianity is upon a more substantial basis. I will give you one extract from my daily journal to show the superstition which still exists amongst the people in Western Africa. While in the district of Ikrofiang, in passing through a large native town in the Ebebo country, I saw a straggling shrub belonging to Bignoniaceæ. While in the act of pulling down some of the flowers, I was surrounded by some hundreds of men, women, and children, shouting and dancing like so many fiends. At first I was inclined to think they were about to hang me in front of their palaver house or heathen temple. On looking round I could see no way of escape, so I held my ground, determined to have some of the flowers; but they were as determined that I should not get them. At last they put me out of the town. On the following Sunday I accompanied the Rev. Zerub Baillie to several of the plantation villages, where he preaches once a week. We met the Ebebo chief. I wished to shake hands with him, but he would not come near. He said he was afraid of the strange medicine I was making, and told Mr Baillie that I was not to come to his town again.

About a fortnight ago a man told me that if I went into the bush I would be shot: so you see it is not all plain sailing at Calabar. But I have an extensive field before me, and I am determined to make the best of it, in spite of the natives, as it will not do to let them have it all their own way. I will mention a few of the leading characteristics of the vegetation which have come under my own observation.

There are five species of *Melastoma*, six species of *Dracæna*, five species of *Amomum*, and several others belonging to *Zingiberaceæ*. There are a number of species belonging to *Scrophulariaceæ*; and amongst them is a *Digitalis*, which is scattered over all waste ground. *Euphorbiaceæ* and *Cucurbitaceæ* are both extensive orders here. Three species of *Amaryllis* are abundant—one in the river, and the other two spread all over the plantations. *Solanaceous* plants are numerous: there

are two kinds sold in the market as purgatives and for bathing the sides of their faces when they have a discharge from the ears. Anonacæ is another extensive order. According to the Rev. Mr Thomson, there are sixteen or eighteen kinds. I have collected a number of Bignoniacæ and Cinchonacæ. I have also met with eight or nine species of Convolvulus, but there are more than that. Amongst the Labiatæ is a large species of *Salvia*, which is used as a medicine. There are three true mints which are used for seasoning; in fact, all this order is made use of as articles of food. A species of *Nymphæa* is frequent in the inland streams.

I think there are from eighteen to twenty-four distinct Orchids: one fine terrestrial species has a flowering stem 6ft. or 7ft. high. There are two fine species of *Strophanthus*. One true *Verbena* and two *Clerodendrons* are abundant. I have also observed two species of *Amaranthus*. "Love-lies-bleeding" is one of them, but I am doubtful if this is indigenous, although the natives say so. Both kinds are used as vegetables in Calabar chop. One *Pentstemon* is found by the margin of a small stream at Ikoroſiong, but not plentiful. A *Phytolacca* and a *Polygonum* also occur at the same place. I have collected specimens of a *Loranthus* from trees by the banks of the river. Two species of *Lonicera* are very common. Leguminous plants are very numerous. Amongst them is a sensitive *Mimosa*. The poison beans (*Physostigma*) are often used for deadly purposes. One species is largely cultivated for putting into the streams to poison the fish, and another is sold in the markets for Calabar chop: one kind is very like our scarlet runner. The ripe pods are from 6 to 8 inches long, and the fruit is beautifully spotted. Compositæ are not so numerous as might be expected: however, there is a due proportion. A *Tillandsia* climbs up the palm trees. There is one fine species of *Calophyllum*, and a tree belonging to *Myrtacæ*; also an *Aristolochia*, which I think is gigantea. There are five different palms. One large species of *Juncus* is abundant on the sides of the river at Creek Town; also another smaller species. There are several *Cyperacæ* by the river, and amongst the lowland plantations there are a number of *Graminæ* sprinkled about. Eighteen varieties of yams and six varieties of *Colocasia* are cultivated; the flowering stems of the latter, with spathe and spadix, are sold in the markets for putting into Calabar chop. The corms are also boiled, and used by the natives. There are two kinds of *Cassava* largely cultivated. There is only one true *Banana* with very small fruit, and eight kinds of plantains, sold in the markets. There is a malvaceous plant cultivated; the fruit is cut up into slices and put into soups. There are also two species of *Agaricus* sold in the markets. They are said to be very nourishing, and to give a fine flavour to Calabar chop. The larger kind is also put into rum, as it is of an intoxicating nature. Calabar chop is composed of the following ingredients:—Palm oil, yams, mints of all kinds; flowering stems, leaves, and corms of the *colocasia*; two species of *agaricus*, the fruit of two leguminous plants, the leaves of two kinds of *amaranthus*. The rest consists of monkey's flesh, dogs, rats, fish, goats, fowls, parrots, and birds of all kinds; in fact, everything eatable, whether animal or vegetable, is put into this wonderful dish. It is a favourite dish among the natives, and relished by Europeans, only they take nothing but palm oil, fowls, and goats' flesh. I have only seen three species of snakes. There is only one parrot, five species of monkeys, three rats, and three mice, four or five land crabs, and a number of fresh-water fish, some sixty or eighty butterflies, and a host of beetles and other insects. Amongst my collection I find twenty-four different ants. Such is a very brief outline of the botany and natural history around Old Calabar.

Creek Town, July 1, 1863.

I am hard at work exploring one of the finest rivers on the west coast of Africa. As you ascend this noble river, the banks and surrounding country are one vast amphitheatre of everlasting green. I am delighted with the appearance of the country and its vegetable products. I have got an extensive field before me, and undoubtedly, if all goes well, I will find many a novelty. This great continent is teeming with animal and vegetable life. Even in my room I have got use for an insect net. It is a great pity that the climate is so unhealthy. Fevers are very prevalent. I have had three attacks of fever since I landed upon the African coast. Fortunately I am blessed with a strong constitution, and I get easy over it; but such is not the case with everybody. The bones of many a blooming youth are bleaching beneath the sun at Calabar.

The natives are not such a murderous class, except amongst themselves. For instance, there is not a day passes but they are killing their twin children instantly after birth, and banishing the unfortunate women to what they call twin villages, where they are left to languish out a life of silent sorrow, and are denied all intercourse with the rest of the world. Mrs Goldie, about seven weeks ago, saved two of those little unfortunates. She remained by the poor woman until she was confined, and then at the dead hour of midnight she entered the mission-house with a little boy and a girl rolled in her lap. The mother followed about six o'clock in the morning. They are all under the protection of the Rev. Mr Goldie. About a month ago the King of Creek Town had a sister whose daughter died of consumption. She sent for a number of her slaves, to give them poison bean. Three women died, three more escaped by vomiting, and one girl took refuge in the mission-house, under Mr Goldie, where she is now attending the school. These poor people were to be servants to her daughter in the future world. As soon as the rainy season is over, I am going to the Qua Mountains, and that will be the time for plants of all kinds. My collections will be upon a grand scale by-and-by. Only the other day I found a splendid climbing lily; it is a true turn-cap lily, and will form a grand show upon the rafters of any stove.

IV. *Extract from Letter from Dr MEREDITH, Georgetown, Demerara, dated 21st November 1863, to Professor BALFOUR.*

Dr Meredith says:—I daresay you know the topography of the large streams in this neighbourhood. The Essequibo is the largest, and lies nearly north and south. The Euyuni, nearly east and west, and the Massaruni, lies between the two, and runs nearly north-west. The two last meet about four miles above the penal settlement, and their joint current unites with the Essequibo about five miles below the penal settlement. The penal settlement is on the west or left bank of the river. The country is nearly a dead level, and at irregular intervals creeks open into the rivers on both sides, and are often navigable for boats. Their mouths are usually entirely hid by foliage. But the Indians know the right gaps as perfectly as did the old smugglers know the caves and holes on the Scotch coasts. I have often been in these creeks for miles, going with two or three hands in a small canoe—dodging under branches, hauling over a prostrate log, and charging through leafy shrubs, lying down nearly flat in the canoe all the time. It was very seldom we came to a place we could see the sky above—such is the dense nature of the bush. In many parts there is never more than dim twilight, and in the rainy season when the sky is dark or cloudy it is a perfect midnight even at mid-day. Never is there a breeze experienced. We have only a

rustling sough some 80 or 100 feet over head. I have often been thinking that the usual description given of the luxuriance of the vegetation during the coal epoch might be applied with great effect to the vast forest of Guiana. The most remarkable feature of the bush is perhaps the immense quantity of parasites, but particularly epiphytes, with their long descending cord-like roots of all lengths, up to 90 or 100 feet, and not thicker than an ordinary writing quill. They are usually very tough. In one of our excursions my companion and I set to work to pull at one of these. We could not see from what height it came, nor to what kind of plant it was attached; however, we pulled, and with great success, as it happened, but we brought a tremendous shower of water upon our heads, as well as a host of black ants, which teased us dreadfully for a while by getting under our clothing. We had dislodged one of the air-plants belonging to the pine-apple order, and the ants' nest must have been attached to it. The root was, I think, about thirty feet long. The Indians always use them as cables for mooring their canoes. In April last a friend and I started off to see the *Victoria regia* up the Essequibo river. We were away nearly a week. It was a delightful excursion. The scenery all along the river was really magnificent—thoroughly unlike anything I ever saw at home. The clearness of the atmosphere, the rank luxuriance of the bush all along the river, mirrored in its gently flowing stream, gave to the whole a picture of exquisite beauty. The habitat of the lily in this district is a lake on an island in the river about the 6th parallel of N. lat.; where Schomburgk first saw it was up above Berbice on the Corentyn river. This lake (Essequibo) was surrounded by dense vegetation, which rendered it very difficult to approach. It was covered with the lily in all stages of its existence—the large white flower, the beautiful crimson edges of the young leaf changing into green as it grows. When it has attained its full dimensions the leaf begins to wither and die at its circumference; but as fast as it decays its place is taken up by a new one. Thus the great *struggle* goes on. I managed to get a good view of the place by climbing along the trunk of a tree which partly overhung the lake. A drizzling rain had been falling for an hour or so before we arrived at the lake; but just as I had ascended my perch on the tree it ceased, and a gleam of sunshine fell on the water, with a slight breeze, causing the vast flotilla to roll gracefully to leeward, throwing out various shades of colours. It was certainly the prettiest water scene I ever witnessed. A day or two afterwards I saw the Indians poisoning a small lake with the "Hiari" bush rope. I entered into the sport with right goodwill. The Indians cut the "Hiari" into pieces of about two feet long, then proceed into the water and beat out the juice of it as a blacksmith beats hot iron with a hammer. The fish begin to show signs of uneasiness, very soon get regularly intoxicated, and often jump clean out upon the dry land, unable to bear the poisoned water any longer. When they were in this state of helplessness the Indians, as well as my friend and I, with some half a dozen negroes, speared or caught them with nets by the dozen. They were most extraordinary looking fish. Some of them bite like dogs, and have very sharp teeth. One of the Indians got a severe wound from one of them. The most ferocious fish I have met here is what the Indians call the "piari;" it cuts as clean as a knife. The quantity of fish in these rivers is perfectly surprising, and if I am not mistaken, their natural history has yet to be commenced.

We spent a day at an Indian settlement, where the inhabitants were most scrupulous in painting themselves with red paint all over, but considered themselves amply clothed by wearing a lappet about six inches square. I asked one young man who had painted his body various

colours like the belts on a tiger, and had his head dressed out with Macaw feathers, why he did it? His answer was, "because it looked pretty, and the women liked it." I wonder what an Eastern satrap would say if he was transported into the midst of an Indian village, and saw the difference of clothing from that in his own country, where a woman dare hardly show the tips of her fingers. We spent a few nights in the bush, swinging hammocks between trees with a sail stretched over them. We were often disturbed by the noise of the "howling monkeys." They make a most awful and dismal noise. They can be heard several miles away. I see the London Zoological Gardens obtained a pair of them the other day. I have been very much interested in trying to make out the history of the enormous boulders which are to be met with about the penal settlement and above. There is no rock like them within hundreds of miles, and the ground on which they rest is pure alluvial soil. The ground of the penal settlement is covered by a collection of them, forming what, I suppose, would be called a moraine. How came these huge blocks down here—could it be by means of glaciers? The gold discovered up the Euyuni, which is now beginning to excite deserved attention, is found chiefly in boulders of this description. The parent rock is in some of the mountains in the interior—but where, no one knows. There is ample room for explorers in this part of the world. I have not been up to the gold district, although I once tried it. Our boat proved too heavy for hauling over the falls. Descending these rapids is most exciting work. We came down almost at railway-train speed. We have always to get Indians to steer, two in each boat.

V. *Notice of Mosses found near Blair-Athole, Perthshire.* By Miss M'INROY, of Lude. Communicated by Mr JOHN SADLER.

VI. *Principal Plants of the Sutlej Valley with Hill, Botanical, and English Names; together with approximate Elevations, and Remarks.* By Dr HUGH F. C. CLEGHORN.

VII. *Letter from Henry Stephens, Esq., relative to Dry-rot.*

Thursday, 11th February 1864.—Professor BALFOUR, President, in the Chair.

The following Communications were read :—

I. *On Diplostemonous Flowers, with some remarks upon the position of the Carpels in the Malvaceæ.*—By Dr ALEXANDER DICKSON.
(This paper appears in the present number of the Journal.)

II. *On the Cinchona Plantations in connection with the Botanical Garden at Bath, Jamaica.*—By NATHANIEL WILSON, Curator of the Garden. Communicated by Dr LAUDER LINDSAY.

The most important event in the history of this Botanic Garden for many years past has been the introduction, by seeds, of the quinine-yielding *Cinchona* in the autumn of 1860. By the month of October 1861, I succeeded in rearing upwards of 400 healthy plants, quite ready for planting out; but unfortunately the selection of a proper site for their final establishment was overlooked, and in consequence one-half of the number perished. Being anxious, however, to test the adaptability of the plants for cultivation in the higher altitudes of this island, I caused the whole of them to be removed in small pots to Cold Spring Coffee Plantation, the elevation of which is about 4000 feet. I soon found the climate and soil

of this locality to be all I could desire for the plants; and as it afforded every facility for carrying out so valuable an experiment, I at once availed myself of it, and planted out in the coffee fields on the 16th November several plants of each species, then about two or two and a half inches in height. In twelve months after, a plant of the red bark, *Cinchona succirubra*, had attained to the height of forty-four inches, with leaves measuring thirteen and a half inches in length, by eight and three-quarters in breadth. The same plant, now two years old, measures six feet in height with ten branches, having a circumference of stem at base of four and a half inches. The plants of *Cinchona micrantha*, grey bark, being of more slender habit of growth, have not made such rapid progress; the highest has attained to five feet with three branches. The leaves, however, are larger, and measure fourteen by ten inches. So far the experiment has thus proved eminently successful. It would be difficult to find more healthy trees in the forests of that neighbourhood; and, in about three or four years hence, they may be expected to produce seeds. In the meantime they can be largely increased by cuttings and layers, in the hands of a skilful propagator. During the months of August and September 1862 the collection was again removed to Bath. The plants were at this time eighteen or twenty months old, a critical period for forest trees in flower pots under artificial treatment, and in a climate too so uncongenial for them as that of Bath, which would have terminated their lives had they not soon afterwards (13th October) been planted out at Mount Essex, near Bath, at an altitude of two thousand feet, or little more. This site, as a temporary one, was had recourse to to save the plants alive until a better could be obtained; and so far it has answered the purpose, and a majority of the plants are healthy, but have not made such rapid progress as could have been desired. The soil is too loamy and by far too stiff to admit of a free and rapid escape of the rains which fall here in torrents during the greater part of the year, and the altitude far too low for the Peruvian barks. The red bark thrives at a much lower altitude, and, being a more hardy tree, the plants are more healthy. A very important fact has now been established—viz., that the climate of our higher, and many of our intermediate, mountains is suited for the growth of the most valuable species of quinine-yielding plants—*Cinchona succirubra*. A knowledge also of the method of increasing the plants, and of the soil best adapted for their full development has been obtained.

III. *Notice of the Occurrence of Woodsia alpina (hyperborea) in Gaspé, Canada East.*—By GEORGE LAWSON, LL.D., Professor of Chemistry, Dalhousie College, Halifax, Nova Scotia.

Professor Lawson stated that this rare fern (*Woodsia alpina*) had not been found by any recent collector in Canada, and no one knew where to look for it. He had recently described, however, in connection with *Woodsia glabella*, and under the name of var. *Belli*, a remarkable *Woodsia* found in Gaspé on the Dartmouth River, 20 miles from its mouth, by his former pupil, Mr John Bell. This plant, on further examination, turned out to be the *Woodsia alpina*. He enclosed a specimen.

IV. *Remarks on Myrica cerifera, or Candleberry Myrtle.*—By Professor LAWSON, Dalhousie College, Halifax.

I found *Myrica cerifera* a few days ago in some quantity on a hillside near Halifax, with small birds feeding on the berries. It is variously called Wax Myrtle, Candleberry Tree, and Tallow Shrub, in reference to

the wax with which its berries are coated. The wax is yellowish green, emitting a fragrant balsamic odour not so unctuous as bees' wax, nor so brittle as resin, sinking in water, whereas bees' wax swims, sp. gr. 1.015; fusing point, 110°. The wax is obtained by boiling the berries in water. The plant is common all over the hills overlooking the innumerable bays and harbours of the Nova Scotian coast, but I do not know that the product has been made an object of commerce here. Some years ago Professor Simmonds called attention to the desirability of encouraging the production of Myrtle wax, and gave in the "Pharmaceutical Journal, vol. xiii. p. 418," very full details respecting the wax-yielding Myrtles of South Africa, and the mode of manufacturing the product. The species which is cultivated at the Cape as the best South African one is *Myrica cordifolia*. Professor Simmonds speaks of our American Myrtle wax (*M. cerifera*) as identical commercially with that produced at the Cape. A consignment of Cape wax (2561 lbs.) yielded a clear profit of L. 54, 4s. 5d., after payment of all expenses, collecting, shipping, &c. Should any manufacturer in Britain feel an interest in the matter, I will be happy to furnish such additional information as may be procurable here. In "Annales des Sciences Naturelles 1855," it is stated that the European *Myrica Gale* yields a little wax. The *Myrica cerifera*, like its African congener, is a coast plant. I never saw it in Upper Canada; but Professor Gray gives a station on Lake Erie. On the Halifax hills it is a small spreading bush, three or four feet high, forming a close brush. At this season the stems are leafless, but the withered leaves scattered about still retain their fragrance.

V. *Note on the leaves of Ulex (Whin).* By Professor LAWSON, Dalhousie College, Halifax.

The seedling *Ulex* has at first no spines. The young stem is clothed with leaves—from twelve to twenty in number—these are shortly petiolate and trifoliate, consisting each of three small elliptical hairy articulated leaflets. When the stem becomes five or six inches in length (usually) the trifoliate leaves cease to be developed, and spines are then produced. We thus see that in *Ulex* the perfect leaves appear during the early period of the plant's development, while in the Australian *Leguminosæ*, their production is delayed till the maturity of the plant. *Ulex*, however, is truly a plant with compound trifoliate leaves, not simple-leaved, as stated in many works. The above facts were ascertained from observing the development of some seedlings of the whin raised by Mrs Lawson in a flower-pot, from seeds brought from Ireland by Dr Dickson, Dean of the Medical Faculty of Kingston.

VI. *Notice of Mosses found in the neighbourhood of The Burn, near Brechin, Forfarshire.* By Mrs M'INROY. Communicated by Mr JOHN SADLER.

Mrs M'Inroy gave a list of mosses to be met with in the neighbourhood of The Burn, so far as at present known. The greater proportion are to be found within the policies, and principally on the banks of the North Esk, which flows through the grounds. The list contains 144 species, but Mrs M'Inroy states that she believes this by no means exhausts the mosses of the place.

The following note to Professor Balfour from Dr John Kirk was read:—"I enclose two pods of a tree nearly allied to *Bauhinia* or *Copaifera*, but of a new genus. Perhaps you may succeed in growing it. As yet

the flowers are unknown, for my specimens were lost. This tree has hard black wood, called by the Portuguese *Pao ferro*; by the Bechuanas, *Mo-pane*. I have got the foliage and fruit, but, being a new genus, the flower is a sad want. It inhabits the driest and most baked clay plains; no heat kills it, the leaves, like those of Australian forests, turning their edge upwards when the sun shines very powerfully. The testa of the seed is full of resin in large cysts, and the cotyledons are convoluted like the surface of the human brain."

A note from Mr W. J. HAIG, Dollarfield, was read, in which he says:—"I enclose a specimen of a plant which I have just received from a cousin who is settled in the Banda Oriental Republic of Uruguay. He writes to me:—'I should be glad if you could get a scientific opinion on the following case:—It had long been a known fact here that sheep removed from the province of Buenos Ayres to the Banda Oriental, or imported from Europe, are liable to die in great numbers during the first fortnight after arriving. I saw an instance myself last week. At an *estancia* about five leagues from here, the owner had just received from Buenos Ayres sixty rams and ewes freshly imported from Europe, which cost L.15 per head: of these, fourteen died in five days. It is believed that the mortality arises from the sheep eating a plant called *min-min*, peculiar to this province. I enclose a twig of it. It grows in patches about the size of one's hand, and the twig enclosed is of average height. The stomach and intestines after death are found much inflamed; and in some cases the under side of the skin is quite red with effused blood. I may add that sheep born here, or acclimatised, are in no way injured by the plant—in fact, they almost entirely avoid it.'" This seems very analogous to the effect which the twigs of the yew tree have on animals here; and I should think that the only way to avoid the danger would be to put the animals for the first week or two in an enclosure cleared of this plant. By this time desire for green food would be somewhat abated, and they would, like the native born, become more discriminating.

Thursday, 10th March 1864.—Professor BALFOUR, President,
in the Chair.

The following Communications were read:—

- I. *New Researches on Hybridity in Plants*. By M. CH. NAUDIN. Part II. Translated from the French, and communicated by Mr GEORGE M. LOWE.

(This paper appears in the present number of the Journal.)

- II. *On the Chemical and Natural History of Lupuline*. By M. J. PERSONNE. Translated by GEORGE LAWSON, LL.D., Professor of Chemistry and Natural History in Dalhousie College, Nova Scotia.

Drawings of Lupuline were shown, and specimen from the Industrial Museum was exhibited by Professor Archer.

(This paper appears in the present number of the Journal.)

- III. *Remarks on the Sexuality of the Higher Cryptogams, with a Notice of a Hybrid specimen of the species of Selaginella*. By Mr JOHN SCOTT.

Specimens of the species of *Selaginella* and the Hybrid in a living state were exhibited.

(This paper appears in the present number of the Journal.)

IV. *Abstract of a paper on the Constitution of Gymnospermous Flowers.* By A. W. RICHLER. Communicated by Professor BALFOUR.

V. *List of Fossil Plants found in the Tertiary Strata of the South-East of France.* By GASTON DE SAPORTA. Communicated by Professor BALFOUR.

(From "Annales des Sciences Naturelles," 4th ser. Bot. tom. xix.)

PUBLICATIONS RECEIVED.

1. Canadian Naturalist and Geologist, October and December 1863.—*From the Editor.*

2. Bulletin de l'Académie Royale des Sciences, des Lettres, et des Beaux Arts de Belgique, Nos. 11 and 12, 1863.—*From the Editor.*

3. Remarks on the Present Condition of Geological Science. By Geo. E. Roberts.—*From the Author.*

4. The Story of the Guns. By Sir James Emerson Tennent.—*From the Author.*

5. Natural History Review for January 1864.—*From the Publishers.*

6. Functional Diseases of Women By John Chapman, M.D.—*From the Publisher.*

7. The Classification of Animals based on the Principle of Cephalization. By James D. Dana.—*From the Author.*

8. On Fossil Insects from the Carboniferous Formation in Illinois. By James D. Dana.—*From the Author.*

9. Programme de la Société Batave de Philosophie Experimentale de Rotterdam.—*From the Society.*

10. Journal of the Asiatic Society of Bengal, Nos. III. and IV, 1863.—*From the Society.*

11. Handbook of Astronomy. By George F. Chambers, F.R.G.S.—*From the Author.*

12. American Journal of Science and Arts, May and November 1863, and January 1864.—*From the Editors.*

13. Proceedings of the Literary and Philosophical Society of Liverpool, No. XVII.—*From the Society.*

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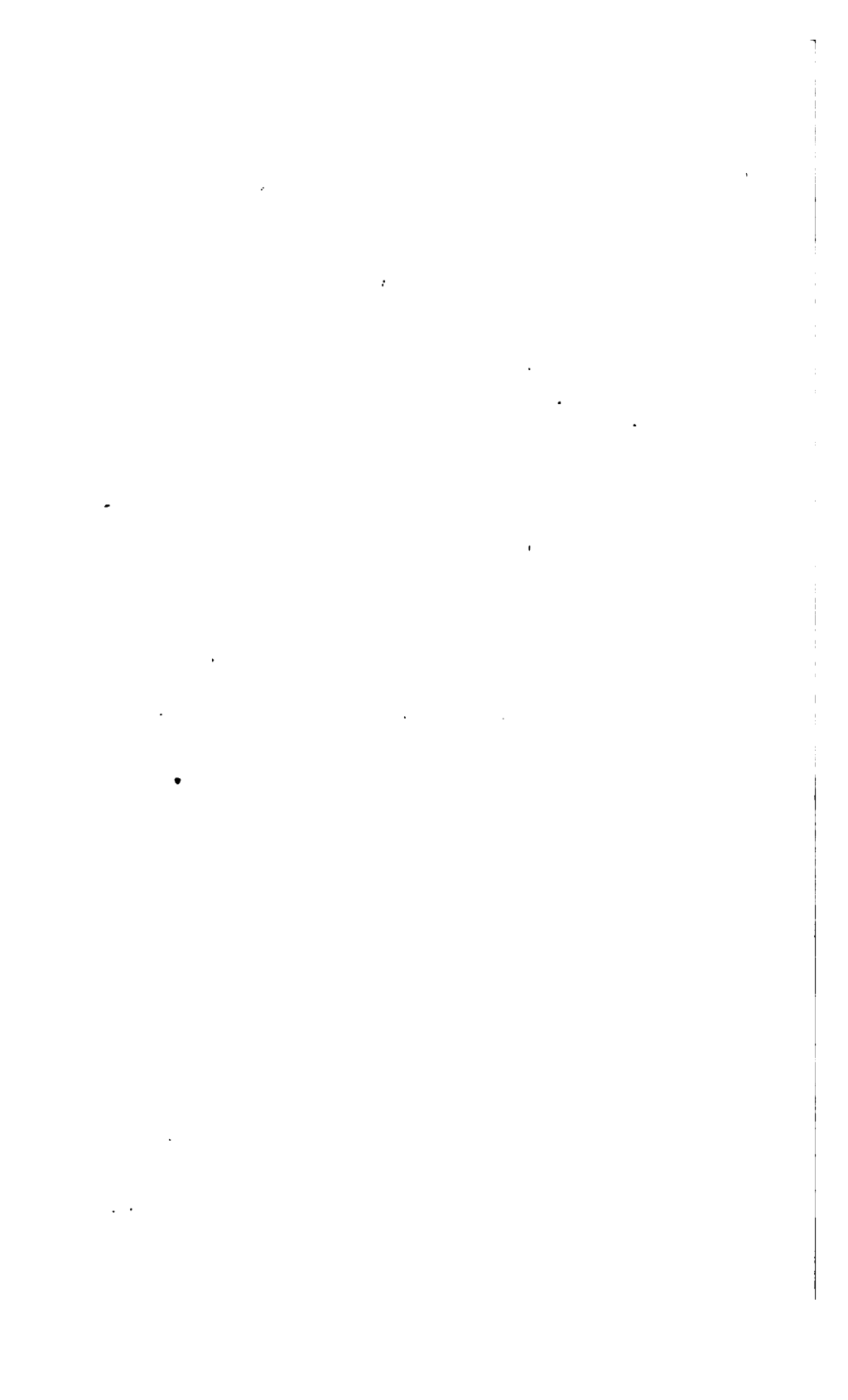
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